

No Hesitations 2013

*A Blog Book*



Francis X. Diebold



No Hesitations



# **No Hesitations**

A Blog Book

**Francis X. Diebold**

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To those readers  
who suggested that  
the whole would be greater  
than the sum of the parts,  
which was meant as a compliment,  
I think.





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# About the Author and the Blog



Day job: Professor of Economics, Finance and Statistics at the University of Pennsylvania. His intellectual family includes more than 50 Ph.D. students, and many grandstudents. Nights and weekends: Husband / father of a family of five, whose youngest member (15) is horrified that Dad is writing a blog. (Someone might see it...)

No Hesitations contains news and views, comments and criticisms, rants and raves, focusing on dynamic predictive modeling in economics and finance, but it also provides broader commentary.



## About the Cover



The 1863 drawing is *Cavalry Soldier with Sword on Horseback*, by Winslow Homer (1836-1910), done in black chalk on tan paper. It's in the Cooper-Hewitt National Design Museum, New York. It's in the public domain, and the image (also in the public domain) is available at Wikimedia Commons ([commons.wikimedia.org](https://commons.wikimedia.org)).





# Preface

Many parts of this book appeared earlier as installments of my *No Hestitations* blog, [www.FXDiebold.blogspot.com](http://www.FXDiebold.blogspot.com). Simultaneously, the book differs slightly from the original blog posts, some of which have been polished, annotated, re-ordered, or deleted.

Many people contributed implicitly to the book's development. Eric Ghysels and Hayley Kelley launched me into the blogosphere. Frank DiTraglia and John Ro pushed me forward. Enthusiastic readers kept me energized. I am exceptionally grateful to all.

Finally, and most importantly, I am grateful to my wonderful blog readers. This collection is my New Year's gift to you.

Francis X. Diebold  
Philadelphia

Thursday 2<sup>nd</sup> January, 2014



No Hesitations



# Chapter 1

## Questions

Two questions loom large in my mind: (1) Am I really writing a blog?, and (2) If so, how did I get here? The answers are (1) Yes, and (2) I'm not exactly sure, but I'm the same as I ever was.

Why *No Hesitations*? At first I liked *No Reservations*, but then I realized that a certain ex-chef turned television star might take issue (read: sue me), so I settled on *No Hesitations*, which is actually better for several reasons. First, it conveys the same flavor of honest, no-holds-barred, observation. Second, it discards the double entendre of “no reservations,” which is sensible since I don't plan to write about restaurant meals (although you never know). Third, again, I won't get sued. So it's good all around.

As it says in the “About The Blog” blurb, this blog will contain news and views, comment and criticism, rants and raves. Perhaps the best way to sketch a bit of what I plan to do is to start by sketching a bit of what I won't do. First, I promise not to torture you with boring policy drivel. Put differently, if I ever post an installment with a title of, say, “On Regulatory Framework Considerations for Theoretical and Empirical Macro-Prudential Analysis of Systemic Risk with Implications for Basel XIV,” please shoot me. In addition, I won't provide too much commentary on current events, as doing so would require me to know about current events. Seriously, though, friends like Jim Hamilton and Menzie Chin at [Econbrowser](#), or John Cochrane at [The Grumpy Economist](#) would run circles around me – their continuous insights and energy

never cease to amaze me.

OK, what then *will* I do, if I don't torture you with current events and policy drivel? Basically I'll torture you with Diebold drivel. In particular, if you're interested in financial markets and associated macroeconomic fundamentals, in their relation to data, statistics and predictive modeling, you'll like the blog. Did I mention data, statistics and predictive modeling?

Let me expand on my assertion of "honest, no-holds-barred, observations." You'll find *No Hesitations* quirky and different, turning stones often unnoticed, let alone turned. For example, in addition to Diebold drivel, you'll find ruminations on things ranging from academic life to Led Zeppelin. And you'll find it mildly irreverent, if you haven't already. By the way, did I mention data, statistics and predictive modeling?

So much for wanton aggrandizing. But while I'm at it, let me take it to the limit, concluding as I began, with a question: Might *No Hesitations* emerge as the most interesting blog in the world? Stay reading my friends.

## Chapter 2

# Simon Kuznets: Penn Professor, Nobel Laureate, and Master of Measurement

A few days ago I received in the snail mail a copy of Robert Fogel's new book, [Political Arithmetic: Simon Kuznets and the Empirical Tradition in Economics](#). Maybe it was in my box for a while; I must admit to checking it only infrequently. I'm also not sure who sent it; maybe it was Fogel, or maybe it was the NBER or the University of Chicago Press, as the book is in the legendary [NBER Series on Long-Term Factors in Economic Development](#), published by the U. of C. Press.

Anyway, Fogel's book is fantastic.

First, it's just a little longer than one hundred pages. I appreciate that.

Second, I personally like it because it's related to my alma mater and employer, the [University of Pennsylvania](#), where Kuznets taught for decades, and about measurement in economics, which I view as central. Many people associate Kuznets with Harvard, which is also correct, but Harvard appointments tend to be lagging indicators – effectively rewards for earlier path-breaking work done elsewhere – and in Kuznets' case the earlier path-breaking work was done at the University of Pennsylvania. Moreover, many of Penn's most-lauded subsequent contributions have also been in the Kuznets' empirical /

measurement tradition, from the early macro-econometric models of Nobel laureate Larry Klein, to the ongoing [Penn World Tables](#) of Irv Kravis, Bob Summers and Al Heston, to a wide variety of more recent work by current faculty. (Hey, if Greg Mankiw can gush about Harvard in [his blog](#), then I can gush about Penn in mine.)

Third, it's a beautifully-written and entertaining history of many fascinating and entangled aspects of economic measurement, from the rise of academic economics in the early twentieth century, to the development of the NBER's stunningly-successful tradition of empirical economics, to the interplay between measurement and theory. I found it especially valuable and informative in that, although the NBER's tradition in empirical business cycle analysis (Burns, Mitchell, Zarnowitz, Stock, Watson, etc.) is well-known, the parallel NBER tradition in empirical growth analysis is less so (certainly by me, but I suspect much more widely).

Fourth, it's eye opening. We tend to take things like GDP data for granted, as with the national income and product accounts more generally. But before Kuznets' path-breaking work, there was nothing. Imagine that. *Seriously, try to imagine that!* Imagine trying to price an asset like foreign exchange with no idea of domestic or foreign GDP growth or inflation, or more generally, imagine trying to make any kind of economic or financial decision – from the national level to the household level – largely in a data vacuum. That's how it was, less than a hundred years ago. Thanks, Simon, for shining your light.

Finally, the book, like Kuznets' work itself, testifies convincingly to the power and centrality of measurement in science. Indeed *measurement is the essence of science*. Yes, yes, of course, my offended theorist friends, theory is important too, but theory is little more than data reduction, and the theory mill needs grist before it can grind. And yes, I'll have more to say about measurement vs. theory in future installments.

But for now, there's only one important matter at hand – thanking Robert Fogel for writing his beautiful and unique book. Here here!



## Chapter 3

# No Hesitations Birthday

Wow, I can't believe that *No Hesitations* now has 1000 page views, and it's only a few days old, and with only one real post. Of course I understand that by the standards of the blogosphere 1000 is basically 0, but I am nevertheless humbled. And I swear that no more than 985 of the page views are mine. Only a billion more until I match Cochrane! Thanks my friends.



# Chapter 4

## My Favorite Blogs

I have noticed in recent days that when people first read my blog, they often immediately ask me to recommend another blog. This is somewhat unsettling. My preferred interpretation is, “Your blog is so wonderful – indeed it has so quickly transformed and enriched my life – that I’d now like to explore other blogs as well.” There is also a less-favorable interpretation.

Either way, I’m happy to oblige. I read many blogs in principle, but few in practice. Historically it’s just laziness. Presently, now that I’m writing *No Hesitations*, I can claim that it’s intentional, like a composer not wanting to listen to the radio. But actually it’s just laziness. Anyway, here are a few favorites.

My discovery of the moment is Greg Laughlin’s blog, [Systemic](#). No, it’s not about systemic risk in financial markets; rather, it’s about characterizing planetary systems. Greg is a fascinating astrophysicist, chair of the [Department of Astronomy and Astrophysics at UC Santa Cruz](#), and a master of astrophysical simulation. But that’s just his day job. At night he’s an equally fascinating econo-physicist, studying trading at the speed of light (literally; see for example his recent [“Information Transmission...”](#) paper), and developing rich insights into the nature of price discovery in financial markets. I’d love to lock Greg in a room with [Joel Hasbrouck](#) for a day; who knows what amazing insights might emerge. (In case you’re wondering: A month ago I had never heard of Greg. I met him at UCSC when I gave a lecture there a few weeks

ago. Thanks very much to [Eric Aldrich](#) for making the introduction.)

I like [Greg Mankiw's Blog](#), by none other than [Greg Mankiw](#). Obviously I don't like it for its clever title. Rather, I like it because it's warm and friendly, like a fireside chat, accessible to all, including students – yet packed with interesting observations. It's just *fun*, and like Greg himself, basically impossible to dislike.

I like [Econbrowser](#), by [Jim Hamilton](#) and [Menzie Chinn](#). Nothing escapes their radar, or their analysis. And here's an item from the Strange but True Department: At least when I last asked, which was many years after they started the blog, Jim and Menzie had still never met in person. Only in the digital age!

I like [The Grumpy Economist](#), by [John Cochrane](#). It's full of useful news and insightful analysis, and Cochrane's views are often close to mine, so what's not to like? I also like his prose. When I read Cochrane, it's as if he's looking across a table straight at me, talking passionately, with the "Cochrane twinkle" in his eye. If you do nothing else today, you must read (or re-read) two things. First, read his ["Comments on the Milton Friedman Institute Protest Letter"](#). It's cliché to say that you just can't make this stuff up, but really, you just can't make this stuff up. Second, read his ["How Did Paul Krugman Get it so Wrong?"](#) (a response to Krugman's *New York Times Magazine* article, "How Did Economists Get It So Wrong?").

Speaking of John Cochrane and Paul Krugman, Cochrane may be interested (shocked?) to know that *Krugman is my all-time favorite*. In the next post, I'll explain why.

## Chapter 5

# Why I Like Paul Krugman

I've never met Paul Krugman, but I've always read his *New York Times* column. I admire him and his column for several reasons.

First, I admire his research. Krugman was a great researcher. When he focused on economics research, he did great economics research.

Second, I admire his versatility. Krugman successfully transformed himself from a great economics researcher into an equally great political commentator. That's really hard.

Third, I admire his sparkling prose, which suits his new political mission perfectly. He writes very, very well. The left has never had a better voice.

Fourth, I admire his perseverance in meeting strict deadlines, week after week. I fear that I couldn't do that. Indeed I fear that just producing another five-paragraph installment of this blog might take, say, a year.

But the most important reason why I like Krugman, by far, is that his column has tremendous practical value. To paraphrase Milton Friedman's long-ago remark about John Kenneth Galbraith, I like Krugman because to take the correct stand on any issue I need only learn Krugman's, and take the opposite.



# Chapter 6

## Research Computing / Data / Writing Environments

You asked for favorite blogs, and I obliged. You also asked for favorite computing environments etc., so here goes. I look forward to comments telling me why I'm wrong, stupid, insane, or worse.

At some level, who cares about computing etc.? If you're deep into retirement, a SAS guru writing in WordPerfect (say), is it worth updating? Almost surely not.

But if your investment horizon is longer, and if you want to be on the cutting edge, and if you want my opinion, I certainly have one. What follows is in part prescriptive, although I realize that one size surely can't fit all. In any event it's certainly descriptive; it's basically what I do, in principle if not always in practice. (I admit that I'm still rather fond of certain ancient low-level environments like Fortran, and certain high-level environments like Eviews.)

My computing epiphany of recent years centers on [R](#), a mid-level environment. I find that R is most often the place to be. Check [R Studio IDE](#), a wonderful R work environment. Check [R-bloggers](#) (I should have mentioned it as a favorite blog – thanks to [Frank DiTraglia](#) for reminding me). And for all you parallelization freaks with GPU's, check the [CRAN Task View on High-Performance and Parallel Computing with R](#) and the [R Tutorial](#).

For time-series data, check [Quandl](#). Totally amazing. Just click on the link and see for yourself. (And yes, there's a seamless [R interface](#).) Imagine having basically any time-series you could ever want, instantly available and continuously updated, for use in your R code.

For writing, obviously it's LaTeX. My favorite flavor is [MiKTeX](#). Enough said.

Now here's the first kicker. I already mentioned that Quandl and R are interfaced. But so too are R and LaTeX, via [Sweave](#). So now data, computing and writing are all linked. Imagine writing a book (in LaTeX) whose graphics and statistical analyses (in R) are automatically updated in real time as new data arrive (in Quandl). It's not a dream.

And here's the second kicker. Everything I've emphasized is public domain, open source, free. Who says that you get only what you pay for? This is highest quality everything, cutting edge, with no license hassles, no renewal hassles, no payment hassles.

Power to the people!



## Chapter 7

# Blogging Environments

[I love this one, and I am immensely pleased that so many others do too.]

Speaking of ideal environments, how could I have forgotten blogging environments in my last post?

Not that I really know much. And not that you care. Anyway, a typical recent Diebold family exchange says it all:

Daughter 1: Dad, why are you doing a stupid blog?

Me: I'm not sure. And it's not stupid. And thanks for your support.

Daughter 1: And why are you doing your stupid blog using stupid Google Blogger?

Me: I'm not sure. And thanks for your support.

Daughter 1: No cool bloggers use Blogger. Only old people who don't know any better. You should use WordPress.

Me: What are you talking about? What's WordPress? All my blogging friends use Blogger.

Daughter 1: They're all old people who don't know any better.

Daughter 2: Don't listen to her, Dad – Blogger is perfectly fine and totally cool.

Wife: What's a blog?

In my experience regarding e-matters, the youngest is generally right. So Daughter 2 (age 14) wins – for now I'm staying with Blogger. I'm delighted that Blogger is cool, and that by implication I'm cool.



# Chapter 8

## SoFiE 2013 Singapore

Greetings my friends, en route to SoFiE's annual meeting, in Singapore this time. If you're new to the [Society for Financial Econometrics \(SoFiE\)](#) (and even if you're not), please take a look and click on the links.

SoFiE is a global network of academics and practitioners dedicated to sharing research and ideas in the fast-growing field of financial econometrics. It's an independent non-profit membership organization (check out the [governing Council](#)), committed to promoting and expanding research and education by organizing and sponsoring conferences, programs and activities at the intersection of finance and econometrics, including links to macroeconomic fundamentals. Its official journal is the *Journal of Financial Econometrics*, published by Oxford University Press.

The annual SoFiE meetings rotate among the Americas, Europe and Asia and feature all aspects of financial econometrics. This our sixth annual meeting, so we've now been around the world twice: New York, Geneva, Melbourne, Chicago, Oxford and now Singapore. Check out the Singapore [program](#) and [plenary speakers](#). We are most grateful for the impressive efforts of this year's local organizers, [Jin-Chuan Duan](#) and his fine team at [National University of Singapore](#), and [Jun Yu](#) and his fine team at [Singapore Management University](#). Jin-Chuan Duan leads the [Risk Management Institute](#) at NUS, and Jun Yu leads the [Sim Kee Boon Institute for Financial Economics](#) at SMU.

Check out the SoFiE [Facebook group](#), and visit its [web page](#). You'll see

other SoFiE activities as well. They include regional thematic SoFiE meetings (the last was at FGV Rio de Janeiro in December 2012, and the next is in [Lugano in October 2013](#) on large-scale factor models, hosted by the University of Lugano – more precisely [The Faculty of Economics of the Universit della Svizzera Italiana](#) – with additional sponsorship from the [Swiss Finance Institute](#)). SoFiE activities also include the annual OMI-SoFiE Financial Econometrics Summer School, thanks to Neil Shephard’s entrepreneurship (the last was July 2012 in Oxford at the [Oxford-Man Institute](#) (OMI), as is the next, [July 2013 on Financial Forecasting](#) featuring [Andrew Patton](#) and [Allan Timmermann](#)).

On the [web page](#) you’ll also find information about membership. The Society benefits from the invaluable support of both a core group of [institutional members](#) and hundreds of individual members. Please consider joining the Society. We’d love to have you!

## Chapter 9

# Structural Models, Reduced-Form Models, and Financial Econometrics

The scene at [SoFiE 2013](#) leads me to reflect on structural vs. reduced-form modeling approaches in econometrics. Much financial-econometric work is reduced-form, whereas structural modeling has recently become fashionable in certain other areas of econometrics. The structure police, especially new recruits, are often fanatical. But the reality is that reduced-form “statistical” models are every bit as scientific as structural models. Structural models are simply restricted reduced-form models, and it’s a delicate and situation-specific matter as to whether imposing structural restrictions on reduced forms is necessary or desirable.

Many central activities in finance involve descriptive and predictive tasks, which are often most effectively executed in reduced-form mode. That is, we don’t necessarily need deep structural understanding to succeed, for example, at prediction, which is wonderful, because we often don’t have deep structural understanding. (Admit it.)

One key example, on my mind because it features prominently at SoFiE, is financial market volatility modeling. GARCH, stochastic volatility, realized volatility, whatever – all such approaches are reduced-form, essentially

autoregressive. Yet financial econometric volatility modeling has been hugely successful in both academic and industrial finance. It is now used routinely and productively in risk management, portfolio management, spot and derivative asset pricing, and more.

And financial econometric volatility modeling is just one example. All told, for descriptive and predictive tasks in a variety of sub-areas of econometrics, reduced-form modeling often provides the best of all worlds, delivering major advances while avoiding structural modeling pitfalls.

## Chapter 10

# Statistical Graphics: The Good, The Bad, and the Ugly

I love good graphics, so I love [Edward Tufte's work](#), and I'm always amazed by the number of allegedly quant-aware people who are actually unaware of Tufte. His beautifully-produced first book, *The Visual Display of Quantitative Information*, is surely an all-time masterpiece on elements of graphical style, not to mention a tremendously engaging and entertaining read. He opened my eyes, massively, to everything from avoiding chartjunk (a marvelous Tufte term), to thinking hard about aspect ratios, to thinking similarly hard about whether/why/how to use color. Indeed I admire Tufte so much that I occasionally find myself jealous. Why can't *I* be Tufte? Why can't *I* be the graphics legend with the stunning [ET Modern](#) studio in Manhattan? Why didn't Apple ask *me* to [design the iPhone GUI](#)? Damn that miserable Tufte.

Tufte always says that Minard's *Napoleon's March* graphic, above, is the greatest ever. ([Click here for detail.](#)) Everyone else says that too, but they're just repeating Tufte. Notwithstanding the futility of "greatest ever" proclamations (except for rock guitarists – it's clearly Jimmy Page, but that's another post...), Tufte might be right. *Napoleon's March* informs instantly, yet it simultaneously repays hours of careful scrutiny. It shows the French army advancing on Moscow and retreating due to the brutal winter, with path widths tracking the number of soldiers alive. It presents a huge amount of information

compactly, telling a rich and textured story moving through space and time, beginning with bravado and devolving into disaster.

Now consider the graphic, by Larry Leemis et al. (*American Statistician*, 2008). I learned of it recently from Oscar Jorda and Glenn Rudebusch. At first I thought it was a joke, a great example of in-your-face bad graphics, perhaps entertaining (unintentionally) but failing to communicate seriously.

Now, a week later, I feel, well, the same. But I've also come to view the *American Statistician* version of Leemis et al. as something of a static straw man, chained as it is to the printed page. It turns out that the Leemis et al. web page has a much better dynamic version. Moving the mouse over the graphic, each distribution is highlighted, together with its immediate relatives. And moving the mouse over any of the distributions listed on the left of the web page locates it and its relatives in the figure, and clicking provides more detailed information. All told, the dynamic version of Leemis et al. is engaging and useful.

Consideration of Minard's *Napoleon* vs. Leemis et al.'s *Distributions* raises important and unresolved issues. Tufte's main mission is to describe how best to make "traditional" graphics, frozen on the static printed page, as with Minard's *Napoleon*, and his descriptions are of course also frozen on the same static printed page. But in recent decades the computer has catapulted us to dynamic and multi-layered graphics, with highlighting, brushing, spinning, clicking, etc., as with Leemis et al.'s dynamic *Distributions*. What are the key new principles of dynamic graphics, and how can one possibly describe them well in print? Tufte shrewdly skirts those issues in large part, leaving it to others to write a new "Tufte for the 21st Century." Pioneers like William S. Cleveland made early progress, and of course the modern dynamic graphics research program continues unabated. But it's still hard – and it will always be hard – to describe and discuss dynamic graphics insightfully on paper.

Will there ever be a Tufte for the 21st Century? Is it even *possible*? What would be its format? (Surely not paper.) And what, precisely, would it contain? The good news, I suppose, is that we have 87 years to continue working on it.



# Chapter 11

## Financial Regulation: Too Big to Fail

In my first post (“Questions”), I promised not to torture you with boring policy drivel. What follows is about financial regulatory policy, but I insist that it’s neither boring nor drivel. Rather it’s about a key and deep issue.

Let’s get right to it. The massive elephant in the room is, and always has been, the free insurance, or the free put option, or the bailout entitlement – call it whatever you want – associated with financial institutions with the coveted status of “too big to fail” (TBTF). Rest assured, there’s no better way to incent financial institutions to take massively undesirable risks, with disastrous macroeconomic consequences, than to give them “get of of jail free” cards.

Effete cognoscenti will yawn and note that this is hardly a new observation. They’re right. Countless observers have been shouting for decades about the adverse incentives generated by TBTF. [Gary Stern](#), former President of the Federal Reserve Bank of Minneapolis, and [David Skeel](#), Professor of Law at the University of Pennsylvania, are two good examples. Take a look at their books [here](#) (Stern) and [here](#) (Skeel).

The problem is that the shouts have fallen on largely deaf legislative ears. Indeed *Dodd-Frank effectively institutionalizes TBTF* through a nasty cocktail of government “partnership” with financial institutions, combined with dis-

cretionary government distress-resolution procedures. A nightmare scenario if ever there was.

The same cognoscenti will now snap to attention and assert that Dodd-Frank *recognizes* TBTF's adverse incentives and thwarts them by increasing capital requirements and intensifying regulatory scrutiny. TBTF is no longer relevant, the story goes, because now we're regulating the large institutions so effectively that they'll never again be in danger of failing.

Do you really believe that? That is, is there any real reason to believe that this time the rain dance will work, that this time we've fixed the TBTF incentive problem, that *this time is different*?

To be continued...

## Chapter 12

# Financial Regulation, Part 2: Rules

I concluded the last post with the question, “Do you really believe that ... this time we’ve fixed the too-big-to-fail (TBTF) incentive problem, that *this time is different?*” Presumably your answer depends on your feelings regarding the efficacy of Dodd-Frank’s (DF’s) increased capital requirements and intensified scrutiny of financial institutions.

Needless to say, I have my doubts.

Left to their own devices, lawyerly types (and politicians and regulators come disproportionately from that realm) tend to aspire to write exhaustive sets of rules that dictate what can and can’t be done, when, and by whom. Economists call that a “complete contract.” DF, at 2000+ pages, is an example of an attempt at such a complete contract, which financial institutions were forced to “sign.”

One can entertain the idea of a complete contract in principle, but the idea of making rules to govern all possible contingencies is preposterous in practice. (Note that many important possible contingencies are surely not even remotely *conceivable* now – more on that in the next post.) No one is so naive as to believe that DF is truly a complete contract, but the spirit of the attempt is one of complete contracting. Let’s call it “rules-based” regulation.

Of course all legislation, regulatory or otherwise, *must* be rules-based.

Rules, after all, are the essence of law. And rules, even massive sets of rules, can be very good things. There is little doubt, for example, that rules enforcing contractual and property rights can play a large role in generating economic prosperity. And there are some good entries in the DF rulebook.

But there are three key related problems with naive implementations of rules-based regulation, and it's important to be aware of them vis--vis DF. First, naive rules-based regulation is mostly backward-looking, effectively regulating earlier crises, with potentially very little relevance for future crises. It's terribly hard, as they say, to drive forward when looking only in the rear-view mirror. When the next major financial crisis hits, it will likely arrive via avenues that DF missed, and then DF will be augmented with another 2000+ pages of rules looking backward at *that* crisis, and so on, and on and on.

Second, naive rules-based regulation invites regulatory arbitrage. That is, as soon as rules are announced, firms start devising ways to skirt them. Indeed modern finance is in many respects an industry of very smart people whose job, for a given set of rules, is to work furiously to reverse-engineer those rules (they're doing it right now with respect to DF), devising clever ways to legally bear as much risk as possible while holding as little capital as possible, by finding and taking risks missed by the rules.

Third, naive rules-based regulation invites regulatory capture. That is, the regulated and the regulators get cozy, and the regulated eventually "capture" the regulator. First the regulators and regulated work side by side, implicitly or explicitly as in DF. Next the regulated are making "suggestions" for creative rule interpretation. Before long the regulated are helping to write the rules, crafting the very loopholes that they'll later exploit.

What to do? How to deal with the fundamental incompleteness of the regulatory contract? More specifically, given the long-term impotence of naive implementations of rules-based regulation, how *really* to thwart the adverse incentives of TBTF? If rules are unavoidable, and if naive rule implementations are problematic, are there better, sophisticated, implementations?

To be continued...

# Chapter 13

## Financial Regulation, Part 3: The Known, the Unknown and the Unknowable

Dick Herring, Neil Doherty and I recently worked on an eye-opening research project that resulted in our book, *The Known, the Unknown and the Unknowable in Financial Risk Management*. I always liked the cover art (thanks to Princeton University Press, which did its usual fine job throughout). I feel bad for the poor guy in the necktie, in the maze. But that's life in financial markets

We abbreviate the known, the unknown and the unknowable by **K**, **u** and **U**, respectively. Roughly, **K** is risk (known outcomes, known probabilities), **u** is uncertainty (known outcomes, unknown probabilities), and **U** is ignorance (unknown outcomes, unknown probabilities).

The book blurb on [my web page](#) reads as follows:

“On the successes and failures of various parts of modern financial risk management, emphasizing the known (**K**), the unknown (**u**) and the unknowable (**U**). We illustrate a **KuU**-based *perspective* for conceptualizing financial risks and designing effective risk management strategies. Sometimes we focus on **K**, and sometimes on **U**, but most often our concerns blend aspects of **K** and **u** and **U**. Indeed **K** and **U** are extremes of a smooth spectrum, with many

of the most interesting and relevant situations interior.”

The blurb continues:

“*Statistical* issues emerge as central to risk *measurement*, and we push toward additional progress. But *economic* issues of incentives and strategic behavior emerge as central for risk *management*, as we illustrate in a variety of contexts.”

The book’s table of contents reveals the breadth of that insight, from risk management, asset allocation, and asset pricing, to insurance, crisis management, real estate, corporate governance, monetary policy, and private investing:

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I say that the project was “eye opening” (for me) because I went into it thinking about econometrics, but I came out of it thinking about economics. Econometrics is invaluable for risk measurement, systemic aspects of which are embodied for example in the [Diebold-Yilmaz network connectedness framework](#), or in parts of [Rob Engle's V-Lab](#). But risk management, in particular risk prevention, is equally (or more) about creating incentives to guide strategic behavior, particularly in situations of **u** and **U**.

The real question, then, is how to write contracts (design organizations, design policies, design rules) that incent firms to “do the right thing,” *whatever that might mean, in myriad situations, many of which may be inconceivable at present* – that is, across the **KuU** spectrum.

How to do it?

To be continued...

[Yes, it will indeed be continued, despite the radio silence thus far. One or two more posts should do it, presumably in 2014...]





# Chapter 14

## The Latest in Statistical Graphics

In a recent gushing review, *The Economist* made [Data Points: Visualization that Means Something](#) by Nathan Yau (Wiley, 2013) sound like the elusive “Tufté for the 21st-Century” discussed in an earlier post ([Statistical Graphics: The Good, The Bad, and the Ugly](#), June 21, 2013). Alas, it’s not. Much of it is just inferior re-hash of old 20th-century Tufté. Nevertheless I like it and I’m glad I bought it. Among other things, there are some really cute examples, like one showing the available colors of Crayola crayons over time.

Yes, I know it’s not original to Yau, and I know it’s comparatively easy to use clever color in a Crayola graphic, but still...) Crayola also brings back good memories: I was a user/fan in the 64-color days of 1958-1972, not only for the awesome 64 colors but also for the totally-cool tiered box with built-in sharpener!

Perhaps most interesting is Yau’s final chapter, where he offers opinions on graphics software environments. (After all, he spends his life doing this stuff, so it’s interesting to learn his preferences.) At a high “canned” level, he likes [Tableau](#), the “[Tableau Public](#)” version of which is free. Well, nothing is really free, and Tableau Public follows an interesting paradigm: the price of using the web-based software is that users must upload their data so that others can use it.

But readers of this blog will be more interested in lower-level scientific software that allows for significant graph customization. In that regard, and not surprisingly, Yau is an R fan. (See my earlier post, [Research Computing / Data / Writing Environments](#), May 31, 2013.) He basically does all his graphics in R, but quite interestingly, he doesn't like to tune his graphs completely in R. Instead, he finalizes them using illustration software like the open-source [Inkscape](#). Hmmmm...

Yau's book also introduced me to his blog, [FlowingData](#), which is interesting and entertaining. Also see [Data Pointed](#), a fine blog by scientist and artist Stephen Von Worley, the author of the Crayola graphic above. And if you're really a Crayola maven, see his post, [Somewhere Over the Crayon-Bow](#).

Finally, and ironically, the most interesting thing about Yau's book is not explicitly discussed in it, yet it lurks massively throughout: Big Data and its interaction with graphics. More on that soon.

## Chapter 15

# GMM (the “Strange American Estimator”)

At three separate recent non-American conferences, I heard three separate European econometricians refer to generalized method of moments (GMM) as a “strange American estimator.” Needless to say, that raised my eyebrows. One doesn’t hear that phrase too often in, say, Stanford or Chicago or Cambridge (Massachusetts, that is).

Although I am American, I have some sympathy for the European view (if I may be so bold as to assert that my sample of size three has indeed uncovered a “view”). I may even have significantly more sympathy than do most Americans. But ultimately my feelings are mixed.

On the one hand, it seems clear that frequentist statisticians dismissed method-of-moments and minimum chi-squared (their term for GMM) ages ago as inefficient relative to MLE, and that Bayesian statisticians never dismissed them because they never paid them any attention in the first place. Instead, both communities have always thoroughly and intentionally focused on the likelihood – frequentists on the location of its max and its curvature in an epsilon-neighborhood of the max, and Bayesians on its entire shape.

Surely this historical background is what drives the European view. And against that background, I too am always a bit perplexed by the GMM phenomenon, as distilled for example in [Hayashi’s classic econometrics text](#), which

reads in significant part as something of a prayer book for the GMM congregation. (Never mind that my friend and former-colleague Hayashi is Japanese; his econometrics training and style are thoroughly American.)

That is, I must admit that, in part, I too am rather skeptical. Somehow my community just never got the religion. My belief is probably restrained significantly by the fact that my interest centers on dynamic predictive econometric modeling, which is often best done in reduced-form (see [No Hesitations, June 12, 2013](#)). Hence one of the grand sources of GMM moment conditions – orthogonality between instruments and disturbances in estimating causal effects – is, for me, typically neither here nor there.

On the other hand, my sympathy for the European view is far from complete. For example, some important classes of economic models produce moment restrictions but not full likelihoods. Despite the GMM crowd’s repeating that mantra ad *nauseum*, it’s as true now as ever. But if the story of GMM’s appeal ended with its usefulness when a model fails to produce a likelihood, I’d be underwhelmed. Maybe I’d even move to Europe.

What then do I find so additionally impressive about GMM? Stay tuned for the next post.

## Chapter 16

# More on the Strange American Estimator: GMM, Simulation, and Misspecification

What's so interesting, then, about GMM? For me there are two key things: its implementation by simulation, and its properties under misspecification.

First consider the implementation of GMM by simulation (so-called simulated method of moments, SMM).

GMM is widely-advertised as potentially useful when a likelihood is unavailable. In other cases the likelihood may be “available” but very difficult to derive or evaluate. But model moments may also be seemingly unavailable (i.e., analytically intractable). SMM recognizes that model moments are effectively *never* intractable, because they can be calculated arbitrarily accurately from an arbitrarily long model simulation. That's really exciting, because simulation ability is a fine litmus test of model understanding. If you can't figure out how to simulate pseudo-data from a given probabilistic model, then you don't really understand the model (or the model is ill-posed). Assembling everything: If you understand a model you can simulate it, and if you can simulate it you can estimate it consistently by SMM, choosing parameters to minimize divergence between data moments and (simulated) model moments. Eureka! No need to work out complex likelihoods, even if they are in principle

“available,” and in this age of Big Data, MLE efficiency lost may be a small price for SMM tractability gained.

Now consider the properties of GMM/SMM under misspecification, which is what intrigues me the most.

All econometric models are approximations to a true but unknown data-generating process (DGP), and hence likely misspecified. GMM/SMM has special appeal from that perspective. Under correct specification any consistent estimator (e.g., MLE or GMM/SMM) unambiguously gets you to the right place asymptotically, and MLE has the extra benefit of efficiency, so it’s preferable. But under misspecification, *consistency* distinguishes the estimators, quite apart from the secondary issue of efficiency. In particular, under misspecification the best asymptotic DGP approximation for one purpose may be very different from the best for another. GMM/SMM is appealing in such situations, because it forces you to *think* about which features of the data (moments,emphM) you’d like to match, and then by construction it’s consistent for the  $M$ -optimal approximation.

In contrast to GMM/SMM, pseudo-MLE ties your hands. Gaussian pseudo-MLE, for example, may be consistent for the KLIC-optimal approximation, but KLIC optimality may not be of maximal relevance. From a predictive perspective, for example, the KLIC-optimal approximation minimizes 1-step-ahead mean-squared prediction error, but 1-step quadratic loss may not be the relevant loss function. The bottom line: under misspecification MLE may not be consistent for what you want, whereas by construction GMM is consistent for what you want (once you *decide* what you want).

So, at least in part, GMM/SMM continues to intrigue me. It’s hard to believe that it’s been three decades since [Lars Hansen’s](#) classic GMM paper (1982, *Econometrica*), and two decades since the similarly-classic indirect inference papers of [Tony Smith](#) (1990, Duke Ph.D. Dissertation, and 1993, *J. Applied Econometrics*) and [Christian Gourieroux, Alain Monfort and Eric Renault](#) (1993, *J. Applied Econometrics*). (SMM is a special case of indirect inference.) If by now the Hansen-Smith-Gourieroux-Monfort-Renault insights seem obvious, it’s only because many good insights are obvious, *ex post*.

# Chapter 17

## Still More on the Strange American Estimator: Indirect Inference, MLE and the Particle Filter

In my last post I praised indirect inference (IE) for its ease-of-use: just simulate the model and fit a simple auxiliary model to the simulated and real-world data, after which evaluation of the objective is immediate. In contrast, likelihood analysis and MLE can be challenging, as the likelihood may be difficult to derive and evaluate.

Some might wonder whether that's a completely fair assessment in modern time-series contexts. In particular, one might claim that evaluation of the likelihood is now as trivial as simulating. As [Andrew Harvey](#) and others have emphasized for decades, for any linear model cast in finite-dimensional state-space form one can simply run the Kalman filter and then evaluate the Gaussian likelihood via a prediction-error decomposition. And much more recently, thanks to path-breaking work by [Arnaud Doucet](#) and others (e.g., [JRSS B, 2010, 1-33](#)), filtering now also provides full likelihood analysis in general non-linear / non-Gaussian environments. In particular, so-called “particle MCMC” – a simulation method! – does the trick. So it would seem that like-

likelihood analysis is made trivial by simulation, just as IE is made trivial by simulation.

Hence we can dispense with comparatively-inefficient IE, right?

Whoaaaanot so fast. The points I made in an earlier post remain valid.

First, IE simulation is good-old model simulation, typically simple and always a good check of model understanding. Successful particle MCMC, in contrast, is a different and often-recalcitrant simulation beast.

Second, even if particle MCMC does make MLE as mechanical as simple model simulation (and again, that's not at all clear), desirable consistency properties under misspecification are generally more easily achieved for IE. *Under misspecification, the necessity of thinking hard about which moments to match, or which auxiliary model to use, is a good thing.*



## Chapter 18

# Congrats to Marc Nerlove, AEA Distinguished Fellow

Congratulations to [Marc Nerlove](#), American Economic Association [Distinguished Fellow](#), as formally announced in the current issue of the *American Economic Review*. Marc was my Penn Ph.D. advisor in the 1980s and Penn colleague in the 1990s.

The American Economic Association's blurb is actually quite good. I've reproduced it below, with a few typos corrected. For interesting additional information, see Marc's [Econometric Theory interview](#) by [Eric Ghysels](#), also Marc's student.

In a career spanning 58 years and counting, Marc Nerlove developed widely used econometric methods in the course of addressing important empirical problems. In early research, he developed dynamic models of producer supply that enabled economists to distinguish and to quantify lags due to costs of adjustment and lags due to expectations of future events. In a series of influential papers, he applied these tools to the dynamics of agricultural supply and created a template that continues to be used on a wide scale in studies around the world. His framework made it possible to identify both short-run and long-run elasticities of supply in response to product price.

Nerlove pioneered the development of modern time series methods including the application of spectral analysis to aggregate economic time series and

the development of unobserved components and time series factor models that formalized the Burns-Mitchell decompositions into trend, cycle, and irregular components. This research stimulated the time series index models by Sargent, Sims, Geweke, Engle, Stock, Watson, and others.

Nerlove's research on the electricity industry in the early 1960s was the first application of duality theory to estimate production functions. He estimated cost functions and from them obtained estimates of firm technology. His magisterial book, *Estimation and Identification of Cobb-Douglas Production Functions*, helped to introduce the concept of partial identification into econometrics and is a prototype for synthesizing economics and statistics to address important economic questions.

Nerlove pioneered the analysis of panel data in econometrics. His fundamental work with Balestra and his subsequent solo research developed widely used frameworks for analyzing dynamic models for panel data in the presence of individual-specific temporally persistent unobservables. The research arose from a practical problem in analyzing and interpreting estimates of the demand for durable goods.

Nerlove, with Razin, has also done basic research on economic demography and life cycle fertility in dynamic equilibrium settings with overlapping generations.

Throughout his long and distinguished career, Nerlove has exemplified the best in applied economics. He brings rigor to the study of important economic problems. He developed empirically relevant econometric tools and showed by example the importance of using economics and econometrics to analyze economic data. Marc Nerlove's appointment as Distinguished Fellow of the American Economic Association recognizes his outstanding contributions to economics and econometrics.

## Chapter 19

# Universities, Parking, and Bonding

Ya gotta love Bill Barnett's email sig: "A university is 'a series of individual faculty entrepreneurs held together by a common grievance over parking'" (Clark Kerr, President, University of California, 1958-1967).

I refuse to drive to work anymore, for a variety of reasons, but still I relate.



## Chapter 20

# Krugman’s “Very Serious Person” (VSP)

Paul Krugman’s term “VSP” is simply wonderful: so concise and apt, capturing a personage previously vaguely sensed but never fully grasped. And of course it’s funny too. Hence it’s even better than classics from decades past, like WASP (coined, by the way, by the late great Penn sociologist, E. Digby Baltzell), which was concise and apt but not funny. I give Krugman a sociological gold star just for coining the term.

Some use VSP to describe pundits and politicians with misguided beliefs, but who are somehow still viewed as respectable. For me the term resonates more broadly, describing generic inside-the-beltway types, especially economist types, as in Krugman’s usage referring to Larry Summers, “Hes been carefully cultivating an image as a Very Serious Person” ([31 July 2013](#)).

Sadly, the Washington VSPdom sucks away some of the finest scientific talent in economics. The reason is misguided professorial benefit-cost comparisons that naively inflate the benefits of “helping the world” and deflate the costs of abandoning research. A top researcher doing path-breaking research *is* helping the world! And who’s helping the world more – a top researcher doing path-breaking research, or that same researcher transformed into a dark-suited VSP roaming the halls of the Old Executive Office Building, jockeying for Very Serious opportunities to attend Very Serious meetings to discuss Very

Serious things?

I'm grateful to Krugman for doing his benefit-cost calculations correctly, refusing to let VSPdom suck *him* away. His punditry – like it or hate it – is immensely more socially valuable than whatever he might contribute as Big Kahuna at the Department of Whatever. Here's to more top academics joining the resistance.

# Chapter 21

## Exponential Smoothing and Stochastic Volatility

Exponential smoothing is alive and well, and evolving. For the latest, check out [Neil Shephard's](#) important 2013 working paper, “[Martingale Unobserved Component Models](#).” (Fortunately for North America, the link to Neil's home page will soon be outdated – he's moving to Harvard as I write this. Congratulations to Harvard's Departments of [Economics](#) and [Statistics](#), and of course to Neil as well.)

In part the paper is interesting because it provides useful perspective on state-space modeling, filtering and estimation from the early linear/Gaussian days of Kalman filtering to the recent nonlinear/non-Gaussian days of particle filtering. There's also some interesting personal reflection. (Background: the paper is for a forthcoming [Andrew Harvey Festschrift](#), and Neil was Andrew's student.)

But the paper's original contribution is even more interesting. It puts exponential smoothing in fresh and fascinating perspective, by considering it in a stochastic volatility (SV) environment.

As is well known, exponential smoothing (ES) is closely related to state-space models of unobserved components. In particular, ES is the MSE-optimal filter when the data-generating process is a latent random walk signal buried in white noise measurement error. The optimal smoothing parameter, moreover,

depends only on the signal / noise ratio (that is, the random walk error variance relative to the measurement error variance).

Neil endows the errors with SV, in which case the signal / noise ratio and hence the optimal smoothing parameter are time-varying. The particle filter facilitates both optimal parameter estimation and optimal tracking of the time-varying volatility, making for real-time ES with an optimally time-varying smoothing parameter. Very cool, both in principle and practice!

More generally, it's interesting that ES remains alive and useful and still the focus of important research, some half-century after its introduction. Seemingly-naive methods sometimes reveal themselves to be sophisticated and adaptable.



## Chapter 22

# Exponential Smoothing Again: Structural Change

Here's another fascinating example of the ongoing and surprisingly modern magic of exponential smoothing (ES).

In my last post I asked you to read the latest from Neil Shephard, on stochastic volatility and exponential smoothing. Now read the latest from [Hashem Pesaran, Andreas Pick and Mikhail Pranovich \( \$P^3\$ \)](#), “[Optimal Forecasting in the Presence of Structural Breaks](#)” (forthcoming, *Journal of Econometrics*), on structural change and yes, again, exponential smoothing.

Let's strip things to a starkly simple stylized case. (The basic idea generalizes to much richer environments.) Consider a time-series forecasting situation with a one-shot structural break in all model parameters at a known past time. Should you simply discard the pre-break data when estimating your model? Your first reaction might be yes, as the pre-break regime is irrelevant moving forward, and your goal is forecasting.

But the correct answer is “not necessarily.” Of course using the full sample will produce a mongrel estimation blend of pre-break and post-break parameters. That is, using the full sample will produce biased estimates of the relevant post-break parameters. But using the full sample may greatly reduce variance, so estimation mean-squared error of post-break parameters may be lower, perhaps much lower, when estimating using the full sample, which may

then translate into lower out-of-sample mean-squared prediction error (MSPE) when using the full-sample estimated parameters.

Suppose, to take a stark example, that the break is miniscule, and that it's near the end of a very long sample. The cost of full-sample estimation is then injection of miniscule bias in estimates of the relevant post-break parameters, whereas the benefit is massive variance reduction. That's a very favorable tradeoff under quadratic loss.

Fine. Good insight. (Interesting historical note: Hashem mentions that it originated in a conversation with the late Benoit Mandelbrot.) But there's much more, and here's where it gets really interesting. Just as it's sub-optimal simply to discard the pre-break data, it's also sub-optimal simply to keep it. It turns out, quite intuitively, that you want to keep the pre-break data but downweight it, and the MSPE-optimal weight-decay scheme turns out to be exponential! In less-rigid forecasting environments involving continuous structural evolution (also considered by  $P^3$ ), that basically amounts to exponential smoothing. Very, very cool.

Strangely,  $P^3$  don't attempt to connect to the well-known and clearly-related work of Mike Clements and David Hendry (CH), which  $P^3$  clarify and extend significantly. In several papers, CH take ES as an exogenously existing method and ask why it's often hard to beat, and closely related, why the martingale "no change" forecast is often hard to beat. See, e.g., section 7 of their survey, "Forecasting with Breaks," in Elliott, Granger and Timmermann, (eds.) *Handbook of Economic Forecasting, 2005*, Elsevier. The CH answer is that breaks happen often in economics, and that ES performs well because it adapts to breaks quickly.  $P^3$  instead ask what approach is *optimal* in various break environments and arrive endogenously at ES. Moreover, the  $P^3$  results are interestingly nuanced. For example, ES is closest to optimality in situations of continuous structural change, not in situations of discrete breaks as emphasized by CH. In any event, the  $P^3$  and CH results are marvelously complementary.

Exponential smoothing is again alive and well, from yet another, and very different, perspective.

## Chapter 23

# *Econometrica*, Heteroskedasticity and the Greeks

Over at *Leisure of the Theory Class*, [Ricky Vohra](#) asks why *Econometrica* is spelled with a “c” rather than a “k.” As he notes: The journal *Econometrica* is spelt as I just wrote. The journal *Biometrika*, however, has a “k” in place of the letter “c.” *Biometrika* has it right, and Pearson its founder credits Edgeworth for this, “...like a good German he knew that the Greek ‘k’ is not a modern ‘c’, and, if any of you at any time wonder where the ‘k’ in *Biometrika* comes from, I will frankly confess that I stole it from Edgeworth. Whenever you see that ‘k’ call to mind dear old Edgeworth.” Ricky (and Edgeworth and Pearson) are correct. Interestingly, [Hu McCulloch](#) has a long-ago note on the same topic, in Hu’s case dealing with heteroscedasticity vs. heteroskedasticity. It’s cutely titled “Heteros\*edasticity,” and he makes clear that “\*” should be “k,” for the same Greek root reasons. McCulloch’s note is little-known, or perhaps well-known but widely-ignored by philistine econometricians, as “c” is still often used despite the note’s publication in one of the profession’s most elite and visible journals. You guessed it – such superb irony – *Econometrica*!

Check it out: [McCulloch, J. Huston, “On Heteros\\*edasticity,” \*Econometrica\*, 53, 1985, p. 483.](#) It’s less than one page, authoritative and also humorous,

beginning with “The most pressing issue in econometric orthography today is whether heteros\*edasticity should be spelled with a ‘c’ or with a ‘k’.”

## Chapter 24

# Is Economics too Important for Economists?

Like piranha fish in a feeding frenzy, different research tribes fight furiously to stake claims in new areas like financial engineering and risk management. Fringe players, in particular, often strive to move toward the center, or to redefine the center in ways that feather their nests.

Such competition is desirable, but healthy competition is based on merit, not mudslinging. Hence my disappointment when watching the video preview for “[Financial Engineering and Risk Management Part I](#),” a massively open online course (MOOC) by [Martin Haugh](#) and [Garud Iyengar](#) (H&I) at Columbia, to be given soon on [Coursera](#). H&I come from [Industrial Engineering and Operations Research](#), and they conclude their sales pitch with the brazen proclamation, “... it’s often said that economics is too important to be left to economists. Well, we feel the same way about finance and financial engineering. It’s too important to be left to economists...”

Wow, strong words. So what’s in their syllabus? Here it is:

- Introduction to forwards, futures and swaps
- Introduction to options and the 1-period binomial model
- The multi-period binomial model and risk-neutral pricing
- Term structure models and pricing fixed income derivative securities
- Introduction to credit derivatives

- Introduction to mortgage mathematics and mortgage-backed securities

Huh? *What?* Isn't that largely financial economics, pioneered and continuously refined by an ongoing parade of financial economists? Of course. Indeed what else *could* it be?

A quick glance at the web indicates that H&I's research is high-quality, and I hope that the same will be true for their course. (I have registered.) I'm also glad that H&I are contributing to the wonderful Coursera MOOC phenomenon, and I applaud their declared desire to increase lay financial literacy. But I suggest that they and their tribe give credit where credit is due – is that not necessary for true literacy? – and think twice before glibly biting the financial economics hand that feeds them.

## Chapter 25

# Forecast-Error Insurance

Yale economists Mark Rosenzweig and Chris Udry have a new paper, “Forecasting Profitability.” It’s related to a project of theirs that examines the Indian Meteorological Department’s annual monsoon rainfall forecasts and their effects on farmer profitability. (Farmers who rely on the forecast to guide their planting-stage investments, and many do, are exposed not only to rainfall risk, but also to “forecast-error risk.”)

Mark raised an interesting related question in an email a few weeks ago, “Do you know of any insurance products for forecasts of any kind? Can one purchase insurance that indemnifies one against forecast errors, anywhere in the world for any forecasts?”

Hmm...quite fascinating! There are financial products insuring against rainfall events (e.g., see the discussion my 2005 *JASA* paper with Sean Campbell, “Weather Forecasting for Weather Derivatives”), but what about rainfall forecast-error events?

Of course the “forecast-error insurance” issue transcends rainfall. More generally, who might demand forecast-error insurance and why? Only users of forecasts? Perhaps also producers of forecasts (not unlike medical malpractice insurance)? Who might supply it, and how might it be priced (*very* tricky...)?

Here are some thoughts.

First, forecast object  $x$  and the associated forecast error  $e = x - x_f$  will generally be correlated, in which case insurance against  $x$  events is also partial

insurance against  $e$  events. (Note well, however, that the implicit  $e$  insurance is only partial, and perhaps *very* partial, depending on the correlation strength).

Second, assuming that the relevant financial markets exist, one could construct an  $e$  hedge by holding an appropriate portfolio of  $x$ -sensitive stocks, which in an efficient market would reflect discounted  $x$  forecasts and hence move only due to “news” ( $e$ ). Long or short positions in that portfolio would hedge against positive  $e$  or negative  $e$ . Better yet, one could implement the hedge using stock options rather than the underlying, going long a call or long a put (or both, a so-called “straddle,” which would hedge against large  $e$  of either sign).

Maybe I’ve missed something? Are the questions well-posed, and if so, are there simpler or more obvious answers? In any event, many assumptions obviously lurk behind the thoughts / suggestions above, but nothing is impossible for the man who doesn’t have to do it himself.



# Chapter 26

## 60 Seconds on Measurement vs. Theory in all the Sciences

At Penn we have something of a unique tradition, an ongoing series of “60-Second Lectures.” Four Penn profs speak each semester, outdoors, at noon on four consecutive Wednesdays. And yes, each of the four lectures is capped at 60 seconds.

60-Second Lectures sound insane. But somehow they work. They’re on Locust Walk, the massively-trodden main drag. There’s a podium and lights and a microphone, and some people actually stop to listen, and it’s all quite an entertaining spectacle, even if 60-Second Cage Fights would be much better. The video dudes record it, and then it’s posted for the masses in Penn’s [online archives](#) as well as its Facebook, YouTube, and Vimeo pages. (Vimeo? Who knows...) Then the video goes completely viral, catapulting the 60-Second Lecturer to unprecedented heights of global intellectual celebrity. At least that’s what the Dean’s Office tells me. Or something like that. Anyway, you surely see through it. Only a complete sucker would agree to attempt a 60-Second Lecture. Yes, that’s right, I agreed.

But seriously, the [60-Second Lectures](#) are a wonderful Penn tradition, and I’m delighted to contribute my minute’s worth. My title will be “Measurement vs. Theory in all the Sciences.” Join us if you’re in Philadelphia. It’s Wednesday, September 18, 11:55 sharp. (If you’re more than a minute late,

you'll miss everything!) It's at Stiteler Plaza, 37th and Locust Walk, unless the cheering throng exceeds 35,000, in which case we move to Franklin Field.

## Chapter 27

# Theory gets too Much Respect, and Measurement Doesn't get Enough (60-Second Lecture)

In the last post, I told you about Penn's "60-Second Lecture". Mine is now completed, and we had a good time. Watch the video, and you'll have a good laugh at the unflattering opening shot, complete with a barking dog in the background, and the blinding sun in my face throughout. A rough transcription follows.

Science is advanced by just two things, measurement and theory. Their interplay pushes science forward, as each disciplines the other.

Some people believe that good research requires tightly-integrated measurement and theory, present in equal amounts.

I submit to you, first, that such views are both naive and false. Measurement and theory are rarely advanced at the same time, by the same team, in the same work. And they don't need to be. Instead we exploit the division of labor, as we should. Measurement can advance significantly with little theory, and theory can advance significantly with little measurement. Still each disciplines the other in the long run, and science advances.

And I submit to you, second, and primarily, and perhaps provocatively, that theory gets too much respect in science, and that measurement doesn't

get enough. A wry observer once remarked that theorists typically have the top-floor offices, while experimentalists and statisticians are tucked away in the basement. But Lord Kelvin got it right more than a century ago, when he argued that *measurement is the essence of science*. And moreover, theory is largely data distillation, attempting to eliminate noise and isolate signal, and hence *dependent* on measurement. The theory mill grinds *data*.

Of course it's futile, and it's not at all my intent, to assert that one or the other of measurement and theory is somehow uniquely important, or even relatively more important. Each is *equally* important, and each most definitely needs the other. But again, that's the point: given their truly equal importance, they should get equal attention and respect. Instead theory gets too much, and measurement doesn't get enough. Perhaps that will change in the emerging age of Big Data.

# Chapter 28

## Deep Inside the LSE

Mary Morgan, Professor of History and Philosophy of Economics at LSE (Department of Economic History) is giving a talk at Penn today, in our Department of History and Sociology of Science. She's done interesting work in the history of econometrics, and more recently in the methodology of economics. We've never met, so I'm looking forward to her talk.

The occasion led me to pop into the LSE web site, which was fun because I haven't been at LSE in ages. Of course one is aware of the LSE leaders in one's own fields (for me, econometrics, economics, finance), but one forgets that there's much, much more there. Look at all the topics and people stuffed into the LSE (Accounting, Anthropology, Economics, Economic History, European Institute, Finance, Gender Institute, Geography & Environment, Government, International Development, International History, International Relations, Language Centre, Law, Management, Mathematics, Media & Communications, Methodology, Philosophy, Logic and Scientific Method, Public Affairs (IPA), Social Policy, Social Psychology, Sociology, Statistics). Truly unique! Basically anything that would fit in *The Economist* fits somewhere at LSE. Even their logos look similar, with white letters on a bright red background. Must be a British thing.



## Chapter 29

# Sheldon Hackney: A Truly Great Penn Man

[Sheldon Hackney](#), Penn's president 1981-1993, recently passed away. See the fine coverage in the [Almanac](#) and [Daily Pennsylvanian](#).

In my younger days as a Penn undergrad, Hackney took a lot of abuse. People felt that he didn't have much backbone. Exhibit 1 was always his failure to stand up to the [water buffalo thing](#), letting political correctness run amok. I too felt that way.

But now, with the benefit of more information providing breadth and depth of hindsight, I see that I was wrong. Yes, he botched the water buffalo thing. But now I see that as just a small detour in a breathtakingly heroic career.

Thanks, Sheldon, for seeing things clearly in Alabama, for taking your case to Penn, the nation, and the world, for testifying to the truth, day in and day out. Thanks for relishing the joy of being a professor first and always. Thanks for engaging Penn's neighbors, and for laying the groundwork for Penn's eastward expansion. And thanks for your famous civility, your stunning grace under pressure, a model for us all. I recall the Fleetwood Mac lyric, from just slightly before your Penn arrival, "Can I sail through the changing ocean tides, can I handle the seasons of my life?" May I do half as well as you.





## Chapter 30

# Another Nobel for Time Series Econometrics?

Thomson Reuters makes annual Nobel Prize forecasts in chemistry, physics, medicine and economics, based on citation counts from its Web of Science database (no surprise). Of course the exercise is largely a marketing tool for their database, but it's still fun and timely.

They boast that their algorithm “has accurately forecast 27 winners since its inception in 2002.” It's not clear how to interpret that statement, and whether the algorithm's performance is nearly as impressive as the statement suggests. (Each year they forecast several prizes, typically shared two or three ways, in each of the fields. If I'm guessing correctly, they score a “correct forecast” whenever the name of any current winner appeared at any earlier time as part of any of their forecasted prizes.)

Anyway, as I said, it's still fun and timely. This year they identify three top candidate prizes in economics:

David Hendry, Hashem Pesaran and Peter Phillips: For their contributions to economic time-series, including modeling, testing and forecasting

Josh Angrist, David Card and Alan Krueger: For their advancement of empirical microeconomics

Sam Peltzman and Richard Posner: For extending economic theories of regulation.

Quite a fine collection!

# Chapter 31

## Big Data the Big Hassle

The hype surrounding “Big Data” has escalated to borderline nauseating. Is it just a sham?

Yes, I know, [I have earlier gushed about the wonders of Big Data](#). But that was then, and now is now, and I hear my inner contrarian alarm sounding.

One thing is clear: Big Data the *phenomenon* is not a sham. It’s here, it’s real, and it must be taken seriously. The ongoing explosion in the quantity of available data, largely the result of recent and unprecedented advancements in data recording and storage technology, is not going away. It’s emerging as one of the defining characteristics of our time.

Big Data the *business* isn’t really a sham either, even if it’s impossible not to smirk when told, for example, that that major firms are rushing to create new executive titles like “Vice President for Big Data.” (I’m not making this up. See [Steve Lohr’s New York Times piece](#).) Big Data consultants and software peddlers smell Big Money, and they’re salivating profusely. But there’s nothing necessarily wrong with that, even if it isn’t pretty.

But what about Big Data the *scientific field*? What is it? Where’s the beef? What’s really new, for example, statistically? Of course Big Data has stimulated much fine new work in dimensionality reduction, shrinkage, selection, sparsity, regularization, etc. But are those not traditional areas? In what sense is the scientific Big Data whole truly greater than the sum of its earlier-existing parts?

But primarily: Why all the endless optimistic Big Data buzz about endless Big Data opportunities? What about *pitfalls*? Isn't Big Data in many respects just a hassle? Aren't we still searching for needles in a haystack, except that the haystack is now growing much more quickly than the needle-discovering technology is improving? Why is that cause for celebration?

## Chapter 32

# Federal Reserve Research: Wake Up Before It's Too Late

I am familiar with the U.S. Federal Reserve System. Long ago I spent the first three (wonderful) years of my working life as an economist at the Board of Governors in DC, 1986-1989. Most recently I chaired the Fed's Model Validation Council, 2012-2013. In the intervening years I've had many engagements with the System, and I've sent many of my Ph.D. students, perhaps twenty, to work there.

So believe me when I say that during the last half-century, there were few better places in the world for a research economist to work, universities included. The research staff quality and esprit de corps were unmatched. Runner-up institutions, world-wide, were miles behind. And believe me as well when I say that I'm now worried.

When I read the recent Huffington Post piece, "[Federal Reserve Employees Afraid To Speak Put Financial System At Risk](#)," I was pretty alarmed. I figured it must be strongly negatively biased, so I made some personal inquiries. No – pretty accurate. Wow. Well, I noticed, it focuses mostly on the Board's division of Supervision and Regulation (Sup&Reg), filled with lawyers. Surely the Board's key research divisions (Research and Statistics, Monetary Affairs, International Finance), filled with economists, are as healthy as ever. So I made some more inquiries. Not yet a Sup&Reg situation, but lots of bewil-

derment, concern, and top talent looking, or thinking of looking, for greener pastures. *Wow.*

I understand that we just went through the worst recession since the Great Depression, and that enforcing the ensuing legislation requires a major effort. But I also understand that effective institutions and stellar reputations take half-centuries to build but can collapse quickly, and moreover that, at a deep level, the Fed's research prowess is largely responsible for its respect and effectiveness. So if a new Fed regulatory culture must be built, then build it, but Fed senior management needs simultaneously to preserve and promote the serious research culture that drives Fed effectiveness. Related, people who don't deeply understand and appreciate serious research should never, ever, be promoted to senior management in divisions like Research and Statistics, Monetary Affairs, and International Finance.

## Chapter 33

# Pure Brilliance From FRB St. Louis: EconomicAcademics.org

This just in from Christian Zimmermann and the RePEc Team at FRB St. Louis:

“Congratulations, you made the list! .. The Federal Reserve Bank of St. Louis is launching a blog aggregator, [EconomicAcademics.org](http://EconomicAcademics.org), to highlight and promote the discussion of economics research. Your blog is part of this effort. This email explains why and how you can help promote the discussion of economic research in the blogosphere ... EconAcademics.org lives at <http://econacademics.org/> and aggregates blog posts that discuss economic research. The aggregator looks through blog posts for a link to some research indexed on a RePEc service, currently EconPapers, IDEAS and NEP. IDEAS then also links back from the abstract page to the blog posts ... This blog aggregator is provided by the Federal Reserve Bank of St. Louis, which also offers with FRED database and graphing tool as a useful resources for bloggers. Feel free to use the graphs on your blog, best done by embedding them so that readers can click on them to get more details about the data. FRED lives at <http://research.stlouisfed.org/fred2/>.”

This is totally brilliant. First, it’s a brilliant public service. I am grateful. Everyone should be grateful. But second, and this is really what I want to emphasize, ya gotta love the brilliant business/marketing move. Instantly,

every blogger now has a strong incentive to report on (and link to) RePEc papers whenever possible – in case you missed it above, blog posts get noticed by the aggregator only if/when they link to a RePEc paper – and hence authors have a correspondingly strong incentive to put their papers on RePEc. And it's all tangled up with the wonderful FRED. The idea may not make billions for FRBSL/RePEc/FRED, but in its own way it's as brilliant as Google's pagerank (and cynics will say as obvious – just sour grapes).

Oh wait. I forgot to mention a RePEc paper above, so this post won't get picked up by EconAcademics. Hmm... In the future I'll have to change that...

Anyway, SSRN et al. must be reeling! Of course it will be interesting to see how they and others respond. FRBSL/RePEc/FRED have scored a significant first-mover blow, but surely the fight isn't over. And as usual with healthy competition, everyone will benefit.



## Chapter 34

# Why You Should Join Twitter

Sounds silly, but it's not. I got talked into joining a few weeks ago, and I'm glad I did. I rarely tweet (except to announce new *No Hesitations* posts), but I follow others. Several times in the last few weeks alone, various pieces of valuable information arrived. Great stuff, *seriously*. So join Twitter and follow @FrancisDiebold.



## Chapter 35

# A Nobel for Financial Econometrics

First it was Engle and Granger (2003); now it's Fama, Hansen and Shiller.

A central issue in the economics of financial markets is whether and how those markets process information efficiently, to arrive at fair prices. Inextricably linked to that central issue is a central tension: certain lines of argument suggest that financial markets should be highly efficient, yet other lines of argument suggest limits to market efficiency. Gene Fama, Lars Hansen and Bob Shiller have individually and collectively made landmark contributions that now shape both academic and practitioner thinking as regards that tension. In so doing they've built much of the foundations of modern financial economics and financial econometrics. Fama empirically championed the efficient markets hypothesis, which in many respects represents the pinnacle of neoclassical financial economics. Shiller countered with additional empirical evidence that seemingly indicated the *failure* of market efficiency, setting the stage for several decades of subsequent work. Throughout, Hansen supplied both powerful economic theory that brought asset pricing in closer touch with macroeconomics, and powerful econometric theory (GMM) that proved invaluable for empirical asset pricing, where moment conditions are often available but likelihoods are not.

If today we celebrate, then tomorrow we return to work – obviously there's

more to be done. But for today, a resounding *bravo* to the three deserving winners!

## Chapter 36

# Network Estimation for Time Series

Matteo Barigozzi and Christian Brownlees have a fascinating new paper, “[Network Estimation for Time Series](#)” that connects the econometric time series literature and the statistical graphical modeling (network) literature. It’s not only useful, but also elegant: they get a beautiful decomposition into contemporaneous and dynamic aspects of network connectedness. Granger causality and “long-run covariance matrices” (spectra at frequency zero), centerpieces of modern time-series econometrics, feature prominently. It also incorporates sparsity, allowing analysis of very high-dimensional networks.

If I could figure out how get LaTeX/Mathjax running inside Blogger, I could show you some details, but no luck after five minutes of fiddling last week, and I haven’t yet gotten a chance to return to it. (Anyone know? [Maybe Daughter 1 is right and I should switch to WordPress?](#)) For now you’ll just have to click on the Barigozzi-Brownlees paper above, and see for yourself.

It’s interesting to see that Granger causality is alive and well after all these years, still contributing to new research advances. And Barigozzi-Brownlees is hardly alone in that regard, as the recent biomedical imaging literature illustrates. Some of [Vic Solo’s](#) recent work is a great example.

Finally, it’s also interesting to note that both the Barigozzi-Brownlees and [Diebold-Yilmaz](#) approaches to network connectedness work in vector-

autoregressive frameworks, yet they proceed in very different, complementary, ways.

# Chapter 37

## Lawrence R. Klein, 1920-2013

I am sad to report that Lawrence R. Klein has passed away. He was in many respects the father of modern econometrics and empirical macroeconomics; indeed his 1980 Nobel Prize citation was “for the creation of econometric models and their application to the analysis of economic fluctuations and economic policies.” He was also a dear friend and mentor to legions of Penn faculty and students, including me. I am grateful to him for many things, including his serving on my Penn Ph.D. dissertation committee nearly thirty years ago.

You can find a beautiful and fascinating autobiographical essay written in 1980, and updated in 2005, at [http://www.nobelprize.org/nobel\\_prizes/economic-sciences/laureates/1980/klein-bio.html](http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1980/klein-bio.html).

Update 1: KLEIN LAWRENCE, October 20, 2013, of Gladwyne, Pa. Husband of Sonia (nee Adelson). Father of Hannah Klein, Rebecca (James) Kennedy, Rachel (Lyle) Klein and Jonathan (Blandina) Klein. Also survived by 7 grandchildren and 4 great-grandchildren. Services and Interment are private. Relatives and friends are invited to the residence of Mrs. Sonia Klein Wednesday, October 23, 2-4 P.M. AND Saturday, October 26, 2-4 P.M. (only). Contributions in his memory may be made to the University of Pennsylvania Department of Economics.

Update 2: Extensive New York Times and Financial Times obituaries

Update 3: Penn Economics memorial statement at <http://economics.sas.upenn.edu/faculty/memorial/lawrence-r-klein>.





## Chapter 38

# NBER/NSF Time-Series Conference: Retrospect and Prospect

I recently reported here on the Barrigozzi-Brownlees paper, “[Network Estimation for Time Series](#).” I heard it presented a few weeks ago at the 2013 NBER/NSF Time Series Conference, hosted this year by the Federal Reserve Board in Washington (a sign, by the way, of the FED’s ongoing research commitment, notwithstanding my [earlier-posted doubts](#)).

I hadn’t attended NBER/NSF Time Series meeting in several years. Attending reminded me of how special it is and jogged me into this post on NBER/NSF more generally. What’s most unique is the way the conference spans so many different communities, all of which do top work in time series but not all of which communicate regularly. For some reason my mind groups into pairs many of the great researchers who participated regularly over the years: Rob Engle and Clive Granger, George Tiao and Arnold Zellner, Jim Stock and Mark Watson, Ted Hannan and Manfred Deistler, Torben Andersen and Tim Bollerslev, Peter Brockwell and Richard Davis, Ron Gallant and George Tauchen, David Findley and Bill Bell, and on and on.

General ongoing info about the conference is at <https://sites.google.com/site/nbernsfts/home> (including upcoming 2014-2016 meetings in St.

Louis, Vienna and New York). An interesting brief history – including year-by-year locations – is at <https://sites.google.com/site/nbernsfts/history>. Programs for recent years are at <https://sites.google.com/site/nbernsfts/history>. Does anyone know whether a complete set of conference programs is available? It would be fascinating to watch the parade of paper titles and authors marching forward from the earliest times.

FYI this year’s program follows.

#### 2013 NBER-NSF Time Series Conference

A conference hosted by the Federal Reserve Board *September 26-27, 2013, Washington, D.C.*

Thursday, September 26, 2013 Conference Registration and Box Lunch: 12:00

1:15 Opening Remarks: 1:15 1:30 *Main Program Session: Factor Models and Latent Variables: 1:30 3:00*

“Generalized Method of Moments with Latent Variables” A. Ronald Gallant, Raffaella Giacomini, Giuseppe Ragusa

“Shrinkage Estimation of Dynamic Factor Models with Structural Instabilities” Xu Cheng, Zhipeng Liao, Frank Schorfheide

“Structural FECM: Cointegration in Large-scale Structural FAVAR Models” Anindya Banerjee, Massimiliano Marcellino, Igor Masten

Coffee Break: 3:00 3:30

*Main Program Session: Forecasting and Model Evaluation: 3:30 5:00*

“Alternative Tests for Correct Specification of Conditional Predictive Densities” Barbara Rossi, Tatevik Sekhposyan

“Non-nested Model Comparisons for Time Series via the Gaussian Likelihood Ratio Statistic” Tucker McElroy, Christopher Blakely

“Efficient Test for Long-Run Predictability: Hybrid of the Q-test and Long-Horizon Regressions” Natalia Sizova

Cocktail Reception and Poster Session 1: 5:00 6:30

Conference Dinner: 6:30 8:30

*Dinner Speaker: Professor George Tiao, University of Chicago, Booth School of Business, “A Tribute to Professor George E.P. Box”*

Friday, September 27, 2013

Continental Breakfast: 8:00–9:00

*Main Program Session: Time Series Analysis: 9:00–10:30*

“Thresholded Multivariate Regression with Application to Robust Forecasting” Ranye Sun, Mohsen Pourahmadi

“Detecting Seasonality in Unadjusted and Seasonally Adjusted Time Series” David F. Findley, Demetra P. Lytras

“Approximate Bias in Time Series Regressions” Kenneth D. West

Coffee Break: 10:30–11:00

*Main Program Session: Macroeconomics: 11:00–12:30*

“Reverse Kalman Filtering US Inflation with Sticky Professional Forecasts” James M. Nason, Gregor W. Smith

“Improving GDP Measurement: A Measurement-Error Perspective” Boragan Aruoba, Francis X. Diebold, Jeremy Nalewaik, Frank Schorfheide, Dongho Song

“Systemic Risk and the Macroeconomy: An Empirical Evaluation” Stefano Giglio, Bryan Kelly, Seth Pruitt, Xiao Qiao

Lunch and Poster Session 2: 12:30–2:00

*Main Program Session: Macro/Finance: 2:00–3:30*

“Daily House Price Indexes: Construction, Modeling, and Longer-Run Predictions” Tim Bollerslev, Andrew Patton, Wenjing Wang

“Estimation of non-Gaussian Affine Term Structure Models” Drew D. Creal, Jing Cynthia Wu

“Robust joint Models of Yield Curve Dynamics and Euro Area (non-)standard Monetary Policy” Geert Mesters, Berd Schwaab, Siem Jan Koopman

Coffee Break: 3:30–4:00

*Main Program Session: Estimation: 4:00–5:30*

“Nets: Network Estimation for Time Series” Matteo Barigozzi, Christian Brownlees

“A Parameter Driven Logit Regression Model for Binary time Series” Rongning Wu, Yunwei Cui

“Definitions and representations of multivariate long-range dependent time series” Stefanos Kechagias, Vladas Pipiras

*Poster Session 1*

“Extended Yule-Walker Identification of a VARMA Model Using Single- or Mixed-Frequency Data” Peter A. Zadrozny

“Testing for Cointegration with Temporally Aggregated and Mixed-frequency Time Series” Eric Ghysels, J. Isaac Miller

“Co-summability: From Linear to Non-linear Co-integration” Vanessa Berenguer-Rico, Jesus Gonzalo

“An Asymptotically Normal Out-Of-Sample Test of Equal Predictive Accuracy for Nested Models” Gray Calhoun

“Nonparametric HAC Estimation for Time Series Data with Missing Observations” Deepa Dhume Datta, Wenxin Du

“Evaluating Forecasts from Bayesian Vector Autoregressions Conditional on Policy Paths” Todd E. Clark, Michael W. McCracken

“Marcenko-Pastur Law for Time Series” Haoyang Liu, Alexander Aue, Debashis Paul

“Dynamic Compositional Regression in Financial Time Series and Application in Portfolio Decisions” Zoey Yi Zhao, Mike West

“Diagnosing the Distribution of GARCH Innovations” Pengfei Sun, Chen Zhou

“Nonlinearity, Breaks, and Long-Range Dependence in Time-Series Models” Eric Hillebrand, Marcelo C. Medeiros

“Measuring Nonlinear Granger Causality in Mean” Xiaojun Song, Abderrahim Taamouti

“Penalized Forecasting in Panel Data Models: Predicting Household Electricity Demand from Smart Meter Data” Matthew Harding, Carlos Lamarche, M. Hashem Pesaran

*Poster Session 2*

“What is the Chance that the Equity Premium Varies over Time? Evidence from Regressions on the Dividend-Price Ratio” Jessica A. Wachter, Missaka Warusawitharana

“Forecasting with Many Models: Model Confidence Sets and Forecast Combination” Jon D. Samuels, Rodrigo M. Sekkel

“Modelling Financial Markets Comovements: A Dynamic Multi Factor Ap-

- proach” Martin Belvisi, Riccardo Pianeti, Giovanni Urga
- “On the Reliability of Output-Gap Estimates in Realtime” Elmar Mertens
- “Testing for Granger Causality with Mixed Frequency Data” Eric Ghysels,  
Jonathan B. Hill, Kaiji Motegi
- “Testing Stationarity for Unobserved Components Models” James Morley,  
Irina B. Panovska, Tara M. Sinclair
- “Symmetry and Separability in TwoCountry Cointegrated VAR Models: Representation and Testing” HansMartin Krolzig, Reinhold Heinlein
- “Detecting and Forecasting Large Deviations and Bubbles in a Near-Explosive Random Coefficient Model” Anurag Banerjee, Guillaume Chevillon, Marie Kratz
- “A Spatio-Temporal Mixture Model for Point Processes with Application to Ambulance Demand” David Matteson
- “Empirical Evidence on Inflation Expectations in the New Keynesian Phillips Curve” Sophocles Mavroeidis, Mikkel Plagborg-Moller, James H. Stock
- “A Non-Gaussian Asymmetric Volatility Model” Geert Bekaert, Eric Engstrom
- “Gaussian Term Structure Models and Bond Risk Premia” Bruno Feunou, Jean-Sebastien Fontain



## Chapter 39

# On the Wastefulness of (Pseudo-) Out-of-Sample Predictive Model Comparisons

Peter Hansen and Allan Timmermann have a fantastic new paper, “Equivalence Between Out-of-Sample Forecast Comparisons and Wald Statistics.”

The finite-sample wastefulness of (pseudo-) out-of-sample model comparisons seems obvious, as they effectively discard the (pseudo-) in-sample observations. That intuition should be true for both nested and non-nested comparisons, but it seems most obvious in the nested case: How could anything systematically dominate full-sample Wald, LR or LM for testing nested hypotheses? Hansen and Timmermann consider the nested case and verify the intuition with elegance and precision. In doing so they greatly clarify the misguided nature of most (pseudo-) out-of-sample model comparisons.

Consider the predictive regression model with  $h$ -period forecast horizon

$$y_t = \beta_1' X_{1,t-h} + \beta_2' X_{2,t-h} + \varepsilon_t,$$

$t = 1, \dots, n$ , where  $X_{1t} \in \mathbb{R}^k$  and  $X_{2t} \in \mathbb{R}^q$ . We obtain out-of-sample forecasts with recursively estimated parameter values by regressing  $y_s$  on  $X_{s-h} = (X'_{1,s-h}, X'_{2,s-h})'$  for  $s = 1, \dots, t$  (resulting in the least squares estimate  $\hat{\beta}_t =$

$(\hat{\beta}'_{1t}, \hat{\beta}'_{2t})'$  and using

$$\hat{y}_{t+h|t}(\hat{\beta}_t) = \hat{\beta}'_{1t}X_{1t} + \hat{\beta}'_{2t}X_{2t}$$

to forecast  $y_{t+h}$ .

Now consider a smaller (nested) regression model,

$$y_t = \delta'X_{1,t-h} + \eta_t.$$

In similar fashion we proceed by regressing  $y_s$  on  $X_{1,s-h}$  for  $s = 1, \dots, t$  (resulting in the least squares estimate  $\hat{\delta}_t$ ) and using

$$\tilde{y}_{t+h|t}(\hat{\delta}_t) = \hat{\delta}'_tX_{1t}$$

to forecast  $y_{t+h}$ .

In a representative and leading contribution to the (pseudo-) out-of-sample model comparison literature in the tradition of West (1996), McCracken (2007) suggests comparing such nested models via expected loss evaluated at population parameters. Under quadratic loss the null hypothesis is

$$H_0 : E[y_t - \hat{y}_{t|t-h}(\beta)]^2 = E[y_t - \tilde{y}_{t|t-h}(\delta)]^2.$$

McCracken considers the test statistic

$$T_n = \frac{\sum_{t=n_\rho+1}^n (y_t - \tilde{y}_{t|t-h}(\hat{\delta}_{t-h}))^2 - (y_t - \hat{y}_{t|t-h}(\hat{\beta}_{t-h}))^2}{\hat{\sigma}_\varepsilon^2},$$

where  $\hat{\sigma}_\varepsilon^2$  is a consistent estimator of  $\sigma_\varepsilon^2 = \text{var}(\varepsilon_{t+h})$  and  $n_\rho$  is the number of observations set aside for the initial estimation of  $\beta$ , taken to be a fraction  $\rho \in (0, 1)$  of the full sample,  $n$ , i.e.,  $n_\rho = \lfloor n\rho \rfloor$ . The asymptotic null distribution of  $T_n$  turns out to be rather complicated; McCracken shows that it is a convolution of  $q$  independent random variables, each with a distribution of  $2 \int_\rho^1 u^{-1} B(u) dB(u) - \int_\rho^1 u^{-2} B(u)^2 du$ .

Hansen and Timmermann show that  $T_n$  is just the difference between two Wald statistics of the hypothesis that  $\beta_2 = 0$ , the first based on the full sample and the second based on the initial estimation sample. That is,  $T_n$  is just the



increase in the Wald statistic obtained by using the full sample as opposed to the initial estimation sample. Hence the power of  $T_n$  derives entirely from the post-split sample, so it must be less powerful than using the entire sample. Indeed Hansen and Timmermann show that power decreases as  $\rho$  increases.

On the one hand, the Hansen-Timmermann results render trivial the calculation of  $T_n$  and greatly clarify its limit distribution (that of the difference between two independent  $\chi^2$ -distributions and their convolutions). So if one insists on doing  $T_n$ -type tests, then the Hansen-Timmermann results are good news. On the other hand, the real news is bad: the Hansen-Timmerman results make clear that, at least in the environments they consider, (pseudo-) out-of-sample model comparison comes at high cost (power reduction) and delivers no extra benefit.

[By the way, my paper, “Comparing Predictive Accuracy, Twenty Years Later: A Personal Perspective on the Use and Abuse of Diebold-Mariano Tests,” makes many related points. Drafts are here. The final (?) version will be delivered as the JBES Invited Lecture at the January 2014 ASSA meetings in Philadelphia. Commentary at the meeting will be by Andrew Patton and Allan Timmerman. The JBES published version will contain the Patton and Timmermann remarks, plus those of Atsushi Inoue, Lutz Kilian, and Jonathan Wright. Should be entertaining!]



## Chapter 40

# Federal Reserve Bank of Philadelphia Launches Improved U.S. GDP Growth Series

Exciting news for empirical macroeconomics and finance: The Federal Reserve Bank of Philadelphia today released a new and improved *GDP* growth series, *GDPplus*. It's an optimal blend of the BEA's expenditure-side and income-side estimates (call them  $GDP_E$  and  $GDP_I$ , respectively). The *GDPplus* web page contains extensive background information and will be updated whenever new or revised data for  $GDP_E$  and/or  $GDP_I$ , and hence *GDPplus*, are released.

*GDPplus* (developed in Aruoba, Diebold, Nalewaik, Schorfheide and Song, "Improving GDP Measurement: A Measurement-Error Perspective," NBER Working Paper 18954, 2013) is based on a dynamic-factor model,

$$\begin{pmatrix} GDP_{Et} \\ GDP_{It} \end{pmatrix} = \begin{pmatrix} 1 \\ 1 \end{pmatrix} GDP_t + \begin{pmatrix} \epsilon_{Et} \\ \epsilon_{It} \end{pmatrix}$$
$$GDP_t = \mu(1 - \rho) + \rho GDP_{t-1} + \epsilon_{Gt},$$

where  $GDP_E$  and  $GDP_I$  are noisy indicators of latent true  $GDP$ ,  $\epsilon_E$  and  $\epsilon_I$  are expenditure- and income-side stochastic measurement errors, and  $\epsilon_G$  is a stochastic shock to true  $GDP$ . The Kalman smoother provides an optimal estimate of  $GDP$  based on the noisy indicators  $GDP_E$  and  $GDP_I$ . That optimal estimate is  $GDPplus$ . Note that  $GDPplus$  is not just a period-by-period simple average, or even a weighted average, of  $GDP_E$  and  $GDP_I$ , because optimal signal extraction averages not only across the  $GDP_E$  and  $GDP_I$  series, but also over time.

The historical perspective on  $GDP$  provided by  $GDPplus$  complements the real-time perspective on the overall business cycle provided by the [Aruoba-Diebold-Scotti \(ADS\) Index](#), also published by the Federal Reserve Bank of Philadelphia.

Moving forward,  $GDPplus$  will be updated at 2 PM on every day that new and/or revised  $GDP_E$  and/or  $GDP_I$  data are released. The next update will be November 7, the day of BEA's NIPA release for Q3 (delayed due to the government shutdown).

# Chapter 41

## The e-Writing Jungle: LaTeX to pdf to the Web

LaTeX and MathML and MathJax and Python and Sphinx and IPython and R and Knitter and Firefox and Chrome and ...

My head is spinning with all this stuff. Maybe yours is too.

One thing is clear: The traditional academic book publishing paradigm (broadly defined) is cracking and will soon be crumbling. In the emerging e-paradigm there will be essentially no difference among books, courses, e-books, e-courses, web sites, blogs, and so on. With no loss of generality, then, let's just call it all "e-books," filled with text, color graphics, audio/video, animations, interactive learning tools, massive numbers of internal and external hyperlinks, etc.

An interesting question is how to create ("write"?) and distribute such e-books. The amazing thing is that the answer remains unclear. Both pitfalls and opportunities abound. Here are some thoughts.

Part 1: LaTeX to pdf to the Web

One obvious e-book creation and distribution route is traditional LaTeX, compiled to pdf and posted on the web. Effete insiders now sneer at that, viewing it as little more than posting page photos of an old-fashioned B&W paper book. I beg to differ. What's true is that most people still fail to use the e-capabilities of LaTeX, so of course their pdf product is little more than an

e-copy of an old paper book, but that's their fault. All of the above-mentioned e-desiderata are readily available in LaTeX/pdf/web; one just has to use them!

Moreover, LaTeX/pdf/web has at least two extra benefits relative to a website (say). First, trivially, the pdf is instantly printable on-demand as a beautiful traditional book, which is sometimes useful. Second, and more importantly, the linear beginning-to-end layout of a "book" – in contrast to the non-linear jumble of links that is that is a website – is pedagogically invaluable when done well. That is, good authors put things in a precise order for a reason, and readers benefit by reading in that order.

OK, you say, but how to restrict access only to those who pay for a LaTeX/pdf/web e-book? (It's true, a pdf web post is basically impossible to copy-protect.) My present view is very simple: Just get over it and forget the chump change. Scholarly monographs and texts are labors of love; the real compensation is satisfaction from helping to advance and spread knowledge. And if that's not quite enough, rest assured that if you write a great book you'll reap handsome monetary rewards in subtle but nevertheless very real ways, even if you post it gratis.

## Chapter 42

# The e-Writing Jungle Part 2: The MathML Impasse and the MathJax Solution

Back to LaTeX and MathJax and MathML and Python and Sphinx and IPython and R and Knitter and Firefox and Chrome and ...

In Part 1, I praised e-books done as LaTeX to pdf to the web, perhaps surprisingly. Now let's go the other way, to an e-book done natively on the web as HTML. Each approach is worth considering, depending on the application, as each has different costs and benefits.

### Part 2: The MathML Impasse and the MathJax Solution

All we want is an HTML version with native support and beautiful rendering of mathematics. That's what HTML5 does, except for a small detail: many browsers (IE, Chrome, ...) won't display HTML5. The real problem is MathML, which is embedded in HTML5, and which is the key to math fonts in HTML5 or anywhere else. It's not just a question of browser suppliers finally waking up and flipping on the MathML switch; rather, successful MathML integration turns out to be really hard (seriously, although I don't really know why), and there are also security issues (again seriously, and again I don't really know why). For those reasons, the good folks at Microsoft and Google, for example, have now basically decided that they'll never support MathML.

There's a lot of noise about all this swirling around right now – some of it quite bitter – but a single recent informative and entertaining piece will catapult you to the cutting edge, [“Google Subtracts MathML from Chrome, and Anger Multiplies,”](#) by Steven Shankland.

The bottom line: Math has now been officially sentenced to an eternity of second-class web citizenship, in the sense that native and broad math browser support is not going to happen. But that brings us to [MathJax, a JavaScript app that works with HTML](#). You simply type in LaTeX and MathJax finds any math expressions and renders them beautifully. Note well that MathJax is not just pasting graphics images; hence its output scales nicely and works well on mobile devices too. For all you need to know, check out [“MathML Forges On,”](#) by Peter Krautzberger.

So what's the big problem? Doesn't HTML plus MathJax basically equal HTML5, with the major additional benefit that it actually works? Of course it's somewhat insulting to us math folk, and certainly it's aesthetically unappealing, to have to overlay something on HTML just to get it to display math. (I'm reminded of the old days of PC hardware, with separate “math co-processors.”) And there are other issues. For example, MathJax loads from the cloud (unless it's on your machine(s), which requires installations and updates, and which can't be done for mobile devices), and the MathJax math rendering may take a few seconds or more, depending on the speed of your connection and the complexity/length of your math.

But are any of the above “problems” truly serious? I don't think so. On the contrary, MathJax strikes me as a versatile and long-overdue solution for web-based math. And its future looks very bright, with official supporters now ranging from the American Mathematical Society to Springer to Matlab. (Not that I'm a fan of Matlab any longer – please join the resistance, purge Matlab from your life, and replace it with Python and R – but that's a topic for another day.)



# Chapter 43

## The e-Writing Jungle Part 3: Web-Based e-books Using Python / Sphinx

In the previous Parts 1 and 2, I essentially dealt with two extremes: (1) LaTeX to pdf to web, and (2) raw HTML (however arrived at) with math rendered by MathJax. Now let's look at something of a middle ground: the Python package, Sphinx, for producing e-books.

### Part 3: Python / Sphinx

Parts 1 and 2 of [Quantitative Economics, by Stachurski and Sargent](#), are great routes into [Python](#) for economists. There's lots of good comparative discussion of Python vs. Matlab or Julia, the benefits of public-domain, open-source code, etc. And it's always up to the minute, because it's an on-line e-book! Just check it out.

Of course we're interested here in e-books, not Python per se. It turns out, however, that Stachurski and Sargent is also a cutting-edge example of a beautiful e-book. It's effectively written in Python using [Sphinx](#), which is a Python package that started as a vehicle for writing software manuals. But a manual is just a book, and one can fill a book with whatever one wants.

Sphinx is instantly downloadable, beautifully documented (the documentation is written in Sphinx, of course!), open source, and public domain (licensed

under BSD). [ReStructuredText](#) is the powerful markup language. (You can learn all you need in ten minutes, since math is the only complicated thing, and math stays in LaTeX, rendered either by JavaScript via MathJax or as png images, your choice.) In addition to publishing to HTML, you can publish to LaTeX or pdf.

Want to see how Sphinx performs with math even more dense than Stachurski and Sargent's? Just check, for example, the Sphinx book [Theoretical Physics Reference](#). Want to see how it performs with graphics even more slick than Stachurski and Sargent's? Just check the [Matplotlib Documentation](#). It's all done in Sphinx.

Sphinx is a totally class act. In my humble opinion, nothing else in its genre comes close.

## Chapter 44

# A New Center to Watch for Predictive Macroeconomic and Financial Modeling

Check out USC's fine new [Center for Applied Financial Economics](https://dornsifecms.usc.edu/conferences/caf2013/), led by the indefatigable Hashem Pesaran. The first event is a fascinating conference, "Recent Developments on Forecasting Techniques for Macro and Finance." There's lots of information at <https://dornsifecms.usc.edu/conferences/caf2013/>.

PROGRAM, Wednesday, November 20th, 2013

*Welcome*

Hashem Pesaran, John E. Elliott Distinguished Chair of Economics and Director of the Centre for Applied Financial Economics (CAFE), USC Dornsife

*9:00-9:50 a.m. SESSION I Chair: Robert Dekle*

Speaker: Óscar Jordá

Title: Semiparametric Estimates of Monetary Policy Effects: String Theory Revisited. With Joshua D. Angrist and Guido Kuersteiner.

Discussant: Eleonora Granziera

*9:50-10:40 a.m. SESSION II Chair: Yu-Wei Hsieh*

Speaker: Michael W. McCracken

Title: Evaluating Forecasts from Vector Autoregressions Conditional on Policy

Paths. With Todd E. Clark.

Discussant: Andreas Pick

*11:00-11:50 p.m. SESSION III Chair: Michael Magill*

Speaker: Jose A. Lopez

Title: A Probability-Based Stress Test of Federal Reserve Assets and Income.

With Jens H.E. Christensen and Glenn D. Rudebusch.

Discussant: Wayne Ferson

*11:50-12:40 p.m. SESSION IV Chair: Yilmaz Kocer*

Speaker: Tae-Hwy Lee

Title: Density and Risk Forecast of Financial Returns Using Decomposition and Maximum Entropy. With Zhou Xi and Ru Zhang.

Discussant: Hyungsik Roger Moon

*2:00-2:50 p.m. SESSION V Chair: Juan D. Carrillo*

Speaker: Allan Timmermann

Title: Equivalence Between Out-of-Sample Forecast Comparisons and Wald Statistics. With Peter Reinhard Hansen.

Discussant: Hashem Pesaran

*2:50-3:40 p.m. SESSION VI Chair: Jeffrey B. Nugent*

Speaker: Gloria Gonzalez-Rivera

Title: In-Sample and Out-of-Sample Performance of Autocontour-Testing in Unstable Environments. With Yingying Sun.

Discussant: Cheng Hsiao

*4:00-4:50 p.m. SESSION VII Chair: Giorgio Coricelli*

Speaker: Gareth M. James

Title: Functional Response Additive Model Estimation with Online Virtual Stock Markets. With Yingying Fan, Natasha Foutz, and Wolfgang Jank.

Discussant: Dalia A. Ghanem

*4:50-5:40 p.m. SESSION VIII Chair: Joel David*

Speaker: Marcelle Chauvet

Title: Nowcasting of Nominal GDP. With William A. Barnett and Danilo Leiva-Leon.

Discussant: Michael Bauer

## Chapter 45

# Collaboration Distance and the Math Genealogy Project

The [American Mathematical Society](#) has a fun site on “collaboration distance” between various mathematicians. The idea is simple: If, for example, I wrote with X, and X wrote with Z, then my collaboration distance to Z is two. There’s a good description at <http://www.qedcat.com/archive/98.html> here, and the actual calculator is at <http://www.ams.org/mathscinet/collaborationDistance.html>.

You can track your collaboration distance not only to Erdos (of course), but also to all-time giants like Gauss or Laplace. The calculator reveals, for example, that my collaboration distance to Gauss is just eight:

I co-authored with Marc Nerlove Marc Nerlove co-authored with Kenneth J. Arrow Kenneth J. Arrow co-authored with Theodore E. Harris Theodore E. Harris co-authored with Richard E. Bellman Richard E. Bellman co-authored with Ernst G. Straus Ernst G. Straus co-authored with Albert Einstein Albert Einstein co-authored with Hermann Minkowski Hermann Minkowski co-authored with Carl Friedrich Gauss.

Wow – and some great company along the way, quite apart from the origin at old Carl Friedrich!

Of course I understand the “small-world” network phenomenon, but it’s nevertheless hard not to be astounded at first.

So how truly astounding is my eight-step connection to Gauss? Let's do a back-of-the-envelope calculation. For a benchmark Erdos-Renyi network we have:

$$max \approx \frac{\ln N}{\ln \mu},$$

where  $max$  is the maximum collaboration distance,  $N$  is the number of authors in the network, and  $\mu$  is the mean number of co-authors. Suppose there are 1,000,000 authors ( $N = 1,000,000$ ), each with 5 co-authors (so, trivially,  $\mu = 5$ ). Then we have  $max \approx 9$ .

Hmmm...I'm no longer feeling so special.

Surely my calibration used too small an  $N$ , too large a  $\mu$ , or both. And of course the  $max$  result is only an approximation, and moreover, it's a valid approximation only for simplistic Erdos-Renyi networks, and moreover, ...

## Chapter 46

# Comparing Predictive Accuracy, Twenty Years Later

I have now posted the [final pre-meeting draft of the “Use and Abuse” paper](#) (well, more-or-less “final”).

I’ll present it as the JBES Lecture, [January 2014 ASSA meetings, Philadelphia](#). Please join if you’re around. It’s Friday January 3, 2:30, Pennsylvania Convention Center Room 2004-C (I think).

By the way, the 2010 [Peter Hansen](#) paper that I now cite in my final paragraph, “A Winners Curse for Econometric Models: On the Joint Distribution of In-Sample Fit and Out-of-Sample Fit and its Implications for Model Selection,” is tremendously insightful. I saw Peter present it a few years ago at a Stanford summer workshop, but I didn’t fully appreciate it and had forgotten about it until he reminded me when he visited Penn last week. He’s withheld the 2010 and later revisions from general circulation evidently because one section still needs work. Let’s hope that he gets it revised and posted soon! ([A more preliminary 2009 version remains online from a University of Chicago seminar.](#)) One of Peter’s key points is that although split-sample model comparisons can be “tricked” by data mining in finite samples, just as can all model comparison procedures, split-sample comparisons appear to be harder to trick, in a sense that he makes precise. That’s potentially a very big deal.

[Comparing Predictive Accuracy, Twenty Years Later: A Personal Perspec-](#)

### tive on the Use and Abuse of Diebold-Mariano Tests

Abstract: The Diebold-Mariano (DM) test was intended for comparing forecasts; it has been, and remains, useful in that regard. The DM test was not intended for comparing models. Much of the large ensuing literature, however, uses DM-type tests for comparing models, in (pseudo-) out-of-sample environments. In that case, simpler yet more compelling full-sample model comparison procedures exist; they have been, and should continue to be, widely used. The hunch that (pseudo-) out-of-sample analysis is somehow the “only,” or “best,” or even necessarily a “good” way to provide insurance against in-sample over-fitting in model comparisons proves largely false. On the other hand, (pseudo-) out-of-sample analysis remains useful for certain tasks, most notably for providing information about comparative predictive performance during particular historical episodes.



## Chapter 47

# FRB St. Louis is Far Ahead of the Data Pack

The email below arrived recently from the Federal Reserve Bank of St. Louis. It reminds me of something that's hardly a secret, but that nevertheless merits applause, namely that [FRBSL's Research Department](#) is a wonderful source of economic and financial data provision (FRED and much more...), and related information provision broadly defined (RePEc and much more...).

FRED, ALFRED, GeoFRED, RePEc, FRASER, etc. – wow! FRBSL supplies not only the data, but also intuitive and seamless delivery interfaces. They're very much on the cutting edge, constantly innovating and leading.

Other Feds of course supply some great data as well. To take just one example close to home, the [Real-Time Data Research Center within FRB Philadelphia's Research Department](#) maintains a widely-respected [Real-Time Dataset](#) and [Survey of Professional Forecasters](#) (and of course my favorites, the [ADS Index](#) and [GDPplus](#)).

But FRBSL is in a league of its own. Maybe there's been an implicit decision within the System that FRBSL will be the de facto data guru? Or maybe it's just me, not looking around thoroughly enough? I suspect it's a bit of both.

In any event I applaud FRBSL for a job marvelously well done.



## Chapter 48

### Holiday Haze

Your dedicated blogger is about to vanish in the holiday haze, presumably stumbling back sometime early in the new year.

Random thought: Obviously I guessed that I'd enjoy blogging or I wouldn't have started, but I had no idea how truly satisfying it would be, or, for that matter, that anyone would actually read me! Many thanks my friends. I look forward to returning soon. Meanwhile, all best wishes for the holidays.

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