THE RISKS OF FINANCIAL MODELING: VAR AND THE ECONOMIC MELTDOWN

HEARING

BEFORE THE

SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT

COMMITTEE ON SCIENCE AND TECHNOLOGY

HOUSE OF REPRESENTATIVES

ONE HUNDRED ELEVENTH CONGRESS

FIRST SESSION

SEPTEMBER 10, 2009

Serial No. 111–48

Printed for the use of the Committee on Science and Technology

Available via the World Wide Web: http://www.science.house.gov

U.S. GOVERNMENT PRINTING OFFICE

51–925PDF WASHINGTON : 2010

For sale by the Superintendent of Documents, U.S. Government Printing Office
Internet: bookstore.gpo.gov Phone: toll free (866) 512–1800; DC area (202) 512–1800
Fax: (202) 512–2104 Mail: Stop IDCC, Washington, DC 20402–0001

COMMITTEE ON SCIENCE AND TECHNOLOGY

HON. BART GORDON, Tennessee, *Chair*

JERRY F. COSTELLO, Illinois
EDDIE BERNICE JOHNSON, Texas
LYNN C. WOOLSEY, California
DAVID WU, Oregon
BRIAN BAIRD, Washington
BRAD MILLER, North Carolina
DANIEL LIPINSKI, Illinois
GABRIELLE GIFFORDS, Arizona
DONNA F. EDWARDS, Maryland
MARCIA L. FUDGE, Ohio
BEN R. LUJÁN, New Mexico
PAUL D. TONKO, New York
PARKER GRIFFITH, Alabama
STEVEN R. ROTHMAN, New Jersey
JIM MATHESON, Utah
LINCOLN DAVIS, Tennessee
BEN CHANDLER, Kentucky
RUSS CARNAHAN, Missouri
BARON P. HILL, Indiana
HARRY E. MITCHELL, Arizona
CHARLES A. WILSON, Ohio
KATHLEEN DAHLKEMPER, Pennsylvania
ALAN GRAYSON, Florida
SUZANNE M. KOSMAS, Florida
GARY C. PETERS, Michigan
VACANCY

RALPH M. HALL, Texas
F. JAMES SENSENBRENNER JR., Wisconsin
LAMAR S. SMITH, Texas
DANA ROHRABACHER, California
ROSCOE G. BARTLETT, Maryland
VERNON J. EHLERS, Michigan
FRANK D. LUCAS, Oklahoma
JUDY BIGGERT, Illinois
W. TODD AKIN, Missouri
RANDY NEUGEBAUER, Texas
BOB INGLIS, South Carolina
MICHAEL T. MCCAUL, Texas
MARIO DIAZ-BALART, Florida
BRIAN P. BILBRAY, California
ADRIAN SMITH, Nebraska
PAUL C. BROUN, Georgia
PETE OLSON, Texas

SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT

HON. BRAD MILLER, North Carolina, *Chair*

STEVEN R. ROTHMAN, New Jersey
LINCOLN DAVIS, Tennessee
CHARLES A. WILSON, Ohio
KATHY DAHLKEMPER, Pennsylvania
ALAN GRAYSON, Florida
BART GORDON, Tennessee

PAUL C. BROUN, Georgia
BRIAN P. BILBRAY, California
VACANCY
RALPH M. HALL, Texas

DAN PEARSON *Subcommittee Staff Director*
EDITH HOLLEMAN *Subcommittee Counsel*
JAMES PAUL *Democratic Professional Staff Member*
DOUGLAS S. PASTERNAK *Democratic Professional Staff Member*
KEN JACOBSON *Democratic Professional Staff Member*
TOM HAMMOND *Republican Professional Staff Member*
MOLLY O'ROURKE *Research Assistant*
ALEX MATTHEWS *Research Assistant*

CONTENTS

September 10, 2009

THE RISKS OF FINANCIAL MODELING: VAR AND THE ECONOMIC MELTDOWN

THURSDAY, SEPTEMBER 10, 2009

HOUSE OF REPRESENTATIVES,
SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
Washington, DC.

The Subcommittee met, pursuant to call, at 10:04 a.m., in Room 2318 of the Rayburn House Office Building, Hon. Brad Miller [Chairman of the Subcommittee] presiding.

BART GORDON, TENNESSEE
CHAIRMAN

RALPH M. HALL, TEXAS
RANKING MEMBER

U.S. HOUSE OF REPRESENTATIVES

COMMITTEE ON SCIENCE AND TECHNOLOGY

SUITE 2320 RAYBURN HOUSE OFFICE BUILDING
WASHINGTON, DC 20515–6301
(202) 225–6375
TTY: (202) 226–4410
http://science.house.gov

Subcommittee on Investigations and Oversight

Hearing on

The Risks of Financial Modeling:
VaR and the Economic Meltdown

Thursday, September 10, 2009
10:00 a.m. – 1:00 p.m.
2318 Rayburn House Office Building

Witness List

PANEL I

Dr. Nassim Nicholas Taleb
Distinguished Professor of Risk Engineering
Polytechnic Institute of New York University

Dr. Richard Bookstaber
Financial Author

PANEL II

Dr. Gregg Berman
Head of Risk Business
RiskMetrics Group

Mr. James G. Rickards
Senior Managing Director
Omnis Inc.

Mr. Christopher Whalen
Managing Director
Institutional Risk Analytics

Dr. David Colander
Christian A. Johnson Distinguished Professor of Economics
Middlebury College

HEARING CHARTER

SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT COMMITTEE ON SCIENCE AND TECHNOLOGY U.S. HOUSE OF REPRESENTATIVES

The Risks of Financial Modeling: VaR and the Economic Meltdown

THURSDAY, SEPTEMBER 10, 2009
10:00 A.M.–1:00 P.M.
2318 RAYBURN HOUSE OFFICE BUILDING

Purpose

The Subcommittee on Investigations and Oversight on Sept. 10, 2009 convenes the first Congressional hearing to examine the role of risk modeling in the global financial meltdown. Risk models, and specifically a method of risk measurement known as Value-at-Risk, or VaR, are widely viewed as an important factor in the extreme risk-taking that financial institutions engaged in leading to last year's economic upheaval. That risk-taking has led to hundreds of billions of dollars in losses to financial firms, and to a global recession with trillions of dollars in direct and indirect costs imposed on U.S. taxpayers and working families.

Given the central role of credit in the economy, the ability of major financial institutions to operate without assuming undue risks that gamble with the stability of the financial system, thereby endangering the broader economy, is of the utmost importance to both business and the public at large. The recent behavior by financial firms that are deemed "too big to fail" suggests that the financial system as currently structured and regulated creates a "moral hazard" because firms can expect that they will be bailed out if their risk-taking fails to pay off. This is exactly what happened in the United States in October of 2008 with great consequences to the taxpayers, who have been called upon to shoulder much of the huge burden arising from financial firms' underestimation of risk, poor judgment, and profligate behavior. Relied on to guide the decisions of both financial firms and federal regulators responsible for monitoring their soundness by ensuring that they have sufficient capital, the VaR, whether it was misused or not, was involved in inducing or allowing this situation to arise.

Given this dual function, it is critical that the Subcommittee examine: the role of the VaR and related risk-measurement methods in the current world financial crisis; the strengths and weaknesses of, and the limits to, the usefulness of the VaR; the degree to which the VaR is understood, and may be manipulated, within the institutions where it is in use; and the capabilities and needs of federal supervisors who may be called upon to work with the VaR in carrying out their regulatory duties. From a policy perspective, the most important question is how regulators will use VaR numbers produced by firms and whether it is an appropriate guide to setting capital reserve requirements.

This is the second in a series of hearings on how economic thinking and methods have been used by policy-makers both inside and outside of government.

The VaR's Origins and Use

Risk assessment models in the financial industry are the product of advances in economic and statistical methods developed in the social sciences over the last fifty years. J.P. Morgan adopted these techniques in developing the VaR in the 1980s as a tool to measure the risk of loss to its traders' portfolios. The VaR could produce a single number rating a trader's (or, in aggregate, the firm's cumulative) risk of loss of portfolio value over a specific period of time at a given level of confidence. The VaR provided managers a tool that appeared to allow them to keep a handle on the risks they were taking as financial instruments became more varied and complex and as assets became more difficult to value. Morgan decided to give the methodology of the VaR away, forming the now-independent RiskMetrics Group;

this resulted in the VaR rapidly becoming "so popular that it was considered the risk-model gold standard."[1]

To put it very simply, the VaR captures the probability of outcomes distributed along a curve-most commonly a "bell" or normal distribution. It provides an answer to the question of, "what is likely to happen tomorrow to the value of an asset?" by drawing from historical performance data. The highest probability of tomorrow's value is that it will be the same as today's value; the next highest probability is for a very small movement in value up or down, and so on. The more radical the movement in value, the lower the probability of that occurring. A manager may ask for a projection of the potential loss of an asset or portfolio at the 95 percent or even the 99 percent confidence level. At those levels, a complete loss of value is unlikely. The complete collapse of an asset or portfolio's value is not a 1-in-100 event; such a collapse is more likely a 1-in-500 or 1-in-10,000 or event. The VaR is unlikely to warn, then, of great shifts in value. The danger to the financial firm or the community comes at the extreme margins of the distribution curves produced by the VaR. As a map to day-to-day behavior, the VaR is probably pretty accurate for normal times, but for asset bubbles or other "non-normal" market conditions, the VaR is likely to misrepresent risks and dangers.

While the VaR was originally designed for financial institutions' use in-house, it has subsequently been given a key role in determining capital requirements for large banks under a major multilateral agreement, the Basel II Accord, published in 2004. That same year, the U.S. Securities and Exchange Commission adopted a capital regime applying Basel II standards to the Nation's largest investment banks,[2] a move that has been viewed as playing a role in those institutions' subsequent over-leveraging and liquidity problems. Those financial institutions assured regulators that the VaR was a way to see the level of risk they were taking on and a low VaR justified lower reserve requirements. (The terms of Basel II are currently being re-evaluated in light of the global economic crisis.)

Along with extensive use, the VaR has come in for extensive criticism. Although its merits were debated at least as far back as 1997,[3] criticism of the VaR has mounted in the wake of last year's collapse of such major financial institutions as Bear Stearns and Lehman Brothers. Among the allegations: that the VaR is inadequate in capturing risks of extreme magnitude but low probability, to which an institution may be left vulnerable; that this shortcoming may open it to manipulation by traders taking positions that seem profitable but whose risks they know the VaR is unlikely to pick up, and that such "gaming" can increase extreme risk; and that use of the VaR, derided for "quantify[ing] the immeasurable with great precision,"[4] promotes an unfounded sense of security within financial institutions creating an environment where firms take on more risk than they would without the security-blanket of a VaR number.

Those who advocate for the VaR argue that any misuse of the model is not the model's fault and that it remains a useful management tool. VaR defenders' argue that its purpose is "not to describe the worst possible outcomes;"[5] that it is essential to the ability of a financial institution to arrive at an estimate of its overall risk; and that in "computing their VAR[, institutions] are forced to confront their exposure to financial risks and to set up a proper risk management function," so that "the process of getting to VAR may be as important as the number itself."[6] Some

[1] "Risk Management," by Joe Nocera, *New York Times,* Jan. 4, 2009. J.P. Morgan was not the only firm to look for statistical tools to measure the risks of their portfolios, however Morgan's model became the most widely used. The model can be tweaked in many, many ways to meet the specific needs of a particular firm.

[2] "Alternative Net Capital Requirements for Broker-Dealers That are Part of Consolidated Supervised Entities; Supervised Investment Bank Holding Companies; Final Rules," Securities and Exchange Commission, June 21, 2004, 69 FR 34428–72. (According to Aswath Damodaran, Professor of Finance at the NYU Stern School of Business, "The first regulatory measures that evoke Value-at-Risk, though, were initiated in 1980, when the SEC tied the capital requirements of financial service firms to the losses that would be incurred, with 95 percent confidence over a thirty-day interval, in different security classes; historical returns were used to compute these potential losses. Although the measures were described as haircuts and not as Value or Capital at Risk, it was clear the SEC was requiring financial service firms to embark on the process of estimating one month 95 percent VaRs and hold enough capital to cover the potential losses." Damodaran, "Value-at-Risk (VAR)," found at *http://pages.stern.nyu.edu/?adamodar/pdfiles/papers/VAR.pdf*)

[3] "The Jorion-Taleb Debate," *DerivativesStrategy.com,* April 1997, *http://www.derivativesstrategy.com/magazine/archive/1997/0497fea2.asp*

[4] "Against VAR," by Nassim Taleb, in "The Jorion-Taleb Debate," *ibid.*

[5] "In Defense of VAR," by Philippe Jorion, in "The Jorion-Taleb Debate," *ibid.*

[6] Jorion, *idem.*

also argue that the VaR remains a useful tool for regulators to use as a baseline for establishing reserve requirements for "normal" times.

Witnesses

Panel I

Dr. Nassim Nicholas Taleb, Distinguished Professor of Risk Engineering, Polytechnic Institute of New York University.

Dr. Richard Bookstaber, Financial Author

Panel II

Dr. Gregg Berman, Head of Risk Business, RiskMetrics Group

Mr. James G. Rickards, Senior Managing Director, Omnis Inc.

Mr. Christopher Whalen, Managing Director, Institutional Risk Analytics

Dr. David Colander, Christian A. Johnson Distinguished Professor of Economics, Middlebury College

Chairman MILLER. Good morning, and welcome to today's hearing: *"The Risks of Financial Modeling: VaR and the Economic Meltdown."*

Economics has not been known in the past for mathematical precision. Harry Truman said he wanted a one-handed economist because he was frustrated with economists who equivocated by saying on the one hand, on the other hand. George Bernard Shaw said that if all the world's economists were laid end to end, they still wouldn't reach a conclusion. And apparently no one is sure who first observed that economics was the only field in which it was possible for two people to share a Nobel Prize for reaching exactly the opposite conclusion about the same question.

In the last 15 or 20 years, math and physics Ph.D.s from academia and the laboratory have entered the financial sector. Quantitative analysts, or 'quants,' directed their mathematical and statistical skills to financial forecasts at a time when global financial markets were becoming more interdependent than ever before.

The quants conceived such financial instruments as collaterized debt obligations, or CDOs, and credit default swaps, or CDSs, that would never have existed without them and their computers. They developed strategies for trading those instruments even in the absence of any underlying security or any real market; for that matter, in the absence of anything at all. They constructed risk models that convinced their less scientifically and technologically adept bosses that their instruments and strategies were infallibly safe. And their bosses spread the faith in the quants' models to regulators, who agreed to apply them to establish capital reserve requirements that were supposed to guarantee the soundness of financial institutions against adverse events. It almost seemed like the economic models had brought the precision of the laws of physics, the same kind of certainty about the movement of the planets, to financial risk management. Engineering schools even offered courses in "financial engineering."

The supposedly immutable laws underlying the quants' models, however, didn't work out, and the complex models turned out to have hidden risks rather than protecting against them, all at a terrible cost. Those risks, concealed and maybe even encouraged by the models, have led to hundreds of billions of dollars in losses to investors and taxpayers, to a global recession imposing trillions of dollars in losses to the world economy and immeasurable monetary and human costs. People around the world are losing their homes, their jobs, their dignity and their hope.

Taxpayers here and around the world are shouldering the burden arising from financial firms' miscalculation of risk, poor judgment, excessive bonuses and general profligate behavior. It is for this reason that the Subcommittee is directing our attention today to the intersection of quantitative analysis, economics and regulation. The Value-at-Risk model, or VaR, stands squarely at the intersection of quantitative analysis, economics and regulation. It is the most prominent risk model used by major financial institutions. The VaR is designed to provide an answer to the question, "What is the potential loss that could be faced within a limited, specified time to the value of an asset?"

The highest probability is that tomorrow's value will be the same as today's. The next highest probability is that there will be a small movement in value up or down, and so on. The more radical the movement in value, the lower the probability that it will happen. In other words, the danger to a financial firm or the community comes at the extreme margins of the VaR distribution curve, in the tails of the distribution. As a map to day-to-day behavior, the VaR is probably pretty accurate for normal times, just as teams favored by odds makers usually win. But just as long shots sometimes come home, just as underdogs do sometimes win, asset bubbles or other non-normal market conditions also occur, and the VaR is unlikely to capture the risks and dangers. The VaR also cannot tell you when you have moved into non-normal market conditions.

While the VaR was originally designed for financial institutions' in-house use to evaluate short-term risk in their trading books, it has been given a key role in determining capital requirements for large banks under a major multilateral agreement, the Basel II Accord, published in 2004. That same year, the U.S. Securities and Exchange Commission, the SEC, at the instigation of the five largest investment banks, adopted a capital reserve regime, applying Basel II standards to the Nation's largest investment banks—a decision that opened the door to their over-leveraging and liquidity problems. Three of the institutions that asked the SEC for this change in rules—Bear Stearns, Merrill Lynch, Lehman Brothers—no longer exist. At the time, those financial institutions assured regulators that the VaR would reflect the level of risk they were taking on, and that a low VaR justified lower capital requirements. The result was exactly what the investment banks asked for: lower capital requirements that allowed them to invest in even more risky financial instruments all justified with risk models that assured regulators that there was nothing to worry about. What could possibly go wrong?

In light of the VaR's prominent role in the financial crisis, this subcommittee is examining that role and the role of related risk-measurement methods. From a policy perspective, the most important immediate question is how regulators use VaR numbers and other such models designed by regulated institutions, and whether they are an appropriate guide to setting capital reserve requirements. But, beyond that, we must also ask whether the scientific and technical capabilities that led us into the current crisis should be applied to prevent future catastrophic events. Can mathematics, statistics and economics produce longer-range models, more reliable models, that could give us early warning when our financial system is headed for trouble? Or are such models inevitably going to be abused to hide risk-taking and encourage gambling by firms whose failures can throw the whole world into a recession, as they have in the last couple of years? If models cannot be a useful guide for regulation, should we just abandon the approach, or simply increase reserves, which will reduce profits and perhaps reduce some useful economic conduct in the short run, but protect taxpayers and the world economy in the long run?

Those are big questions, but the stakes for taxpayers and investors and the world economy justify some effort to get at some answers.

I now recognize Dr. Broun for his opening statement.
[The prepared statement of Chairman Miller follows:]

PREPARED STATEMENT OF CHAIRMAN BRAD MILLER

Economics has not been known in the past for mathematical precision. Harry Truman said he wanted a one-handed economist because he was frustrated with economists who equivocated by saying "on the one hand . . . on the other hand." George Bernard Shaw said that if all the world's economists were laid end to end, they still wouldn't reach a conclusion. And apparently no one knows who first observed that economics was the only field in which two people can share a Nobel Prize for reaching exactly the opposite conclusion.

But in the last 15 or 20 years, math and physics Ph.D.s from academia and the laboratory have entered the financial sector. Quantitative analysts, or "quants," directed their mathematical and statistical skills to financial forecasts at a time when global financial markets were becoming more interdependent than ever before.

The quants conceived such financial instruments as collaterized debt obligations, or "CDOs," and credit default swaps, or "CDSs," that would never have existed without them and their computers. They developed strategies for trading those instruments even in the absence of any underlying security or any real market. They constructed risk models that convinced their less scientifically and technologically adept bosses that their instruments and strategies were infallibly safe. And their bosses spread faith in the quants' models to regulators, who agreed to apply them to establish capital reserve requirements that were supposed to guarantee the soundness of financial institutions against adverse events. It almost seemed like economic models had brought the precision of the laws of physics to financial risk management. Engineering schools even offered courses in "financial engineering."

The supposedly immutable "laws" underlying the quants' models didn't work, and the complex models turn out to have hidden risks rather than protected against them, all at a terrible cost. Those risks—concealed and maybe even encouraged by the models—have led to hundreds of billions of dollars in losses to investors and the taxpayers, to a global recession imposing trillions of dollars in losses to the world economy and immeasurable monetary and human costs. People around the world are losing their jobs, their homes, their dignity and their hope.

Taxpayers here and around the world are shouldering the burden arising from financial firms' miscalculation of risk, poor judgment, excessive bonuses and profligate behavior. It is for this reason that the Subcommittee has chosen to direct its attention today to that intersection of quantitative analysis, economics, and regulation. The "Value-at-Risk" model, or "VaR" stands squarely at the center of this intersection as the most prominent risk model used by major financial institutions. The VaR is designed to provide an answer to the question, "What is the potential loss that could be faced within a limited, specified time to the value of an asset?"

The highest probability is that tomorrow's value will be the same as today's; the next highest probability is of a very small movement in value up or down, and so on. The more radical the movement in value, the lower the probability of its occurrence. In other words, the danger to the financial firm or the community comes at the extreme margins of the VaR distribution curve, in the "tails" of the distribution. As a map to day-to-day behavior, the VaR is probably pretty accurate for normal times, just as teams favored by odds makers usually win. But just as long shots sometimes come home, asset bubbles or other "non-normal" market conditions also occur, and the VaR is unlikely to capture the risks and dangers. The VaR also cannot tell you when you have moved into "non-normal" market conditions.

While the VaR was originally designed for financial institutions' to use in-house to evaluate short-term risk in their trading books, it was given a key role in determining capital requirements for large banks under a major multilateral agreement, the Basel II Accord, published in 2004. That same year, the U.S. Securities and Exchange Commission, at the instigation of the five largest investment banks, adopted a capital reserve regime applying Basel II standards to the Nation's largest investment banks, a decision that opened the door to their over-leveraging and liquidity problems. Three of the institutions that asked the SEC for this change in rules—Bear Stearns, Merrill Lynch, Lehman Brothers—no longer exist. At the time, those financial institutions assured regulators that the VaR would reflect the level of risk they were taking on, and that a low VaR justified lower reserve requirements. The result was exactly what the investment banks asked for; lower capital reserve requirements that allowed them to invest in even more risky financial instruments all justified with risk models that assured regulators that there was nothing to worry about.

In light of the VaR's prominent role in the financial crisis, this Subcommittee is examining that role and the role of related risk-measurement methods. From a policy perspective, the most important immediate question is how regulators use VaR numbers and other such models devised by regulated institutions and whether they are an appropriate guide to setting capital reserve requirements. But, beyond that, we must also ask whether the scientific and technical capabilities that helped lead us into the current crisis should be applied to prevent future catastrophic events. Can mathematics, statistics, and economics produce longer-range models—models that could give us early warning of when our complex financial system is heading for trouble? Or are such models inevitably going to be abused to hide risk-taking and encourage excessive gambling by firms whose failures can throw the whole world into a recession? If models cannot be a useful guide for regulation, should we just abandon this approach and simply increase reserves, reducing profits and perhaps some useful economic conduct in the short run, but protecting taxpayers and the world economy in the long run?

These are big questions, but the stakes for taxpayers and investors and the world economy justify the effort to get at some answers.

I now recognize Mr. Broun for his opening statement.

Mr. BROUN. Thank you, Mr. Chairman. Let me welcome the witnesses here today and thank them for appearing. Today's hearing on financial modeling continues this committee's work on the role of science in finance and economics.

As I pointed out in our previous hearing in May, for the last several years Wall Street has increasingly leveraged mathematics, physics and science to better inform their decisions. Even before Value-at-Risk was developed to characterize risk, bankers and economists were looking for a silver bullet to help them to beat the market.

Despite the pursuit of a scientific panacea for financial decisions, models are simply tools employed by decision-makers and risk managers. They add another layer of insight but are not crystal balls. Leveraging a position too heavily or assuming future solvency based on modeling data alone is hazardous, to say the least. Conversely, it stands to reason that if we could accurately predict markets, then both losses and profits would be limited since there would be very little risk involved.

Modeling is a subject this committee has addressed several times in the past, whether it is in regard to climate change, chemical exposures, pandemics, determining spacecraft survivability or attempting to value complex financial instruments. Models are only as good as the data and assumptions that go into them. Ultimately decisions have to be made based on a number of variables which should include scientific models but certainly not exclusively. As witnesses in our previous hearing stated, "Science describes, it does not prescribe." No model will ever relieve a banker, trader or risk manager of the responsibility to make difficult decisions and hedge inevitable uncertainly.

This committee struggles enough with the complexities of modeling, risk assessment and risk management regarding physical sciences. Attempting to adapt those concepts to economics and finance is even more complex. Appreciating this complexity and understanding the limitations and intended purpose of financial models is just as important as what the models tell you.

We have two esteemed panels of witnesses here today who will discuss appropriate roles and limitations of models such as VaR. They will explain how these models are used and shed some light on what role they may have played in the recent economic crisis.

I look forward to you all's testimony and I yield back my time. Thank you, Mr. Chairman.

[The prepared statement of Mr. Broun follows:]

PREPARED STATEMENT OF REPRESENTATIVE PAUL C. BROUN

Thank you Mr. Chairman.

Let me welcome the witnesses here today and thank them for appearing.

Today's hearing on Financial Modeling continues this committee's work on the role of science in finance and economics.

As I pointed out at our previous hearing in May, over the last 30 years Wall Street has increasingly leveraged mathematics, physics, and science to better inform their decisions. Even before Value-at-Risk (VaR) was developed to characterize risk, bankers and economists were looking for a silver bullet to help them beat the market.

Despite the pursuit of a scientific panacea for financial decisions, models are simply tools employed by decision-makers and risk managers. They add another layer of insight, but are not crystal balls. Leveraging a position too heavily or assuming future solvency based on modeling data alone is hazardous to say the least. Conversely, it stands to reason that if we could accurately predict markets, then both losses and profits would be limited since there would be very little risk involved.

Modeling is a theme this committee has addressed several times in the past. Whether it is in regard to climate change, chemical exposures, pandemics, determining spacecraft survivability, or attempting to value complex financial instruments, models are only as good as the data and assumptions that go into them. Ultimately, decisions have to be made based on a number of variables which should include scientific models, but certainly not exclusively. As a witness at a previous hearing stated, "science describes, it does not prescribe." No model will ever relieve a banker, trader, or risk manager of the responsibility to make difficult decisions and hedge for inevitable uncertainty.

This committee struggles enough with the complexities of modeling, risk assessment, and risk management regarding physical sciences. Attempting to adapt those concepts to economics and finance is even more complex. Appreciating this complexity, and understanding the limitations and intended purpose of financial models is just as important as what the models tell you.

We have two esteemed panels of witnesses here today who will discuss the appropriate roles and limitations of models such as VaR. They will explain how these models are used and shed some light on what role they may have played in the recent economic crisis. I look forward to their testimony and yield back my time.

Thank you.

Chairman MILLER. Thank you, Dr. Broun.

I now ask unanimous consent that all additional opening statements submitted by Members be included in the record. Without objection, that is so ordered.

Panel I:

We do have an outstanding group of witnesses today. I know that Chairmen at hearings always say that but it is certainly true. This time I mean it. On our first panel, we have two very well known and respected authors whose books and other writings warned against many of the practices of the financial industry that resulted in the current economic meltdown. Both of them have years of experience on Wall Street. Dr. Nassim Taleb is the author of *"Fooled by Randomness"* and *"The Black Swan."* After a career as a trader and fund manager, Dr. Taleb is now the Distinguished Professor of Risk Engineering at the Polytechnic Institute of New York University. And if you are one of that slice of the American population for whom Bloomberg and CNBC are your favorite TV channels, Dr. Taleb is a rock star. Dr. Taleb is joined by another rock star, Dr. Richard Bookstaber, who is the author of *"A Demon* *of Our Own Design: Markets, Hedge Funds and the Risk of Financial Innovation."* Dr. Bookstaber has worked as a risk manager for Salomon Brothers, Morgan Stanley and Moore Capital Management. He also runs equity funds and he began on Wall Street designing derivative instruments. Does your mother know about that?

As our witnesses should know, you will each have five minutes for your spoken testimony. Your written testimony will be included in the record for the hearing. When you all have completed your spoken testimony, we will begin with questions and each Member will have five minutes to question the panel. It is the practice of this subcommittee—it is an investigative and oversight subcommittee—to receive testimony under oath. As I pointed out to the panelists at our last hearing on economic issues, to prosecute a case for perjury, the prosecutor, the U.S. attorney would have to prove what the truth was, that you knew the truth and that you consciously departed from it. I think you can sleep easily without worrying about a prosecution for perjury, but we will ask you to take an oath. Do either of you have any objection to taking an oath? Okay. You also have the right to be represented by counsel. Do either of you have counsel here? If you would, please stand and raise your right hand. Do you swear to tell the truth and nothing but the truth?

The record will reflect that both witnesses did take the oath. We will begin with Dr. Taleb. Dr. Taleb.

STATEMENT OF DR. NASSIM N. TALEB, DISTINGUISHED PROFESSOR OF RISK ENGINEERING, POLYTECHNIC INSTITUTE OF NEW YORK UNIVERSITY; PRINCIPAL, UNIVERSA INVESTMENTS L.P.

Dr. TALEB. Mr. Chairman, Ranking Member, Members of the Committee, thank you for giving me this opportunity to testify on the risk measurement methods used by banks, particularly those concerned with the risks of VaR events. You know, Value-at-Risk is just a method. It is a very general method, not very precise method, that measures the risks of VaR events. For example, a standard daily Value-at-Risk tells you that if your VaR is a million, daily VaR is a million, you have—it is at one percent probability, that you have less than one percent chance of losing a million or more on a given day. There are of course a lot of variations around VaR. For me, they are equally defective.

Thirteen years ago, I wrote that the VaR encourages misdirected people to take risks with shareholders' and ultimately taxpayers' money—that is, regular people's money. I have been since begging for suspension of these measurements of tail risks. We just don't understand tail events. And lot of people say, oh, let's measure risks. My idea is very different. Let's find what risks we can measure and any other risks we should be taking instead of doing it the opposite way. We take a lot of risks and then we try to find some scientists who can confirm these methods, you know, that these risks we can measure and that these methods are sound.

I have been begging, and actually I wrote that I would be on the witness stand 13 years ago, and today I am here. The banking system lost so far more than $4.3 trillion, according to the International Monetary Fund—that is more than they ever made in the history of banking—on tail risks, measurements of rare events. Most of the losses of course were in the United States, and I am not counting the economic consequences. But this shouldn't have happened. Data shows that banks routinely lose everything they made over a long period of time in one single blow-up. It happened in 1982 because of multi-center banks losing everything made in the history of multi-center banking, one single event, loans to Latin America. The same thing in variation happened in 1991, and of course now. And every time society bails them out. Bank risk takers retain their bonuses and say oh, one fluke, all right, and we start again. This is an aberrant case of capitalism for the profit, and socialism for the losses.

So I have five points associated with VaR that I will go over very quickly, and I will give my conclusion. Number one: these problems were obvious all along. This should not have happened. We knew about the defects of the VaR when it was introduced. A lot of traders, a lot of my friends, everyone—I am not the only person ranting against VaR. A lot of people were ranting against it before. Nobody heard us. Regulators did not listen to anyone who knew what was going on, is my point number one.

Point number two: VaR is ineffective. I guess I don't need more evidence than the recent events to convince you.

Point number three, and that to me is crucial. You have a graph that shows you the performance profile of someone making steady earnings for a long time and then losing back everything. You can see from that graph, figure one on page four, that this is a strategy that is pretty much pursued by the majority of people on Wall Street, by banks. They make steady income for a long time, and when they blow up, they say, well, you know, it was unexpected, it was a black swan. I wrote a book called *"The Black Swan."* Unfortunately, they used my book backwards. Oh, and it was unexpected, highly unexpected. They keep their bonuses. They go on vacation and here you have a regular person working very hard, a taxpayer, a taxi driver, a post office worker paying taxes to subsidize retrospectively, all right, bonuses made. For example, a former government official made $121 million in bonuses at Citibank. Okay. He keeps his bonuses. We retrospectively are paying for that. That I said 13 years ago, and it keeps happening, and now we are still in the same situation.

So number four, and that is another crucial point. VaR has side effects. It is not neutral. You give someone a number—it has been shown and shown repeatedly, if you give someone a number, he will act on that number even if you tell him that that number is random. We humans cannot be trusted with numbers. You don't give someone the map of the Alps if he is on the Mount Ararat, all right, because he is going to act on that map. Even nothing is alot better, if it doesn't work. This is my central point, the side effects of numerical precision given to people who do not need it.

Number five: VaR-style quantitative risk management was behind leverage. We increased our leverage in society as we thought we thought we could measure risk. If you think you can measure your blow-up risk, you are going to borrow, you know. You have more overconfidence, also, as a side effect of measurement, and you are going to borrow. Instead of, you know, taking equity from people, you borrow, so when you blow up, you owe that money. And of course, as was discussed in my paper, debt bubbles are very vicious. Equity bubbles are not very vicious.

Conclusion: What should we be doing? Well, regulators should understand that finance is a complex system and complex systems have very clear characteristics, you know, and one of them is low levels of predictability, particularly of tail events. We have to worry—regulators should not encourage model error. My idea is to build a society that is resistant to expert mistakes. Regulators increased the dependence of society on expert mistakes and other things also in the Value-at-Risk, these AAA things. Okay. So we want to reduce that. We want to build a society that can sustain shocks because we are moving more and more into a world that delivers very large-scale variables, and we know exactly how they affect us or we know with some precision how they affect us, and we know how to build shocks. So the job of regulators should be to lower the impact of model error, and this is reminiscent of medicine. You know, the FDA, they don't let you bring any medicine without showing the side effects. Well, we should be doing the same thing in economic life. Thank you very much for this opportunity.

[The prepared statement of Dr. Taleb follows:]

PREPARED STATEMENT OF NASSIM N. TALEB

Report on the Risks of Financial Modeling, VaR and the Economic Breakdown

INTRODUCTION

Mr. Chairman, Ranking Member, Members of the Committee, thank you for giving me the opportunity to testify on the risk measurement methods used by banks, particularly those concerned with blowup risk, estimates of probabilities of losses from extreme events ("tail risks"), generally bundled under VaR.[1]

What is the VaR? It is simply a model that is supposed to project the expected extreme loss in an institution's portfolio that can occur over a specific time frame at a specified level of confidence. Take an example. A standard daily VaR of $1 million at a one percent probability tells you that you have less than a one percent chance of losing $1 million or more on a given day.[2] There are many modifications around VaR, "conditional VaR,"[3] so my discussion concerns *all* quantitative (and probabilistic) methods concerned with losses associated with rare events. Simply, there are limitations to our ability to measure the risks of extreme events.

Thirteen years ago, I warned that "VaR encourages misdirected people to take risks with shareholders', and ultimately taxpayers' money." I have since been begging for the suspension of these measurements of tail risks. But this came a bit late. For the banking system has lost so far, according to the International Monetary Fund, in excess of four trillion dollars directly as a result of faulty risk management. Most of the losses were in the U.S. and will be directly borne by taxpayers. These losses do not include the other costs of the economic crisis.

---

[1] The author thanks Daniel Kahneman, Pablo Triana, and Eric Weinstein for helpful discussions.

[2] Although such definition of VaR is often presented as a "maximum" loss, it is technically not so in an open-ended exposure: since, conditional on losing more than $1 million, you may lose a lot more, say $5 million.

[3] Data shows that methods meant to improve the standard VaR, like "expected shortfall" or "conditional VaR" are equally defective with economic variables—past losses do not predict future losses. Stress testing is also suspicious because of the subjective nature of "reasonable stress" number—we tend to underestimate the magnitude of outliers. "Jumps" are not predictable from past jumps. See Taleb, N.N. (in press) "Errors, robustness, and the fourth quadrant," *International Journal of Forecasting* (2009).

Data shows that banks routinely lose everything earned in their past history in single blowups—this happened in 1982, 1991, and, of course now. Every time society bails them out—while bank risk-takers retain their past bonuses and start the game afresh. This is an aberrant case of capitalism for the profits and socialism for the losses.

MAIN PROBLEMS ASSOCIATED WITH VAR-STYLE RISK MEASUREMENT

1. **These problems have been obvious all along**

My first encounter with the VaR was as a derivatives trader in the early 1990s when it was first introduced. I saw its underestimation of the risks of a portfolio by a factor of 100—you set up your book to lose no more than $100,000 and you take a $10,000,000 hit. Worse, there was no way to get a handle on how much its underestimation could be.

Using VaR after the crash of 1987 proved strangely gullible. But the fact that its use was not suspended after the many subsequent major events, such as the Long-Term Capital Management blowup in 1998, requires some explanation. Furthermore, regulators started promoting VaR (Basel 2) just as evidence was mounting against it.[4]

2. **VaR is ineffective and lacks in robustness**

Alas, we cannot "measure" the risk of *future* rare events like we measure the temperature. By robustness, I mean that the measure does not change much if you change the model, technique, or theory. Indeed risk estimation has nothing to do with the notion of *measure.* And the rarer the event, the harder it is to compute its probability—yet the rarer the event, the larger the consequences.[5]

Furthermore, the type of randomness we have with economic variables does not have a well-tractable, well-known structure, and can deliver vastly large events—and we are unable to get a handle on "how large." Conventional statistics, derived on a different class of variables, fail us here.[6,7]G5,[8]

3. **VaR encourages "low volatility, high blowup" risk taking which can be gamed by the Wall Street bonus structure**

Figure 1–A typical "blow-up" strategy with hidden risks: appearance of low volatility, with a high risk of blowup. The trader makes 11 bonuses, with no subsequent "clawback" as losses are borne by shareholders, then taxpayers. This is the profile for banks (losses in 1982,1991, and 2008) and many hedge funds. VaR encourages such types of risk taking.

---

[4] My recollection is that the VaR was not initially taken seriously by traders and managers. It took a long time for the practice to spread—and it was only after regulators got involved that it became widespread.

[5] See Taleb, N.N. and Pilpel, A. (2007) Epistemology and Risk Management, *Risk and Regulation,* 13.

[6] We are in the worst type of complex system characterized by high interdependence, low predictability, and vulnerability to extreme events. See N.N. Taleb, *The Black Swan,* Random House, 2007.

[7] There are other problems. 1) VaR does not replicate out of sample—the past almost never predicts subsequent blowups. (see data in the *Fourth Quadrant*). 2) A decrease in VaR does not mean decrease in risks; often quite the opposite holds, which allows the measure to be gamed.

[8] The roots of VaR come from modern financial theory (Markowitz, Sharpe, Miller, Merton, Scholes) which, in spite of its patent lack of scientific validity, continues to be taught in business schools. See Taleb, N.N., (2000), *The Black Swan: The Impact of the Highly Improbable,* Random House.

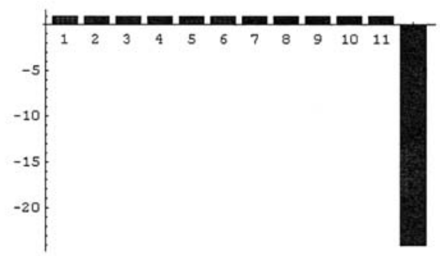


Figure 1- A typical "blow-up" strategy with hidden risks: appearance of low volatility, with a high risk of blowup. The trader makes 11 bonuses, with no subsequent "clawback" as losses are borne by shareholders, then taxpayers. This is the profile for banks (losses in 1982,1991, and 2008) and many hedge funds. VaR encourages such types of risk taking.

I have shown that operators like to engage in a "blow-up" strategy, (*switching risks from visible to hidden*), which consists in producing steady profits for a long time, collecting bonuses, then losing everything in a single blowup.[9] Such trades pay extremely well for the trader—but not for society. For instance, a member of Citicorp's executive committee (and former government official) collected $120 million of bonuses over the years of hidden risks before the blowup; regular taxpayers are financing him retrospectively.

Blowup risks kept increasing over the past few years, while the appearance of stability has increased.[10]

4. **Var has severe side effects (anchoring)**

Many people favor the adjunct application of VaR on grounds that it is "not harmful," using arguments like "we are aware of its defects." VaR has side effects of increasing risk-taking, even by those who know that it is not reliable. We have ample evidence of so called "anchoring"[11] in the calibration of decisions. Information, even when it is known to be sterile, increases overconfidence.

5. **VaR-style quantitative risk measurement is the engine behind leverage, the main cause of the current crisis**

Leverage[12] is a direct result of underestimation of the risks of extreme events—and the illusion that these risks are measurable. Someone more careful (or realistic) would issue equity.

April 28, 2004 was a very sad day, when the SEC, at the instigation of the investment banks, initiated the abandonment of hard (i.e., robust) risk measures like leverage, in favor of more model-based probabilistic, and fragile, ones.

[9] Taleb, N.N. (2004) "Bleed or Blowup: What Does Empirical Psychology Tell Us About the Preference For Negative Skewness?," *Journal of Behavioral Finance,* 5.

[10] Even Chairman Bernanke was fooled by the apparent stability as he pronounced it the "great moderation."

[11] Numerous experiments provide evidence that professionals are significantly influenced by numbers that they know to be irrelevant to their decision, like writing down the last four digits of one's social security number before making a numerical estimate of potential market moves. German judges rolling dice before sentencing showed an increase of 50 percent in the length of the sentence when the dice show a high number, without being conscious of it. See Birte Englich and Thomas Mussweiler, "Sentencing under Uncertainty: Anchoring Effects in the Courtroom," *Journal of Applied Social Psychology,* Vol. 31, No. 7 (2001), pp. 1535–1551; Birte Englich, Thomas Mussweiler, and Fritz Strack, "Playing Dice with Criminal Sentences: the Influence of Irrelevant Anchors on Experts' Judicial Decision Making," *Personality and Social Psychology Bulletin,* Vol. 32, No. 2 (Feb. 2006), pp. 188–200.

[12] There is a large difference between equity and credit bubbles. Equity bubbles are benign. We went through an equity bubble in 2000, without major problems.

Some credit can be benign. Credit that facilitates trade and economic transactions and finances conservative house-ownership does not have the same risk properties as credit for speculative reasons resulting from overconfidence.

CONCLUSION: WHAT REGULATORY STRUCTURE DO WE NEED?

Regulators should understand that financial markets are a complex system and work on increasing the robustness in it, by preventing "too big to fail" situations, favoring diversity in risk taking, allowing entities to absorb large shocks, and reducing the effect of model error (see "Ten Points for a Black Swan Robust Society," in Appendix II). This implies reliance on "hard," non-probabilistic measures rather than more error-prone ones. For instance "leverage" is a robust measures (like the temperature, it does not change with your model), while VaR is not.

Furthermore, we need to examine the toxicity of models; financial regulators should have the same test as the Food and Drug Administration does. The promoter of the probability model must be able to show that no one will be harmed even if the event is rare. Alas, the history of medicine shows translational gaps, the lag between the discovery of harm and suspension of harmful practice, lasting up to 200 years in pre-modern medicine.[13] Unfortunately, economics resemble pre-modern medicine.[14] But we cannot afford to wait 200 years to find out that the medicine is far worse than the disease. We cannot afford to wait even months.

APPENDIX I:

AUTHOR'S WARNINGS, 1996–2007

1996–1997

VaR is charlatanism because it tries to estimate something that is scientifically impossible to estimate, namely the risk of rare events. It gives people a misleading sense of precision. (*Derivatives Strategy, citing from Dynamic Hedging*)

VaR encourages misdirected people to take risks with shareholders', and ultimately taxpayers' money. (*Derivatives Strategy*)

2003

Fannie Mae's models (for calibrating to the risks of rare events) are pseudoscience. (*New York Times—Alex Berenson's article on FNMA)*

"What happened to LTCM will look like a picnic compared to what should happen to you." (*Lecture, Women in Hedge Funds Association, cited in Hedge World*)

2007

Fannie Mae, when I look at its risks, seems to be sitting on a barrel of dynamite, vulnerable to the slightest hiccup. But not to worry: their large staff of scientists deems these events "unlikely." (*The Black Swan*)

Banks are now more vulnerable to the Black Swan than ever before with "scientists" among their staff taking care of exposures. The giant firm, J.P. Morgan, put the entire world at risk by introducing in the nineties RiskMetrics, a phony method aiming at managing people's risks. A related method called "Value-at-Risk," which relies on the quantitative measurement of risk, has been spreading. (*The Black Swan*)

[13] "When William Harvey demonstrated the mechanism of blood circulation in the 1620s, humoral theory and its related practices should have disappeared, because the anatomy and physiology on which it relied was incompatible with this picture of the organism. In fact, people continued to refer to spirits and humors, and doctors continued to prescribe phlebotomies, enemas, and cataplasms, for centuries more—even when it was established in the mid-1800, most notably by Louis Pasteur, that germs were the cause of disease." Noga Arikha "Just Life in a Nutshell: Humours as common sense," in *The Philosophical Forum Quarterly,* XXXIX, 3.

[14] Most of the use of probabilistic methods lacking both mathematical and empirical justification can be attributed to the prestige given to modern finance by the various Nobel memorial prizes in economics. See P. Triana, 2009, *Lecturing Birds on Flying: Can Mathematical Theories Destroy the Markets?,* J. Wiley.

APPENDIX II:

TEN PRINCIPLES FOR A BLACK SWAN ROBUST WORLD

(*FINANCIAL TIMES,* APRIL 8, 2009)

1. **What is fragile should break early while it is still small.** Nothing should ever become too big to fail. Evolution in economic life helps those with the maximum amount of hidden risks—and hence the most fragile—become the biggest.

2. **No socialization of losses and privatization of gains.** Whatever may need to be bailed out should be nationalized; whatever does not need a bail-out should be free, small and risk-bearing. We have managed to combine the worst of capitalism and socialism. In France in the 1980s, the socialists took over the banks. In the U.S. in the 2000s, the banks took over the government. This is surreal.

3. **People who were driving a school bus blindfolded (and crashed it) should never be given a new bus.** The economics establishment (universities, regulators, central bankers, government officials, various organizations staffed with economists) lost its legitimacy with the failure of the system. It is irresponsible and foolish to put our trust in the ability of such experts to get us out of this mess. Instead, find the smart people whose hands are clean.

4. **Do not let someone making an "incentive" bonus manage a nuclear plant—or your financial risks.** Odds are he would cut every corner on safety to show "profits" while claiming to be "conservative." Bonuses do not accommodate the hidden risks of blow-ups. It is the asymmetry of the bonus system that got us here. No incentives without disincentives: capitalism is about rewards and punishments, not just rewards.

5. **Counter-balance complexity with simplicity. Complexity from globalization and highly networked economic life needs to be countered by simplicity in financial products.** The complex economy is already a form of leverage: the leverage of efficiency. Such systems survive thanks to slack and redundancy; adding debt produces wild and dangerous gyrations and leaves no room for error. Capitalism cannot avoid fads and bubbles: equity bubbles (as in 2000) have proved to be mild; debt bubbles are vicious.

6. **Do not give children sticks of dynamite, even if they come with a warning.** Complex derivatives need to be banned because nobody understands them and few are rational enough to know it. Citizens must be protected from themselves, from bankers selling them "hedging" products, and from gullible regulators who listen to economic theorists.

7. **Only Ponzi schemes should depend on confidence. Governments should never need to "restore confidence."** Cascading rumors are a product of complex systems. Governments cannot stop the rumors. Simply, we need to be in a position to shrug off rumors, be robust in the face of them.

8. **Do not give an addict more drugs if he has withdrawal pains.** Using leverage to cure the problems of too much leverage is not homeopathy, it is denial. The debt crisis is not a temporary problem, it is a structural one. We need rehab.

9. **Citizens should not depend on financial assets or fallible "expert" advice for their retirement.** Economic life should be definancialized. We should learn not to use markets as storehouses of value: they do not harbor the certainties that normal citizens require. Citizens should experience anxiety about their own businesses (which they control), not their investments (which they do not control).

10. **Make an omelet with the broken eggs.** Finally, this crisis cannot be fixed with makeshift repairs, no more than a boat with a rotten hull can be fixed with ad hoc patches. We need to rebuild the hull with new (stronger) materials; we will have to remake the system before it does so itself. Let us move voluntarily into Capitalism 2.0 by helping what needs to be broken break on its own, converting debt into equity, marginalizing the economics and business school establishments, shutting down the "Nobel" in economics, banning leveraged buy-outs, putting bankers where they belong, clawing back the bonuses of those who got us here, and teaching people to navigate a world with fewer certainties.

Then we will see an economic life closer to our biological environment: smaller companies, richer ecology, no leverage. A world in which entrepreneurs, not bankers, take the risks, and companies are born and die every day without making the news.

In other words, a place more resistant to black swans.

Ten principles for a Black Swan-proof world

By Nassim Nicholas Taleb
Published: April 7 2009 20:02 | Last updated: April 7 2009 20:02

1. *What is fragile should break early while it is still small.* Nothing should ever become too big to fail. Evolution in economic life helps those with the maximum amount of hidden risks – and hence the most fragile – become the biggest.

2. *No socialisation of losses and privatisation of gains.* Whatever may need to be bailed out should be nationalised; whatever does not need a bail-out should be free, small and risk-bearing. We have managed to combine the worst of capitalism and socialism. In France in the 1980s, the socialists took over the banks. In the US in the 2000s, the banks took over the government. This is surreal.

3. *People who were driving a school bus blindfolded (and crashed it) should never be given a new bus.* The economics establishment (universities, regulators, central bankers, government officials, various organisations staffed with economists) lost its legitimacy with the failure of the system. It is irresponsible and foolish to put our trust in the ability of such experts to get us out of this mess. Instead, find the smart people whose hands are clean.

4. *Do not let someone making an "incentive" bonus manage a nuclear plant – or your financial risks.* Odds are he would cut every corner on safety to show "profits" while claiming to be "conservative". Bonuses do not accommodate the hidden risks of blow-ups. It is the asymmetry of the bonus system that got us here. No incentives without disincentives: capitalism is about rewards and punishments, not just rewards.

5. *Counter-balance complexity with simplicity.* Complexity from globalisation and highly networked economic life needs to be countered by simplicity in financial products. The complex economy is already a form of leverage: the leverage of efficiency. Such systems survive thanks to slack and redundancy; adding debt produces wild and dangerous gyrations and leaves no room for error. Capitalism cannot avoid fads and bubbles: equity bubbles (as in 2000) have proved to be mild; debt bubbles are vicious.

6. *Do not give children sticks of dynamite, even if they come with a warning* . Complex derivatives need to be banned because nobody understands them and few are rational enough to know it. Citizens must be protected from themselves, from bankers selling them "hedging" products, and from gullible regulators who listen to economic theorists.

7. *Only Ponzi schemes should depend on confidence. Governments should never need to "restore confidence".* Cascading rumours are a product of complex systems. Governments cannot stop the rumours. Simply, we need to be in a position to shrug off rumours, be robust in the face of them.

8. *Do not give an addict more drugs if he has withdrawal pains.* Using leverage to cure the problems of too much leverage is not homeopathy, it is denial. The debt crisis is not a temporary problem, it is a structural one. We need rehab.

9. *Citizens should not depend on financial assets or fallible "expert" advice for their retirement.* Economic life should be definancialised. We should learn not to use markets as storehouses of value: they do not harbour the certainties that normal citizens require. Citizens should experience anxiety about their own businesses (which they control), not their investments (which they do not control).

10. *Make an omelette with the broken eggs.* Finally, this crisis cannot be fixed with makeshift repairs, no more than a boat with a rotten hull can be fixed with ad-hoc patches. We need to rebuild the hull with new (stronger) materials; we will have to remake the system before it does so itself. Let us move voluntarily into Capitalism 2.0 by helping what needs to be broken break on its own, converting debt into equity, marginalising the economics and business school establishments, shutting down the "Nobel" in economics, banning leveraged buyouts, putting bankers where they belong, clawing back the bonuses of those who got us here, and teaching people to navigate a world with fewer certainties.

Then we will see an economic life closer to our biological environment: smaller companies, richer ecology, no leverage. A world in which entrepreneurs, not bankers, take the risks and companies are born and die every day without making the news.

In other words, a place more resistant to black swans.

The writer is a veteran trader, a distinguished professor at New York University's Polytechnic

Institute and the author of The Black Swan: The Impact of the Highly Improbable

Print article Email article Order reprints

Digg reddit LinkedIn Facebook Delicious

Mixx Propeller Yahoo! Buzz stumbleupon

MORE IN THIS SECTION

For America, the problem is Pakistan
Financial groups must still be free to compete
A chance for bankers to refocus their talents
A freer China would stimulate spending
How to save the market economy in Europe
Why this will not be a normal cyclical recovery
Outside Edge: The new off-pitch sobriety
No easy way to put a face on the stay-at-home recession
Obama's message on security should be candid
What the French revolution can teach America

FT Home Site map Contact us Help

Statistical and applied probabilistic knowledge is the core of knowledge; statistics is what tells you if something is true, false, or merely anecdotal; it is the "logic of science"; it is the instrument of risk-taking; it is the applied tools of epistemology; you can't be a modern intellectual and not think probabilistically—but... let's not be suckers. The problem is much more complicated than it seems to the casual, mechanistic user who picked it up in graduate school. Statistics can fool you. In fact it is fooling your government right now. It can even bankrupt the system (let's face it: use of probabilistic methods for the estimation of risks did just blow up the banking system).

THE FOURTH QUADRANT: A MAP OF THE LIMITS OF STATISTICS
[9.15.08]
By Nassim Nicholas Taleb

An *Edge* Original Essay

Introduction

When Nassim Taleb talks about the limits of statistics, he becomes outraged. "My outrage," he says, "is aimed at the scientist-charlatan putting society at risk using statistical methods. This is similar to iatrogenics, the study of the doctor putting the patient at risk." As a researcher in probability, he has some credibility. In 2006, using FNMA and bank risk managers as his prime perpetrators, he wrote the following:

> The government-sponsored institution Fannie Mae, when I look at its risks, seems to be sitting on a barrel of dynamite, vulnerable to the slightest hiccup. But not to worry: their large staff of scientists deemed these events "unlikely."

In the following *Edge* original essay, Taleb continues his examination of Black Swans, the highly improbable and unpredictable events that have massive impact. He claims

that those who are putting society at risk are "no true statisticians", merely people using statistics either without understanding them, or in a self-serving manner. "The current subprime crisis did wonders to help me drill my point about the limits of statistically driven claims," he says.

Taleb, looking at the cataclysmic situation facing financial institutions today, points out that "the banking system, betting *against* Black Swans, has lost over 1 Trillion dollars (so far), more than was ever made in the history of banking".

But, as he points out, there is also good news.

> We can identify *where the danger zone is located*, which I call "the fourth quadrant", and show it on a map with more or less clear boundaries. A map is a useful thing because you know where you are safe and where your knowledge is questionable. So I drew for the *Edge* readers a tableau showing the boundaries where statistics works well and where it is questionable or unreliable. Now once you identify where the danger zone is, where your knowledge is no longer valid, you can easily make some policy rules: how to conduct yourself in that fourth quadrant; what to avoid.

—John Brockman

NASSIM NICHOLAS TALEB, essayist and former mathematical trader, is Distinguished Professor of Risk Engineering at New York University's Polytechnic Institute. He is the author of *Fooled by Randomness* and the international bestseller *The Black Swan*.

Nassim Taleb's Edge Bio Page

REALITY CLUB: Jaron Lanier, George Dyson

BLOGWATCH

THE FOURTH QUADRANT: A MAP OF THE LIMITS OF STATISTICS

Statistical and applied probabilistic knowledge is the core of knowledge; statistics is what tells you if something is true, false, or merely anecdotal; it is the "logic of science"; it is the instrument of risk-taking; it is the applied tools of epistemology; you can't be a modern intellectual and not think probabilistically—but... let's not be suckers. The problem is much more complicated than it seems to the casual, mechanistic user who picked it up in graduate school. Statistics can fool you. In fact it is fooling your government right now. It can even bankrupt the system (let's face it: use of probabilistic methods for the estimation of risks did just *blow up* the banking system).

The current subprime crisis has been doing wonders for the reception of any ideas about probability-driven claims in science, particularly in social science, economics, and "econometrics" (quantitative economics). Clearly, with current International Monetary Fund estimates of the costs of the 2007-2008 subprime crisis, the banking system seems to have lost more on risk taking (from the failures of quantitative risk management) than every penny banks *ever* earned taking risks. But it was easy to see from the past that the pilot did not have the qualifications to fly the plane and was using the wrong navigation tools: The same happened in 1983 with money center banks losing cumulatively every penny ever made, and in 1991-1992 when the Savings and Loans industry became history.

It appears that financial institutions earn money on transactions (say fees on your mother-in-law's checking account) and lose everything taking risks they don't understand. I want this to stop, and stop now— the current patching by the banking establishment worldwide is akin to using the same doctor to cure the patient when the doctor has a track record of systematically killing them. And this is not limited to banking—I generalize to an entire class of random variables that do not have the structure we thing they have, in which we can be suckers.

And we are beyond suckers: not only, for socio-economic and other nonlinear, complicated variables, we are riding in a bus driven a blindfolded driver, but we refuse to acknowledge it in spite of the evidence, which to me is a pathological problem with academia. After 1998, when a "Nobel-crowned" collection of people (and the crème de la crème of the financial economics establishment) blew up Long Term Capital Management, a hedge fund, because the "scientific" methods they used misestimated the role of the rare event, such methodologies and such claims on understanding risks of rare events should have been discredited. Yet the Fed helped their bailout *and* exposure to rare events (and model error) patently increased exponentially (as we can see from banks' swelling portfolios of derivatives that we do not understand).

Are we using models of uncertainty to produce certainties?

This masquerade does not seem to come from statisticians—but from the commoditized, "me-too" users of the products. Professional statisticians can be remarkably introspective and self-critical. Recently, the American Statistical Association had a special panel session on the "black swan" concept at the annual Joint Statistical Meeting in Denver last August. They insistently made a distinction between the "statisticians" (those who deal with the subject itself and design the tools and methods) and those in other fields who pick up statistical tools from textbooks without really understanding them. For them it is a problem with statistical education and half-baked expertise. Alas, this category of blind users includes regulators and risk managers, whom I accuse of creating more risk than they reduce.

So the good news is that we can identify *where the danger zone is located*, which I call "the fourth quadrant", and show it on a map with more or less clear boundaries. A map is a useful thing because you know where you are safe and where your knowledge is questionable. So I drew for the *Edge* readers a tableau showing the boundaries where statistics works well and where it is questionable or unreliable. Now once you identify where the danger zone is, where your knowledge is no longer valid, you can easily make some policy rules: how to conduct yourself in that fourth quadrant;

what to avoid.

So the principal value of the map is that it allows for policy making. Indeed, I am moving on: my new project is about methods on how to domesticate the unknown, exploit randomness, figure out *how to live in a world we don't understand very well*. While most human thought (particularly since the enlightenment) has focused us on how to turn knowledge into decisions, my new mission is to build methods to turn lack of information, lack of understanding, and lack of "knowledge" into decisions—how, as we will see, not to be a "turkey".

This piece has a technical appendix that presents mathematical points and empirical evidence. (See link below.) It includes a battery of tests showing that no known conventional tool can allow us to make precise statistical claims in the Fourth Quadrant. While in the past I limited myself to citing research papers, and evidence compiled by others (a less risky trade), here I got hold of more than 20 million pieces of data (includes 98% of the corresponding macroeconomics values of transacted daily, weekly, and monthly variables for the last 40 years) and redid a systematic analysis that includes recent years.

What Is Fundamentally Different About Real Life

My anger with "empirical" claims in risk management does not come from research. It comes from spending twenty tense (but entertaining) years taking risky decisions in the real world managing portfolios of complex derivatives, with payoffs that depend on higher order statistical properties —and you quickly realize that a certain class of relationships that "look good" in research papers almost *never* replicate in real life (in spite of the papers making some claims with a "p" close to infallible). But that is not the main problem with research.

For us the world is vastly simpler in some sense than the academy, vastly more complicated in another. So the central lesson from decision-making (as opposed to working with data on a computer or bickering about logical constructions) is the following: *it is the exposure (or payoff) that creates the complexity —and the opportunities and dangers— not so much the knowledge (i.e., statistical distribution, model representation, etc.)*. In some situations, you can be extremely wrong and be fine, in others you can be slightly wrong and explode. If you are leveraged, errors blow you up; if you are not, you can enjoy life.

So knowledge (i.e., if some statement is "true" or "false") matters little, very little in many situations. In the real world, there are very few situations where what you do and your belief if some statement is true or false naively map into each other. Some decisions require vastly more caution than others—or highly more drastic confidence intervals. For instance you do not "need evidence" that the water is poisonous to *not* drink from it. You do not need "evidence" that a gun is loaded to avoid playing Russian roulette, or evidence that a thief a on the lookout to lock your door. You need evidence of safety—not evidence of lack of safety— a central asymmetry that affects us with rare events. This asymmetry in skepticism makes it easy to draw a map of danger spots.

The Dangers Of Bogus Math

I start with my old crusade against "quants" (people like me who do mathematical work in finance), economists, and bank risk managers, my prime perpetrators of iatrogenic risks (the healer killing the patient). Why iatrogenic risks? Because, not only have economists been unable to *prove that* their models work, but no one managed to prove that the use of a model that does not work is *neutral*, that it does not increase blind risk taking, hence the accumulation of hidden risks.

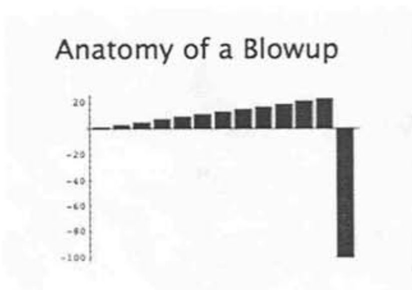


Figure 1 *My classical metaphor: A Turkey is fed for a 1000 days—every days confirms to its statistical department that the human race cares about its welfare "with increased statistical significance". On the 1001^{st} day, the turkey has a surprise.*

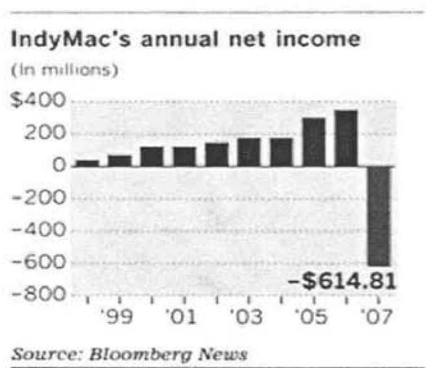


Figure 2 *The graph above shows the fate of close to 1000 financial institutions (includes busts such as FNMA, Bear Stearns, Northern Rock, Lehman Brothers, etc.). The banking system (betting AGAINST rare events) just lost > 1 Trillion dollars (so far) on a single error, more than was ever earned in the history of banking. Yet bankers kept their previous bonuses and it looks like citizens have to foot the bills. And one Professor Ben Bernanke pronounced right before the blowup that we live in an era of stability and "great moderation" (he is now piloting a plane*

and we all are passengers on it).

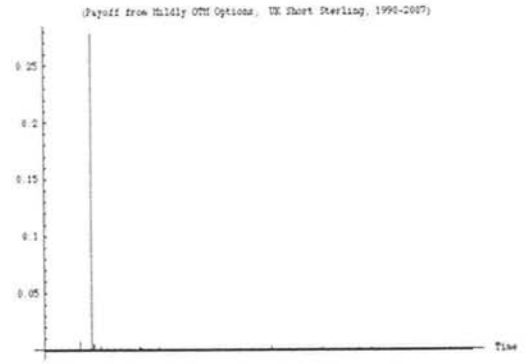


Figure 3 *The graph shows the daily variations a derivatives portfolio exposed to U.K. interest rates between 1988 and 2008. Close to 99% of the variations, over the span of 20 years, will be represented in 1 single day—the day the European Monetary System collapsed. As I show in the appendix, this is typical with ANY socio-economic variable (commodity prices, currencies, inflation numbers, GDP, company performance, etc.). No known econometric statistical method can capture the probability of the event with any remotely acceptable accuracy (except, of course, in hindsight, and "on paper"). Also note that this applies to surges on electricity grids and all manner of modern-day phenomena.*

Figures 1 and 2 show you the classical problem of the turkey making statements on the risks based on past history (mixed with some theorizing that happens to narrate well with the data). A friend of mine was sold a package of subprime loans (leveraged) on grounds that "30 years of history show that the trade is safe." He found the argument unassailable "empirically". And the unusual dominance of the rare event shown in Figure 3 is not unique: it affects *all* macroeconomic data—if you look long enough almost *all* the contribution in some classes of variables will come from rare events (I looked in the appendix at 98% of trade-weighted data).

Now let me tell you what worries me. Imagine that the Turkey can be the most powerful man in world economics, managing our economic fates. How? A then-Princeton economist called Ben Bernanke made a pronouncement in late 2004 about the "new moderation" in economic life: the world getting more and more stable—before becoming the Chairman of the Federal Reserve. Yet the system was getting riskier and riskier as we were turkey-style sitting on more and more barrels of dynamite—and Prof. Bernanke's predecessor the former Federal Reserve Chairman Alan Greenspan was systematically increasing the hidden risks in the system, making us all more vulnerable to blowups.

By the "narrative fallacy" the turkey economics department will always manage to state, before thanksgivings that "we are in a new era of safety", and back-it up with thorough and "rigorous" analysis. And Professor Bernanke indeed found plenty of economic explanations—what I call the narrative fallacy—with graphs, jargon, curves, the kind of facade-of-knowledge that you find in economics textbooks. (This is the find of glib, snake-oil facade of knowledge—even more dangerous because of the mathematics—that made me, before accepting the new position in NYU's engineering department, verify that there was not a single economist in the building. I have nothing against economists: you should let them entertain each others with their theories and elegant mathematics, and help keep college students inside buildings. But beware: they can be plain wrong, yet frame things in a way to make you feel stupid arguing with them. So make sure you do not give any of them risk-management responsibilities.)

Bottom Line: The Map

Things are made simple by the following. There are two *distinct* types of decisions, and two *distinct* classes of randomness.

Decisions: The first type of decisions is simple, "binary", i.e. you just care if something is true or false. Very true or very false does not matter. Someone is either pregnant or not pregnant. A statement is "true" or "false" with some confidence interval. (I call these M0 as, more technically, they depend on the zeroth moment, namely just on probability of events, and not their magnitude —you just care about "raw" probability). A biological experiment in the laboratory or a bet with a friend about the outcome of a soccer game belong to this category.

The second type of decisions is more complex. You do not just care of the frequency—but of the impact as well, or, even more complex, some function of the impact. So there is another layer of uncertainty of impact. (I call these M1+, as they depend on higher moments of the distribution). When you invest you do not care how many times you make or lose, you care about the expectation: how many times you make or lose *times* the amount made or lost.

Probability structures: There are two classes of probability domains—very distinct qualitatively and quantitatively. The first, thin-tailed: Mediocristan", the second, thick tailed Extremistan. Before I get into the details, take the literary distinction as follows:

In Mediocristan, exceptions occur but don't carry large consequences. Add the heaviest person on the planet to a sample of 1000. The total weight would barely change. In Extremistan, exceptions can be everything (they will eventually, in time, represent everything). Add Bill Gates to your sample: the wealth will jump by a factor of >100,000. So, in Mediocristan, large deviations occur but they are not consequential—unlike Extremistan.

Mediocristan corresponds to "random walk" style randomness that you tend to find in regular textbooks (and in popular books on randomness). Extremistan corresponds to a "random jump" one. The first kind I can call "Gaussian-Poisson", the second "fractal" or Mandelbrotian (after the works of the great Benoit Mandelbrot linking it to the geometry of nature). But note here an epistemological question: there is a category of "I don't know" that I also bundle in Extremistan for the sake of decision making—simply because I don't know much about the probabilistic structure or the role of large events.

The Map

Now it lets see where the traps are:

> First Quadrant: Simple binary decisions, in Mediocristan: Statistics does wonders. These situations are, unfortunately, more common in academia, laboratories, and games than real life—what I call the "ludic fallacy". In other words, these are the situations in casinos, games, dice, and we tend to study them because we are successful in modeling them.
>
> Second Quadrant: Simple decisions, in Extremistan: some well known problem studied in the literature. Except of course that there are not many simple decisions in Extremistan.
>
> Third Quadrant: Complex decisions in Mediocristan: Statistical methods work surprisingly well.
>
> **Fourth Quadrant**: Complex decisions in Extremistan: Welcome to the Black Swan domain. Here is where your limits are. Do not base your decisions on statistically based claims. Or, alternatively, try to move your exposure type to make it third-quadrant style ("clipping tails").

APPLICATION DOMAIN	**Simple payoffs**	**Complex payoffs**
Distribution 1 ("thin tailed")	Extremely robust to Black Swans	Quite robust to Black Swans
Distribution 2 ("heavy" and/or unknown tails, no or unknown characteristic scale)	Quite robust to Black Swans	**LIMITS of Statistics – extreme fragility to Black Swans**

The four quadrants. The South-East area (in orange) is where statistics and models fail us.

Tableau Of Payoffs

SIMPLE PAYOFFS	**COMPLEX PAYOFFS**	**VERY COMPLEX PAYOFFS**
"True/False?"	**"How Much?"**	**"How Much? Really?"**
Mo **(depend on raw probability, i.e. probability times payoff to the 0^{th} power)**	**M1** **Expectations** **LINEAR PAYOFF** **(Simple expectation, probability times payoff)**	**M2+** **NONLINEAR PAYOFF** **(Probability times a power (or convex function) of the payoff - square, cubic, etc.)**
Medicine (health not epidemics)	Security: Terrorism, Natural catastrophes	Payoffs from innovation, convex technologies
Psychological experiments	Environmental problems	Societal consequences of pandemics
Bets (prediction markets)	Climate	Calibration of nonlinear models
Binary/Digital derivatives	General risk management	Expectation weighted by nonlinear utility
Life/Death (for one person)	Epidemics, Life/Death for *n* persons (n stochastic)	Errors in analyses of deviations
Bankruptcy (for a single company)	Insurance, measures of expected shortfall	Leveraged portfolios (around the loss point)
Single insurance (capped)	Insurance (large portfolio), open-ended catastrophe insurance	Derivative payoffs, Dynamically hedged portfolios
What Else? **Very Little!**	Finance (Investments)	Kurtosis-based positioning ("volatility trading")
	Casinos	Cubic payoffs

Two Difficulties

Let me refine the analysis. The passage from theory to the real world presents two distinct difficulties: "inverse problems" and "pre-asymptotics".

> **Inverse Problems.** It is the greatest epistemological difficulty I know. In real life we do not observe probability distributions (not even in Soviet Russia, not even the French government). We just observe events. So we do not know the statistical properties—until, of course, after the fact. Given a set of observations, plenty of statistical distributions can correspond to the exact same realizations—each would extrapolate differently outside the set of events on which it was derived. The inverse problem is more acute when more theories, more distributions can fit a set a data.
>
> This inverse problem is compounded by the small sample properties of rare events as these will be naturally rare in a past sample. It is also acute in the presence of nonlinearities as the families of possible models/parametrization explode in numbers.
>
> **Pre-asymptotics**. Theories are, of course, bad, but they can be worse in some situations when they were derived in idealized situations, the asymptote, but are used outside the asymptote (its limit, say infinity or the infinitesimal). Some asymptotic properties do work well preasymptotically (Mediocristan), which is why casinos do well, but others do not, particularly when it comes to Extremistan.
>
> Most statistical education is based on these asymptotic, Platonic properties—yet we live in the real world that rarely resembles the asymptote. Furthermore, this compounds the ludic fallacy: most of what students of statistics do is assume a structure, typically with a known probability. Yet the problem we have is not so much making computations once you know the probabilities, but finding the true distribution.

The Inverse Problem Of The Rare Events

Let us start with the inverse problem of rare events and proceed with a simple, nonmathematical argument. In August 2007, *The Wall Street Journal* published a statement by one financial economist, expressing his surprise that financial markets experienced a string of events that "would happen once in 10,000 years". A portrait of the gentleman accompanying the article revealed that he was considerably younger than 10,000 years; it is therefore fair to assume that he was not drawing his inference from his own empirical experience (and not from history at large), but from some theoretical model that produces the risk of rare events, or what he perceived to be rare events.

Alas, the rarer the event, the more theory you need (since we don't observe it). So *the rarer the event, the worse its inverse problem*. And theories are fragile (just think of Doctor Bernanke).

The tragedy is as follows. Suppose that you are deriving probabilities of future occurrences from the data, assuming (generously) that the past is representative of the future. Now, say that you estimate that an event happens every 1,000 days. You will need a lot more data than 1,000 days to ascertain its frequency, say 3,000 days. Now, what if the event happens once every 5,000 days? The estimation of this probability requires some larger number, 15,000 or more. The smaller the probability, the more observations you need, and the greater the estimation error for a set number of observations. Therefore, to estimate a rare event you need a sample that is larger and larger in inverse proportion to the occurrence of the event.

If small probability events carry large impacts, and (at the same time) these small probability events are more difficult to compute from past data itself, then: our empirical knowledge about the potential contribution—or role—of rare events (probability × consequence) is inversely proportional to their impact. This is why we should worry in the fourth quadrant!

For rare events, the confirmation bias (the tendency, Bernanke-style, of finding samples that confirm your opinion, not those that disconfirm it) is very costly and very distorting. Why? Most of histories of Black Swan prone events is going to be Black Swan free! Most samples will not reveal the black swans—except after if you are hit with them, in which case you will not be in a position to discuss them. Indeed I show with 40 years of data that past Black Swans *do not* predict future Black Swans in socio-economic life.

Figure 4 *The Confirmation Bias At Work. For left-tailed fat-tailed distributions, we do not see much of negative outcomes for surviving entities AND we have a small sample in the left tail. This is why we tend to see a better past for a certain class of time series than warranted.*

Fallacy Of The Single Event Probability

Let us look at events in Mediocristan. In a developed country a newborn female is expected to die at around 79, according to insurance tables. When she reaches her 79th birthday, her life expectancy, assuming that she is in typical health, is another 10 years. At the age of 90, she should have another 4.7 years to go. So if you are told that a person is older than 100, you can estimate that he is 102.5 and conditional on the person being older than 140 you can estimate that he is 140 plus a few minutes. The conditional expectation of additional life drops as a person gets older.

In Extremistan things work differently and the conditional expectation of an increase in a random variable does not drop as the variable gets larger. In the real world, say with stock returns (and all economic variable), conditional on a loss being worse than the 5 units, to use a conventional unit of measure units, it will be around 8 units. Conditional that a move is more than 50 STD it should be around 80 units, and if we go all the way until the sample is depleted, the average move worse than 100 units is 250 units! This extends all the way to areas in which we have sufficient sample.

This tells us that there is "no typical" failure and "no typical" success. You may be able to predict the occurrence of a war, but you will not be able to gauge its effect! Conditional on a war killing more than 5 million people, it should kill around 10 (or more). Conditional on it killing more than 500 million, it would kill a billion (or more, we don't know). You may correctly predict a skilled person getting "rich", but he can make a million, ten million, a billion, ten billion—there is no typical number. We have data, for instance, for predictions of drug sales, conditional on getting things right. Sales estimates are totally uncorrelated to actual sales—some drugs that were correctly predicted to be successful had their sales underestimated by up to 22 times!

This absence of "typical" event in Extremistan is what makes prediction markets ludicrous, as they make events look binary. "A war" is meaningless: you need to estimate its damage—and no damage is typical. Many predicted that the First War would occur—but nobody predicted its magnitude. Of the reasons economics does not work is that the literature is almost completely blind to the point.

A Simple Proof Of Unpredictability In The Fourth Quadrant

I show elsewhere that if you don't know what a "typical" event is, fractal power laws are the most effective way to *discuss* the extremes mathematically. It does not mean that the real world generator is actually a power law—it means you don't understand the structure of the external events it delivers and need a tool of analysis so you do not become a turkey. Also, fractals simplify the mathematical discussions because all you need is play with one parameter (I call it "alpha") and it increases or decreases the role of the rare event in the total properties.

For instance, you move alpha from 2.3 to 2 in the publishing business, and the sales of books in excess of 1 million copies triple! Before meeting Benoit Mandelbrot, I used to play with combinations of scenarios with series of probabilities and series of payoffs filling spreadsheets with clumsy simulations; learning to use fractals made such analyses immediate. Now all I do is change the alpha and see what's going on.

Now the problem: *Parametrizing a power law lends itself to monstrous estimation errors* (I said that heavy tails have horrible inverse problems). Small changes in the "alpha" main parameter used by power laws leads to monstrously large effects in the tails. Monstrous.

And we don't observe the "alpha. Figure 5 shows more than 40 thousand computations of the tail exponent "alpha" from different samples of different economic variables (data for which it is impossible to refute fractal power laws). We clearly have problems figuring it what the "alpha" is: our results are marred with errors. Clearly the mean absolute error is in excess of 1 (i.e. between alpha=2 and alpha=3). Numerous papers in econophysics found an "average" alpha between 2 and 3—but if you process the >20 million pieces of data analyzed in the literature, you find that the variations between single variables are extremely significant.

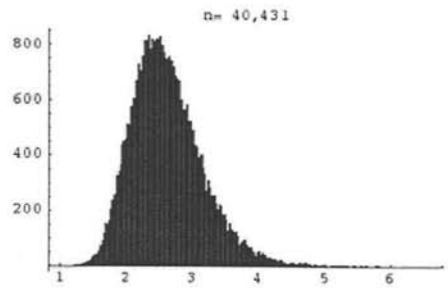


***Figure 5**—Estimation error in "alpha" from 40 thousand economic variables. I thank Pallop Angsupun for data.*

Now this mean error has massive consequences. Figure 6 shows the effect: the expected value of your losses in excess of a certain amount(called "shortfall") is multiplied by >10 from a small change in the "alpha" that is less than its mean error! These are the losses banks were talking about with confident precision!

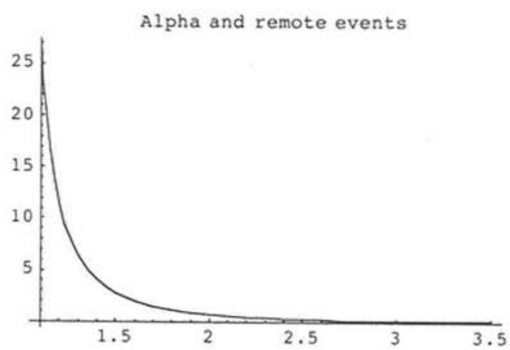


***Figure 6**—The value of the expected shortfall (expected losses in excess of a certain threshold) in response to changes in tail exponent "alpha". We can see it explode by an order of magnitude.*

What if the distribution is not a power law? This is a question I used to get once a day. Let me repeat it: my argument would not change—it would take longer to phrase it.

Many researchers, such as Philip Tetlock, have looked into the incapacity of social scientists in forecasting (economists, political scientists). It is thus evident that while the forecasters might be just "empty suits", the forecast errors are dominated by rare events, and we are limited in our ability to track them. The "wisdom of crowds" might work in the first three quadrant; but it certainly fails (and has failed) in the fourth.

Living In The Fourth Quadrant

Beware the Charlatan. When I was a quant-trader in complex derivatives, people mistaking my profession used to ask me for "stock tips" which put me in a state of rage: a charlatan is someone likely (statistically) to give you positive advice, of the "how to" variety.

Go to a bookstore, and look at the business shelves: you will find plenty of books telling you how to make your first million, or your first quarter-billion, etc. You will not be likely to find a book on "how I failed in business and in life"—though the second type of advice is vastly more informational, and typically less charlatanic. Indeed, the only popular such finance book I found that was not quacky in nature—on how someone lost his fortune—was both self-published and out of print. Even in academia, there is little room for promotion by publishing negative results—though these, are vastly informational and less marred with statistical biases of the kind we call data snooping. So all I am saying is "what is it that *we don't know*", and my advice is what to avoid, no more.

You can live longer if you avoid death, get better if you avoid bankruptcy, and become prosperous if you avoid blowups in the fourth quadrant.

Now you would think that people would buy my arguments about lack of knowledge and accept unpredictability. But many kept asking me "now that you say that our measures are wrong, do you have anything better?"

I used to give the same mathematical finance lectures for both graduate students and practitioners before giving up on academic students and grade-seekers. Students cannot understand the value of "this is what we don't know"—they think it is *not* information, that they are learning nothing. Practitioners on the other hand value it immensely. Likewise with statisticians: I never had a disagreement with statisticians (who build the field)—only with users of statistical methods.

Spyros Makridakis and I are editors of a special issue of a decision science journal, *The International Journal of Forecasting*. The issue is about "What to do in an environment of low predictability". We received tons of papers, but guess what? Very few addressed the point: they mostly focused on showing us that they predict better (on paper). This convinced me to engage in my new project: "how to live in a world we don't understand".

So for now I can produce phronetic rules (in the Aristotelian sense of *phronesis*, decision-making wisdom). Here are a few, to conclude.

Phronetic Rules: What Is Wise To Do (Or Not Do) In The Fourth Quadrant

1) Avoid Optimization, Learn to Love Redundancy. Psychologists tell us that getting rich does not bring happiness—if you spend it. But if you hide it under the mattress, you are less vulnerable to a black swan. Only fools (such as Banks) optimize, not realizing that a simple model error can blow through their capital (as it just did). In one day in August 2007, Goldman Sachs experienced 24 x the average daily transaction volume—would 29 times have blown up the system? The only weak point I know of financial markets is their ability to drive people & companies to "efficiency" (to please a stock analyst's earnings target) against risks of extreme events.

Indeed some systems tend to optimize—therefore become more fragile. Electricity grids for example optimize to the point of not coping with unexpected surges—Albert-Lazlo Barabasi warned us of the possibility of a NYC blackout like the one we had in August 2003. Quite prophetic, the fellow. Yet energy supply kept getting more and more efficient since. Commodity prices can double on a short burst in demand (oil, copper, wheat) —we no longer have any slack. Almost everyone who talks about "flat earth" does not realize that it is overoptimized to the point of maximal vulnerability.

Biological systems—those that survived millions of years—include huge redundancies. Just consider why we like sexual encounters (so redundant to do it so often!). Historically populations tended to produced around 4-12 children to get to the historical average of ~2 survivors to adulthood.

Option-theoretic analysis: redundancy is like long an option. You certainly pay for it, but it may be necessary for survival.

2) Avoid prediction of remote payoffs—though not necessarily ordinary ones. Payoffs from remote parts of the distribution are more difficult to predict than closer parts.

A general principle is that, while in the first three quadrants you can use the best model you can find, this is dangerous in the fourth quadrant: no model should be better than just any model.

3) Beware the "atypicality" of remote events. There is a sucker's method called "scenario analysis" and "stress testing"—usually based on the past (or some "make sense" theory). Yet I show in the appendix how past shortfalls that do not predict subsequent shortfalls. Likewise, "prediction markets" are for fools. They might work for a binary election, but not in the Fourth Quadrant. Recall the very definition of events is complicated: success might mean one million in the bank ...or five billions!

4) Time. It takes much, much longer for a times series in the Fourth Quadrant to reveal its property. At the worst, we don't know how long. Yet compensation for bank executives is done on a short term window, causing a mismatch between observation window and necessary window. They get rich in spite of negative returns. But we can have a pretty clear idea if the "Black Swan" can hit on the left (losses) or on the right (profits).

The point can be used in climatic analysis. Things that have worked for a long time are preferable—they are more likely to have reached their ergodic states.

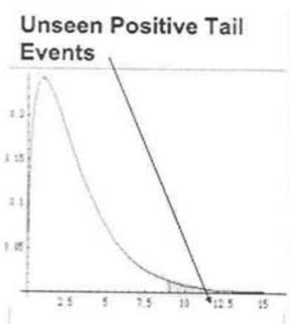
5) Beware Moral Hazard. Is optimal to make series of bonuses betting on hidden risks in the Fourth Quadrant, then blow up and write a thank you letter. Fannie Mae and Freddie Mac's Chairmen will in all likelihood keep their previous bonuses (as in all previous cases) and even get close to 15 million of severance pay each.

6) Metrics. Conventional metrics based on type 1 randomness don't work. Words like "standard deviation" are not stable and does not measure anything in the Fourth Quadrant. So does "linear regression" (the errors are in the fourth quadrant), "Sharpe ratio", Markowitz optimal portfolio, ANOVA shmnamova, Least square, etc. Literally anything mechanistically pulled out of a statistical textbook.

My problem is that people can both accept the role of rare events, agree with me, *and* still use these metrics, which is leading me to test if this is a psychological disorder.

The technical appendix shows why these metrics fail: they are based on "variance"/"standard deviation" and terms invented years ago when we had no computers. One way I can prove that anything linked to standard deviation is a facade of knowledge: There is a measure called Kurtosis that indicates departure from "Normality". It is very, very unstable and marred with huge sampling error: 70-90% of the Kurtosis in Oil, SP500, Silver, UK interest rates, Nikkei, US deposit rates, sugar, and the dollar/yet currency rate come from 1 day in the past 40 years, reminiscent of figure 3. This means that no sample will ever deliver the true variance. It also tells us anyone using "variance" or "standard deviation" (or worse making models that make us take decisions based on it) in the fourth quadrant is incompetent.

7) Where is the skewness? Clearly the Fourth Quadrant can present left or right skewness. If we suspect right-skewness, the true mean is more likely to be underestimated by measurement of past realizations, and the total potential is likewise poorly gauged. A biotech company (usually) faces positive uncertainty, a bank faces almost exclusively negative shocks. I call that in my new project "concave" or "convex" to model error.



Left	Right
Concave to model Error	Convex to model error
Ex: Banks, Short volatility, security	Example: Long options, Biotech, technology, entrepreneurship, etc.

8) Do not confuse absence of volatility with absence of risks. Recall how conventional metrics of using volatility as an indicator of stability has fooled Bernanke—as well as the banking system.

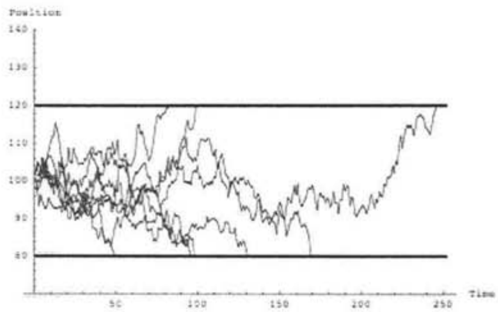


Figure 7 *Random Walk—Characterized by volatility. You only find these in textbooks and in essays on probability by people who have never really taken decisions under uncertainty.*

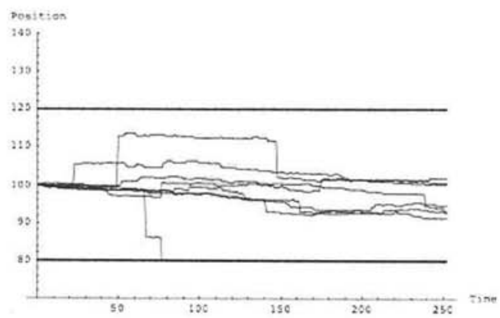


Figure 8 *Random Jump process—It is not characterized by its volatility. Its exits the 80-120 range much less often, but its extremes are far more severe. Please tell Bernanke if you have the chance to meet him.*

9) Beware presentations of risk numbers. Not only we have mathematical problems, but risk perception is subjected to framing issues that are acute in the Fourth Quadrant. Dan Goldstein and I are running a program of experiments in the psychology of uncertainty and finding that the perception of rare events is subjected to severe framing distortions: people are aggressive with risks that hit them "once every thirty years" but not if they are told that the risk happens with a "3% a year" occurrence. Furthermore it appears that risk representations are not neutral: they cause risk taking even when they are known to be unreliable.

Technical Appendix to "The Fourth Quadrant"—Click Here

Edge
THE REALITY CLUB

On "THE FOURTH QUADRANT: A MAP OF THE LIMITS OF STATISTICS"
By Nassim Nicholas Taleb

Jaron Lanier, George Dyson

...

John Brockman, Editor and Publisher
Russell Weinberger, Associate Publisher
contact: editor@edge.org

[top]

ARTICLE IN PRESS

Available online at www.sciencedirect.com

ScienceDirect

International Journal of Forecasting ■ (■■■■) ■■■–■■■

international journal of forecasting

www.elsevier.com/locate/ijforecast

Errors, robustness, and the fourth quadrant

Nassim Nicholas Taleb

New York University–Polytechnic Institute and Universa Investments, United States

Abstract

The paper presents evidence that econometric techniques based on variance – L^2 norm – are flawed and do not replicate. The result is un-computability of the role of tail events. The paper proposes a methodology to calibrate decisions to the degree (and computability) of forecast error. It classifies decision payoffs in two types: simple (true/false or binary) and complex (higher moments); and randomness into type-1 (thin tails) and type-2 (true fat tails), and shows the errors for the estimation of small probability payoffs for type 2 randomness. The fourth quadrant is where payoffs are complex with type-2 randomness. We propose solutions to mitigate the effect of the fourth quadrant, based on the nature of complex systems.

Keywords: Complexity; Decision theory; Fat tails; Risk management

1. Background and purpose

It appears scandalous that, of the hundreds of thousands of professionals involved, including prime public institutions such as the World Bank, the International Monetary Fund, different governmental agencies and central banks, private institutions such as banks, insurance companies, and large corporations, and, finally, academic departments, only a few individuals considered the possibility of the total collapse of the banking system that started in 2007 (and is still worsening at the time of writing), let alone the economic consequences of such breakdown. Not a single official forecast turned out to be close to the outcome experienced—even those issuing "warnings" did not come close to the true gravity of the situation. A few warnings about the risks, such as Taleb (2007a) or the works of the economist Nouriel Roubini,[1] went unheeded, often ridiculed.[2] Where did such sophistication go? In the face of miscalculations of such proportion, it would seem fitting to start an examination of the conventional forecasting methods for risky outcomes and assess their fragility—indeed, the size of the damage comes from confidence in forecasting and the mis-estimation of potential forecast errors for a certain classes of variables and a certain type of exposures. However, this was not

E-mail address: nnt@fooledbyrandomness.com.

[1] "Dr. Doom", *New York Times*, August 15, 2008.

[2] Note the irony that the ridicule of the warnings in Taleb (2007a) and other ideas came from the academic establishment, not from the popular press.

0169-2070/$ - see front matter © 2009 International Institute of Forecasters. Published by Elsevier B.V. All rights reserved.
doi:10.1016/j.ijforecast.2009.05.027

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

the first time such events have happened—nor was it a "Black Swan" (when capitalized, an unpredictable outcome of high impact) to the observer who took a close look at the robustness and empirical validity of the methods used in economic forecasting and risk measurement.

This examination, while grounded in economic data, generalizes to all decision-making under uncertainty in which there is a potential miscalculation of the risk of a consequential rare event. The problem of concern is the rare event, and the exposure to it, of the kind that can fool a decision maker into taking a certain course of action based on a misunderstanding of the risks involved.

2. Introduction

Forecasting is a serious professional and scientific endeavor with a certain purpose, namely to provide predictions to be used in formulating decisions, and taking actions. The forecast translates into a decision, and, accordingly, the uncertainty attached to the forecast, i.e., the error, needs to be endogenous to the decision itself. This holds particularly true of risk decisions. In other words, the use of the forecast needs to be determined — or modified — based on the estimated accuracy of the forecast. This in turn creates an interdependency about what we should or should not forecast—as some forecasts can be harmful to decision makers.

Fig. 1 gives an example of harm coming from building risk management on the basis of extrapolative (usually highly technical) econometric methods, providing decision-makers with false confidence about the risks, and leaving society exposed to several trillions in losses that put capitalism on the verge of collapse.

A key word here, *fat tails*, implies the outsized role in the total statistical properties played by one single observation—such as one massive loss coming after years of stable profits or one massive variation unseen in past data.

- "Thin-tails" lead to ease in forecasting and tractability of the errors;
- "Thick-tails" imply more difficulties in getting a handle on the forecast errors and the fragility of the forecast.

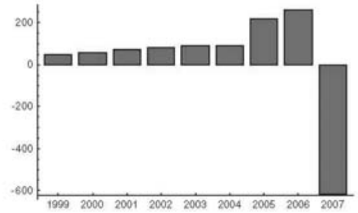


Fig. 1. Indy Mac's annual income (in millions) between 1998 and 2007. We can see fat tails at work. Tragic errors come from underestimating potential losses, with the best known cases being FNMA, Freddie Mac, Bear Stearns, Northern Rock, and Lehman Brothers, in addition to numerous hedge funds.

Close to 1000 financial institutions have shut down in 2007 and 2008 from the underestimation of outsized market moves, with losses up to 3.6 trillion.[3] Had their managers been aware of the unreliability of the forecasting methods (which were already apparent in the data), they would have requested a different risk profile, with more robustness in risk management and smaller dependence on complex derivatives.

2.1. *The smoking gun*

We conducted a simple scientific examination of economic data, using a near-exhaustive set that includes 38 "tradable" variables[4] that allow for daily prices: major equity indices across the globe (US, Europe, Asia, Latin America), most metals (gold, silver), major interest rate securities, and main currencies — what we believe represents around 98% of tradable volume.

[3] Bloomberg, Feb 5, 2009.

[4] We selected a near-exhaustive set of economic data that includes "tradable" securities that allow for a future or a forward market: most equity indices across the globe, most metals, most interest rate securities, and most currencies. We collected all available traded futures data—what we believe represents around 98% of tradable volume. The reason we selected tradable data is because of the certainty of the practical aspect of a price on which one can transact: a nontradable currency price can lend itself to all manner of manipulation. More precisely we selected "continuously rolled" futures in which the returns from holding a security are built-in. For instance, analyses of Dow Jones that fail to account for dividend payments or analyses of currencies that do not include interest rates provide a bias in the measurement of the mean and higher moments.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

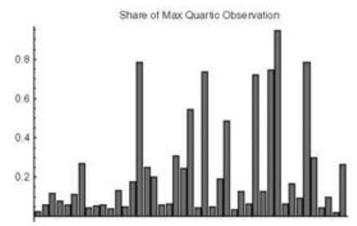


Fig. 2. The smoking gun: Maximum contribution to the fourth moment kurtosis coming from the largest observation in ~10,000 (29–40 years of daily observations) for 43 economic variables. For the Gaussian the number is expected to be ~0.006 for $n = 10,000$.

We analyzed the properties of the logarithmic returns $r_{t,\Delta t} = \mathrm{Log}\left(\frac{X_t}{X_{t-\Delta t}}\right)$, where Δt can be 1 day, 10 days, or 66 days (non-overlapping intervals).[5]

A conventional test of nonnormality used in the literature is the excess kurtosis over the normal distribution. Thus, we measured the fourth noncentral moment $k(\Delta t) = \frac{\sum r_{t,\Delta t}^4}{n}$ of the distributions and focused on the stability of the measurements.

By examining Table 1 and Figs. 2 and 3, it appears that:

(1) Economic variables (currency rates, financial assets, interest rates, commodities) are patently fat tailed—with no known exception. The literature (Bundt & Murphy, 2006) shows that this also applies to data not considered here, owing to a lack of daily changes, such as GDP, or inflation.

(2) Conventional methods, not just those relying on a Gaussian distribution, but those based on least-square methods, or using variance as a measure of dispersion, are, according to the data, incapable of tracking the kind of "fat-tails" we see (more technically, in the L^2 norm, as will be discussed in Section 5). The reason is that most of the kurtosis is concentrated in a few observations, making it practically unknowable using conventional methods—see Fig. 2. Other tests in Section 5 (the conditional expectation above a threshold) show further instability. This incapacitates least-square methods, linear regression, and similar tools, including risk management methods such as "Gaussian Copulas" that rely on correlations or any form of the product of random variables.[6, 7, 8]

[5] By convention we use $t = 1$ as one business day.

[6] This should predict, for instance, the total failure in practice of the ARCH/GARCH methods (Engle, 1982), in spite of their successes in-sample, and in academic citations, as they are based on the behavior of squares.

[7] One counterintuive result is that sophisticated operators do not seem to be aware of the norm they are using, thus mis-estimating volatility, see Goldstein and Taleb (2007).

[8] Practitioners have blamed the naive L^2 reliance on the risk management of credit risk for the blowup of banks in the crisis that started in 2007. See Felix Salmon's "Recipe For Disaster: The Formula That Killed Wall Street" in Wired. 02/23/2009.

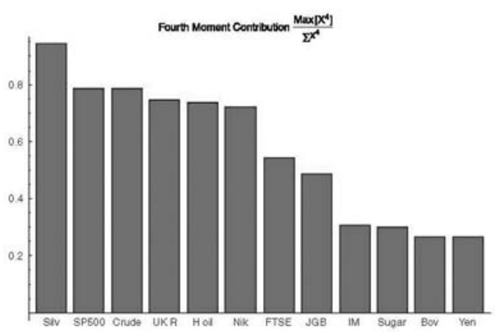


Fig. 3. A selection of the 12 most acute cases among the 43 economic variables.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

Table 1
Fourth Noncentral Moment at daily, 10 day, and 66 day windows for the random variables.

	K (1)	K (10)	K (66)	Max quartic	Years
Australian Dollar/USD	6.3	3.8	2.9	0.12	22
Australia TB 10y	7.5	6.2	3.5	0.08	25
Australia TB 3y	7.5	5.4	4.2	0.06	21
BeanOil	5.5	7.0	4.9	0.11	47
Bonds 30Y	5.6	4.7	3.9	0.02	32
Bovespa	24.9	5.0	2.3	0.27	16
British Pound/USD	6.9	7.4	5.3	0.05	38
CAC40	6.5	4.7	3.6	0.05	20
Canadian Dollar	7.4	4.1	3.9	0.06	38
Cocoa NY	4.9	4.0	5.2	0.04	47
Coffee NY	10.7	5.2	5.3	0.13	37
Copper	6.4	5.5	4.5	0.05	48
Corn	9.4	8.0	5.0	0.18	49
Crude Oil	29.0	4.7	5.1	0.79	26
CT	7.8	4.8	3.7	0.25	48
DAX	8.0	6.5	3.7	0.2	18
Euro Bund	4.9	3.2	3.3	0.06	18
Euro Currency/DEM previously	5.5	3.8	2.8	0.06	38
Eurodollar Depo 1M	41.5	28.0	6.0	0.31	19
Eurodollar Depo 3M	21.1	8.1	7.0	0.25	28
FTSE	15.2	27.4	6.5	0.54	25
Gold	11.9	14.5	16.6	0.04	35
Heating Oil	20.0	4.1	4.4	0.74	31
Hogs	4.5	4.6	4.8	0.05	43
Jakarta Stock Index	40.5	6.2	4.2	0.19	16
Japanese Gov Bonds	17.2	16.9	4.3	0.48	24
Live Cattle	4.2	4.9	5.6	0.04	44
Nasdaq Index	11.4	9.3	5.0	0.13	21
Natural Gas	6.0	3.9	3.8	0.06	19
Nikkei	52.6	4.0	2.9	0.72	23
Notes 5Y	5.1	3.2	2.5	0.06	21
Russia RTSI	13.3	6.0	7.3	0.13	17
Short Sterling	851.8	93.0	3.0	0.75	17
Silver	160.3	22.6	10.2	0.94	46
Smallcap	6.1	5.7	6.8	0.06	17
SoyBeans	7.1	8.8	6.7	0.17	47
SoyMeal	8.9	9.8	8.5	0.09	48
Sp500	38.2	7.7	5.1	0.79	56
Sugar # 11	9.4	6.4	3.8	0.3	48
SwissFranc	5.1	3.8	2.6	0.05	38
TY10Y Notes	5.9	5.5	4.9	0.1	27
Wheat	5.6	6.0	6.9	0.02	49
Yen/USD	9.7	6.1	2.5	0.27	38

(3) There is no evidence of "convergence to normality" by aggregation, i.e., looking at the kurtosis of weekly or monthly changes. The "fatness" of the tails seems to be conserved under aggregation.

Clearly, had decision-makers been aware of such facts, and such unreliability of conventional methods in tracking large deviations, fewer losses would have been incurred, as they would have reduced exposures in some areas rather than rely on more "sophisticated" methods. The financial system has been fragile, as this simple test shows, with the evidence staring at us all along.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

2.2. *The problem of large deviations*

2.2.1. *The empirical problem of small probabilities*

The central problem addressed in this paper is that small probabilities are difficult to estimate empirically (since the sample set for these is small), with a greater error rate than that for more frequent events. But since, in some domains, their effects can be consequential, the error concerning the contribution of small probabilities to the total moments of the distribution becomes disproportionately large. The problem has been dealt with by assuming a probability distribution and extrapolating into the tails—which brings model error into play. Yet, as we will discuss, model error plays a larger role with large deviations.

2.2.2. *Links to decision theory*

It is not necessary here to argue that a decision maker needs to use a full tableau of payoffs (rather than the simple one-dimensional average forecast) and that payoffs from decisions vary in their sensitivity to forecast errors. For instance, while it is acceptable to take a medicine that might be effective with a 5% error rate, but offers no side effects otherwise, it is foolish to play Russian roulette with the knowledge that one should win with a 5% error rate—indeed, standard theory of choice under uncertainty requires the use of full probability distributions, or at least a probability associated with every payoff. But so far this simple truism has not been integrated into the forecasting activity itself—as no classification has been made concerning the tractability and consequences of the errors. To put it simply, the mere separation between forecasting and decisions is lacking in both rigor and practicality, as it ruptures the link between forecast error and the quality of the decision.

The extensive literature on decision theory and choices under uncertainty so far has limited itself to (1) assuming *known* probability distributions (except for a few exceptions in which this type of uncertainty has been called "ambiguity"[9]), and (2) ignoring fat tails. This paper introduces a new structure of fat tails and classification of classes of randomness into the analysis, and focuses on the interrelation between errors and decisions. To establish a link between decision and quality of forecast, this analysis operates along two qualitative lines: qualitative differences between decisions along their vulnerability to error rates on one hand, and qualitative differences between two types of distributions of error rates. So there are two *distinct* types of decisions, and two *distinct* classes of randomness.

This classification allows us to isolate situations in which forecasting needs to be suspended—or a revision of the decision or exposure may be necessary. What we call the "fourth quadrant" is the area in which both the magnitude of forecast errors is large and the sensitivityt to these errors is consequential. What we recommend is either changes in the payoff itself (clipping exposure) or the shifting of exposures away from that part. For that we will provide precise rules.

The paper is organized as follows. First, we classify decisions according to targeted payoffs. Second, we discuss the problem of rare events, as these are the ones that are both consequential and hard to predict. Third, we present the classification of the two categories of probability distributions. Finally, we present the "fourth quadrant" and what we need to do to escape it, thus answering the call for how to handle "decision making under low predictability".

3. The different types of decisions

The first type of decisions is simple, it aims at "binary" payoffs, i.e. you just care whether something is true or false. Very true or very false does not matter. Someone is either pregnant or not pregnant. A biological experiment in the laboratory or a bet about the outcome of an election belong to this category. A scientific statement is traditionally considered "true" or "false" with some confidence interval. More technically, they depend on the zeroth moment, namely just on the probability of events, and not their magnitude —for these one just cares about "raw" probability.[10]

[9] Ellsberg's paradox, Ellsberg (1961); see also Gardenfors and Sahlin (1982) and Levi (1986).

[10] The difference can be best illustrated as follows. One of the most erroneous comparisons encountered in economics is the one between the "wine rating" and "credit rating" of complex securities. Errors in wine rating are hardly consequential for the buyer (the "payoff" is binary); errors in credit ratings have bankrupted banks, as these carry massive payoffs.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

Clearly these are not very prevalent in life—they mostly exist in laboratory experiments and in research papers.

The second type of decisions depends on more complex payoffs. The decision maker does not just care about the frequency, but about the impact as well, or, even more complex, some function of the impact. So there is another layer of uncertainty of impact. These depend on higher moments of the distribution. When one invests one does not care about the frequency, how many times he makes or loses, he cares about the expectation: how many times money is made or lost *times* the amount made or lost. We will see that there are even more complex decisions.

More formally, where $p[x]$ is the probability distribution of the random variable x, and D the domain on which the distribution is defined, the payoff $\lambda(x)$ is defined by integrating on D as:

$$\lambda(x) = \int f(x)p(x)\mathrm{d}x.$$

Note that we can incorporate utility or nonlinearities of the payoff in the function $f(x)$. But let us ignore utility for the sake of simplification.

For a simple payoff, $f(x) = 1$. So $L(x)$ becomes the simple probability of exceeding x, since the final outcome is either 1 or 0 (or 1 and -1).

For more complicated payoffs, $f(x)$ can be complex. If the payoff depends on a simple expectation, i.e., $\lambda(x) = E[x]$, the corresponding function $f(x) = x$, and we need to ignore frequencies since it is the payoff that matters. One can be right 99% of the time, but this does not matter at all, since with some skewed distributions, the consequences of the expectation of the 1% error can be too large. Forecasting typically has $f(x) = x$, a linear function of x, while measures such as least squares depend on the higher moments $f(x) = x^2$.

Note that some financial products can even depend on the fourth moment (see Table 2).[11]

Next we turn to a discussion of the problem of rare events.

4. The problem of rare events

The passage from theory to the real world presents two distinct difficulties: "inverse problems" and "pre-asymptotics".

4.1. Inverse problems

It is the greatest difficulty one can encounter in deriving properties. In real life we do not observe probability distributions, we just observe events. So we do not know the statistical properties — until, of course, after the fact — as we can see in Fig. 1. Given a set of observations, plenty of statistical distributions can correspond to the exact same realizations—each would extrapolate differently outside the set of events on which it was derived. The inverse problem is more acute when more theories, more distributions can fit a set of data—particularly in the presence of nonlinearities or nonparsimonious distributions.[12]

So this inverse problem is compounded of two problems:

+ *The small sample properties of rare events*, as these will be naturally rare in a past sample. This is also acute in the presence of nonlinearities, as the families of possible models/parametrization explode in numbers.
+ *The survivorship bias effect of high impact rare events*. For negatively skewed distributions (with a thicker left tail), the problem is worse. Clearly, catastrophic events will be necessarily absent from the data, since the survivorship of the variable itself will depend on such effect. Thus, left tailed distributions will (1) overestimate the mean; (2) underestimate the variance and the risk.

Fig. 4 shows how we normally lack data in the tails; Fig. 5 shows the empirical effect (see Fig. 6).

4.2. Pre-asymptotics

Theories can be extremely dangerous when they were derived in idealized situations, the asymptote, but are used outside the asymptote (at its limit, say infinity

[11] More formally, a linear function with respect to the variable x has no second derivative; a convex function is one with a positive second derivative. By expanding the expectation of $f(x)$ we end up with $E[f(x)] = f(x)e[\Delta x] + 1/2 f''(x)E[\Delta x2] + \cdots$, and hence higher orders matter to the extent of the importance of higher derivatives.

[12] A Gaussian distribution is parsimonious (with only two parameters to fit). But the problem of adding layers of possible jumps, each with a different probabilities, opens up endless possibilities of combinations of parameters.

Table 2
Tableau of decisions.

Mo	M1	M2+
"True/False"	Expectations	
	LINEAR PAYOFF	NONLINEAR PAYOFF
$f(x) = 0$	$f(x) = 1$	$f(x)$ nonlinear($= x^2, x^3$, etc.)
Medicine (health not epidemics)	Finance: nonleveraged investment	Derivative payoffs
Psychology experiments	Insurance, measures of expected shortfall	Dynamically hedged portfolios
Bets (prediction markets)	General risk management	Leveraged portfolios (around the loss point)
Binary/Digital derivatives	Climate	Cubic payoffs (strips of out of the money options)
Life/Death	Economics (Policy)	Errors in analyses of volatility
	Security: Terrorism, Natural catastrophes	Calibration of nonlinear models
	Epidemics	Expectation weighted by nonlinear utility
	Casinos	Kurtosis-based positioning ("volatility trading")

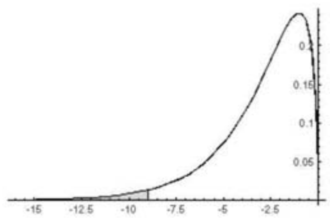


Fig. 4. The confirmation bias at work. The shaded area shows what tend to be missing from the observations. For negatively-skewed, fat-tailed distributions, we do not see much of negative outcomes for surviving entities AND we have a small sample in the left tail. This illustrates why we tend to see a better past for a certain class of time series than is warranted.

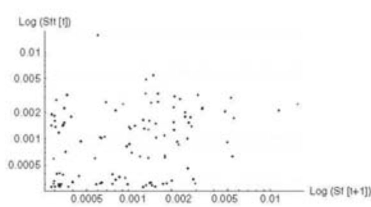


Fig. 5. Outliers don't predict outliers. The plot shows (on a logarithmic scale) a shortfall in one given year against the shortfall the following one, repeated throughout for the 43 variables. A shortfall here is defined as the sum of deviations in excess of 7%. Past large deviations do not appear to predict future large deviations, at different lags.

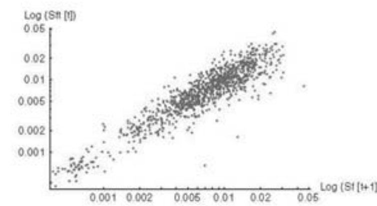


Fig. 6. Regular events predict regular events. This plot shows, by comparison with Fig. 5, how, for the same variables, the mean deviation in one period predicts the one in the subsequent period.

or the infinitesimal). Some asymptotic properties do work well pre-asymptotically (as we'll see, with type-1 distributions), which is why casinos do well, but others do not, particularly when it comes to the class of fat-tailed distributions.

Most statistical education is based on these asymptotic, laboratory-style Platonic properties—yet we take economic decisions in the real world that very rarely resembles the asymptote. Most of what students of statistics do is assume a structure, typically with a known probability. Yet the problem we have is not so much making computations once you know the probabilities as finding the true distribution.

5. The two probabilistic structures

There are two classes of probability domains — very distinct qualitatively and quantitatively — according to precise mathematical properties. The first, Type-1,

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

we call "benign" thin-tailed nonscalable, the second, Type 2, "wild" thick tailed scalable, or fractal (the attribution "wild" comes from the classification of Mandelbrot, 1963, 2001).

Taleb (2009) makes the distinction along the lines of convergence to the Central Limit Theorem. Type-1 converges in an acceptable form, while Type-2 either does not converge (infinite variance), or converges only in a remote asymptote and needs to be treated pre-asymptotically. Taleb (2009) also shows that one of the mistakes in the economics literature that "fattens the tails", with two main classes of nonparsimonious models and processes (the jump-diffusion processes of Merton, 1976,[13] or stochastic volatility models such as Engels' ARCH[14]) is to believe that the second type of distribution is amenable to analyses like the first—except with fatter tails. In reality, a fact commonly encountered by practitioners is that fat-tailed distributions are very unwieldy—as we can see in Fig. 2. Furthermore, we often face a problem of mistaking one for the other: a process that is extremely well behaved, but, on occasions, delivers a very large deviation, can easily be mistaken for a thin-tailed one—a problem known as the "problem of confirmation" (Taleb, 2007a,b). So we need to be suspicious of the mistake of taking Type-2 for Type-1, as it is more severe (and more readily made) than the one in the other direction.[15]

As we saw from the data presented, this classification of "fat tails" does not just mean having a fourth moment worse than the Gaussian. The Poisson distribution, or a mixed distribution with a known Poisson jump, would have tails thicker than the Gaussian; but this mild form of fat tails can be dealt with rather easily—the distribution has all its moments finite. The problem comes from the structure of the decline in probabilities for larger deviations and the ease with which the tools at our disposal can be tripped into producing erroneous results from observations of data in a finite sample and jumping to wrong decisions.

[13] See the general decomposition into diffusion and jump (non-scalable) in Duffie, Pan, and Singleton (2000) and Merton (1976); and the discussion in Baz and Chacko (2004) and Haug (2007).

[14] Engle (1982).

[15] Makridakis et al. (1993) and Makridakis and Hibon (2000) present evidence that more complicated methods of forecasting do not deliver superior results to simple ones (already bad). The obvious reason is that the errors in calibration swell with the complexity of the model.

5.1. The scalable property of type-2 distributions

Take a random variable x. With scalable distributions, asymptotically, for x large enough (i.e. "in the tails"), $\frac{P[X>nx]}{P[X>x]}$ depends on n, not on x (the same property can hold for $P[X < nx]$ for negative values). This induces statistical self-similarities. Note that owing to the finiteness of the realizations of random variables, and the lack of samples in the tails, we might not be able to observe such a property, yet not be able to rule out.

For economic variables, there is no fundamental reason for the ratio of "exceedances" (i.e., the cumulative probability of exceeding a certain threshold) to decline, as both the numerator and the denominators are multiplied by 2.

This self-similarity at all scales generates power-law, or Paretian, tails, i.e., above a crossover point, $P[X > x] = Kx^{-\alpha}$.[16, 17]

Let us now draw the implications of type-2 distributions.

5.1.1. Finiteness of moments and higher order effects

For thick tailed distributions, moments higher than α are not "finite", i.e., they cannot be computed. They can certainly be measured in finite samples—thus giving the illusion of finiteness. But they typically show a great degree of instability. For instance, a distribution with an infinite variance will always provide, in a sample, the illusion of finiteness of variance.

In other words, while errors converge for type-1 distributions, the expectations of higher orders of x, say of order n, such as $1/n!E[x^n]$, where x is the error, do not decline; in fact, they become explosive (see Fig. 7).

[16] Scalable discussions: introduced by Mandelbrot (1963), Mandelbrot (1997) and Mandelbrot and Taleb (in press).

[17] Complexity and power laws: Amaral et al. (1997), Sornette (2004), and Stanley, Amaral, Gopikrishnan, and Plerou (2000); for scalability in different aspects of financial data, Gabaix, Gopikrishnan, Plerou, and Stanley (2003a,b), Gabaix, Ramalho, and Reuter (2003c), Gopikrishnan, Meyer, Amaral, and Stanley (1998), Gopikrishnan, Plerou, Amaral, Meyer, and Stanley (1999), and Gopikrishnan, Plerou, Gabaix, and Stanley (2000). For the statistical mechanics of scale-free networks see Albert, Jeong, and Barabási (2000), Albert and Barabasi (2002) and Barabási and Albert (1999). The "sandpile effect" (i.e., avalanches and cascades) is discussed by Bak (1996) and Bak, Tang, and Wiesenfeld (1987, 1988), as power laws arise from conditions of self-organized criticality.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

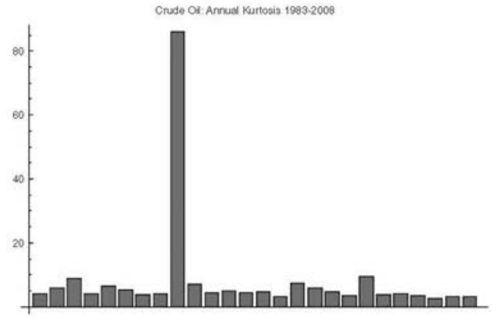


Fig. 7. Kurtosis over time: example of an "infinite moment". The graph shows the fourth moment for crude oil in annual nonoverlapping observations between 1982 and 2008. The instability shows in the dependence of the measurement on the observation window.

5.1.2. "Atypicality" of moves

For thin tailed domains, the conditional expectation of a random variable X, conditional on its exceeding a number K, converges to K for larger values of K.

$$\lim_{K\to\infty} E[X|_{X>K}] = K.$$

For instance, the conditional expectation for a Gaussian variable (assuming a mean of 0) conditional on the variable exceeding 0 is approximately 0.8 standard deviations. But with K equals 6 standard deviations, the conditional expectation converges to 6 standard deviations. The same applies to all of the random variables that do not have a Paretian tail. This induces some "typicality" of large moves.

For fat tailed variables, such a limit does not seem to hold:

$$\lim_{K\to\infty} E[X|_{X>K}] = Kc,$$

where c is a constant. For instance, the conditional expectation of a market move, given that it is in excess of 3 mean deviations, will be around 5 mean deviations. The expectation of a move conditional on it being higher than 10 mean deviations will be around 18. This property is quite crucial.

The atypicality of moves has the following significance.

- One may correctly predict a given event, say, a war, a market crash, or a credit crisis. But the amplitude of the damage will be unpredicted. The open-endedness of the outcomes can cause a severe miscalculation of the expected payoff function. For instance, the investment bank Morgan Stanley predicted a credit crisis but was severely hurt (and needed to be rescued) because it did not anticipate the extent of the damage.
- Methods like Value-at-Risk[18] that may correctly compute, say, a 99% probability of not losing no more than a given sum, called "value-at-risk", will nevertheless miscompute the conditional expectation should such a threshold be exceeded. For instance, one has 99% probability of not exceeding a $1 million loss, but should such a loss occur, it can be $10 million or $100 million.

This lack of typicality is of some significance. Stress testing and scenario generation are based on assuming a "crisis" scenario and checking robustness to it. Unfortunately such luxury is not available for fat tails, as "crises" do not have a typical magnitude.

Tables 3 and 4 show the evidence of a lack of convergence to thin tails, and hence a lack of "typicality" of the moves. We stopped for segments for which the number of observations becomes small, since a lack of observations in the tails can provide the illusion of "thin" tails.

[18] For the definition of Value at Risk see, Jorion (2001); for a critique, see Joe Nocera, "Risk Mismanagement: What led to the Financial Meltdown", *New York Time Magazine*, Jan 2, 2009.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

Table 3
Conditional expectation for moves > K, 43 economic variables.

K, Mean deviations	Mean move (in MAD) in excess of K	n
1	2.01443	65,958
2	3.0814	23,450
3	4.19842	8,355
4	5.33587	3,202
5	6.52524	1,360
6	7.74405	660
7	9.10917	340
8	10.3649	192
9	11.6737	120
10	13.8726	84
11	15.3832	65
12	19.3987	47
13	21.0189	36
14	21.7426	29
15	24.1414	21
16	25.1188	18
17	27.8408	13
18	31.2309	11
19	35.6161	7
20	35.9036	6

Table 4
Conditional expectation for moves < K, 43 economic variables.

K, Mean deviations	Average move (in MAD) below K	n
−1	−2.06689	62,803
−2	−3.13423	23,258
−3	−4.24303	8,676
−4	−5.40792	3,346
−5	−6.66288	1,415
−6	−7.95766	689
−7	−9.43672	392
−8	−11.0048	226
−9	−13.158	133
−10	−14.6851	95
−11	−17.02	66
−12	−19.5828	46
−13	−21.353	38
−14	−25.0956	27
−15	−25.7004	22
−16	−27.5269	20
−17	−33.6529	16
−18	−35.0807	14
−19	−35.5523	13
−20	−38.7657	11

5.1.3. Preasymptotics

Even if we eventually converge to a probability distribution of the kind well known and tractable, it is central that the time to convergence plays a large role.

For instance, much of the literature invokes the Central Limit Theorem to assume that fat-tailed distributions with a finite variance converge to a Gaussian under summation. If daily errors are fat-tailed, cumulative monthly errors will become Gaussian. In practice, this does not appear to hold. The data, as we saw earlier, show that economic variables do not remotely converge to the Gaussian under aggregation.

Furthermore, finiteness of variance is a necessary but highly insufficient condition. Bouchaud and Potters (2003) showed that the tails remain heavy while the body of the distribution becomes Gaussian (see Fig. 8).

5.1.4. Metrics

Much of time series work seems to be based on metrics which are in the square domain, and hence patently intractable. Define the norm L^p:

$$\left(\frac{1}{n}\sum |x|^p\right)^{\frac{1}{p}};$$

it will increase along with p. The numbers can become explosive, with rare events taking a disproportionately larger share of the metric at higher orders of p. Thus the variance/standard deviation ($p = 2$), as a measure of dispersion, will be far more unstable than mean deviation ($p = 1$). The ratio of mean-deviation to variance (Taleb, 2009) is highly unstable for economic variables. Thus, modelizations based on variance become incapacitated. More practically, this means that for distributions with a finite mean (tail exponent greater than 1), the mean deviation is more "robust".[19]

[19] A note on the weaknesses of nonparametric statistics: the mean deviation is often used as a robust, nonparametric or distribution-free statistic. It does work better than the variance, as we saw, but does not contain information on rare events, by the argument seen before. Likewise, nonparametric statistical methods (relying on the empirical frequency) will be extremely fragile to the "black swan problem", since the absence of large deviations in the past leave us in a near-total opacity about their occurrence in the future—as we saw in Fig. 4, these are confirmatory. In other words, nonparametric statistics that consist of fitting a kernel to empirical frequencies, assume, even more than other methods, that a large deviation will have a predecessor.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

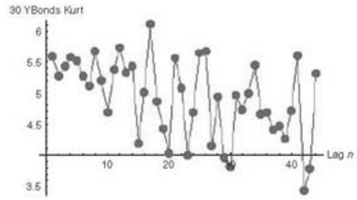


Fig. 8. Behavior of kurtosis under aggregation: we lengthen the window of changes from 1 day to 50 days. Even for variables with an infinite fourth moment, the kurtosis tends to drop under aggregation in small samples, then rise abruptly after a large observation.

5.1.5. *Incidence of rare events*

One common error is to believe that thickening the tails leads to an *increase* of the probability of rare events. In fact, it usually leads to a decrease of the incidence of such events, but the magnitude of the event, should it happen, will be much larger.

Take, for instance, a normally distributed random variable. The probability of exceeding 1 standard deviation is about 16%. Observed returns in the markets, with a higher kurtosis, present a lower probability of exceeding the same threshold, around 7%–10%, but the depth of the excursions is greater.

5.1.6. *Calibration errors and fat tails*

One does not need to accept power laws to use them. A convincing argument is that if we don't know what a "typical" event is, fractal power laws are the most effective way to *discuss* the extremes mathematically. It does not mean that the real world generator is actually a power law—it means that we don't understand the structure of the external events it delivers and need a tool of analysis. Also, fractals simplify the mathematical discussions because all you need to do is to perturbate one parameter, here the α, and it increases or decreases the role of the rare event in the total properties.

Say, for instance, that, in an analysis, you move α from 2.3 to 2 for data in the publishing business; the sales of books in excess of 1 million copies would triple! This method is akin to generating combinations of scenarios with series of probabilities and series of payoffs, fattening the tail at each time.

The following argument will help illustrate the general problem with forecasting under fat tails. Now the problem: *Parametrizing a power law lends itself to extremely large estimation errors* (since heavy tails have inverse problems). Small changes in the α main parameter used by power laws lead to extremely large effects in the tails.

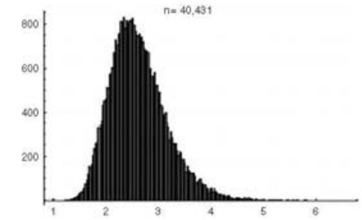


Fig. 9. Estimation error from 40 thousand economic variables.

And we don't observe the α—an uncertainty that comes from the measurement error. Fig. 9 shows more than 40 thousand computations of the tail exponent α from different samples of different economic variables (data for which it is impossible to refute fractal power laws). We clearly have problems figuring out what the α is: our results are marred by errors. The mean absolute error in the measurement of the tail exponent is in excess of 1 (i.e. between $\alpha = 2$ and $\alpha = 3$). Numerous papers in econophysics found an "average" alpha between 2 and 3—but if you process the >20 million pieces of data analyzed in the literature, you find that the variations between single variables are extremely significant.[20]

Now this mean error has massive consequences. Fig. 10 shows the effect: the expected value of your losses in excess of a certain amount (called the "shortfall") is multiplied by >10 from a small change in the α that is less than its mean error.[21]

[20] One aspect of this inverse problem is even pervasive in Monte Carlo experiments (much better behaved than the real world), see Weron (2001).

[21] Note that the literature on extreme value theory (Embrechts, Klüppelberg, & Mikosch, 1997) does not solve much of the problem, as the calibration errors stay the same. The argument about calibration we saw earlier makes the values depend on the unknowable tail exponent. This calibration problem explains how Extreme Value Theory works better on computers than in the real world (and has failed completely in the economic crisis of 2008–2009).

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

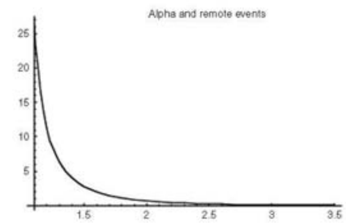


Fig. 10. The value of the expected shortfall (expected losses in excess of a certain threshold) in response to changes in the tail exponent α. We can see it explode by an order of magnitude.

6. The map

First quadrant: Simple binary decisions, under type-1 distributions: forecasting is safe. These situations are, unfortunately, more common in laboratories and games than in real life. We rarely observe these in payoffs in economic decision making. Examples: some medical decisions, casino bets, prediction markets.

Second quadrant: Complex decisions under type-1 distributions: Statistical methods may work satisfactorily, though there are some risks. True, thin-tails may not be a panacea, owing to preasymptotics, lack of independence, and model error. There are clearly problems there, but these have been addressed extensively in the literature (see Freedman, 2007).

Third quadrant: Simple decisions, under type-2 distributions: there is little harm in being wrong—the tails do not impact the payoffs.

Fourth quadrant: Complex decisions under type-2 distributions: this is where the problem resides. We need to avoid the prediction of remote payoffs—though not necessarily ordinary ones. Payoffs from remote parts of the distribution are more difficult to predict than closer parts.

A general principle is that, while in the first three quadrants you can use *the best* model you can find, this is dangerous in the fourth quadrant: no model should be better than just any model. So the idea is to exit the fourth quadrant.

The recommendation is to move into the third quadrant—it is not possible to change the distribution; but it is possible to change the payoff, as will be discussed in Section 7 (see Table 5).

The subtlety is that, while we have a poor idea about the expectation in the 4th quadrant, exposures to rare events are not symmetric.

7. Decision-making and forecasting in the fourth quadrant

7.1. Solutions by changing the payoff

Finally, the main idea proposed in this paper is to endogenize decisions, i.e., escape the 4th quadrant whenever possible by changing the payoff in reaction to the high degree of unpredictability and the harm it causes. How?

Just consider that the property of "atypicality" of the moves can be compensated by truncating the payoffs, thus creating an organic "worst case" scenario that is resistant to forecast errors. Recall that a binary payoff is insensitive to fat tails precisely because above a certain level, the domain of integration, changes in probabilities do not impact the payoff. So making the payoff no longer open-ended mitigates the problems, thus making it more tractable mathematically.

A way to express it using moments: all moments of the distribution become finite in the absence of open-ended payoffs, by putting a floor L below which $f(x) = 0$, as well a ceiling H. Just consider that if you are integrating payoffs in a finite, rather than an open-ended domain, i.e. between L and H, respectively, *the tails of the distributions outside that domain no longer matter*. Thus the domain of integration becomes the domain of payoff.

$$\lambda(x) = \int_L^H f(x)\, p(x) \mathrm{d}x.$$

With an investment portfolio, for instance, it is possible to "put a floor" on the payoff using insurance, or, even better, by changing the allocation. Insurance products are tailored with a maximum payoff; catastrophe insurance products are also set with a "cap", though the cap might be high enough to allow for a dependence on the error of the distribution.[22]

[22] Insurance companies might cap the payoff of a single claim, but a collection of capped claims might represent some problems, as the maximum loss becomes so large as to be almost undistinguishable from that with an uncapped payoff.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

Table 5
The four quadrants.

	Simple payoffs	Complex payoffs
Distribution 1 ("thin tailed")	First quadrant: Extremely safe	Second quadrant: Safe
Distribution 2 (no or unknown characteristic scale)	Third quadrant: Safe	Fourth quadrant: Dangers[a]

[a] The dangers are limited to exposures in the negative domain (i.e., adverse payoffs). Some exposures, we will see, can only be "positive".

7.1.1. *The effect of skewness*

We omitted earlier to discuss asymmetry in either the payoff or the distribution. Clearly, the fourth quadrant can present either left or right skewness. If we suspect right-skewness, the true mean is more likely to be underestimated by the measurement of past realizations, and the total potential is likewise poorly gauged. A biotech company (usually) faces positive uncertainty, a bank faces almost exclusively negative shocks.

More significantly, by raising the L (the lower bound), one can easily produce positive skewness, with a set floor for potential adverse outcomes and open upside. For instance, what Taleb (2007a) calls a "barbell" investment strategy consists of allocating a high portion of a portfolio to T-Bills (or equivalent), say α, with $0 < \alpha < 1$, and a small portion $(1 - \alpha)$ to high-variance securities. While the total portfolio has medium variance, $L = (1 - \alpha)$ times the face value invested, another portfolio of the same variance might lose 100%.

7.1.2. *Convex and concave to error*

If a source of uncertainty can offer more benefits than a potential harm, then there may be gains from it—which we label "convex" or "concave".

More generally, we can be concave to model error if the payoff from the error (obtained by changing the tails of the distribution) has a negative second derivative with respect to the change in the tails, or is negatively skewed (like the payoff of a short option). It will be convex if the payoff is positively skewed (like the payoff of a long option).

7.1.3. *The effect of leverage in operations and investment*

Leveraging in finance has the effect of increasing concavity to model error. As we will see, it is exactly the opposite of redundancy—it causes payoffs to increase, but at the costs of an absorbing barrier should there be an extreme event that exceeds the allowance made in the risk measurement. Redundancy, on the other hand, is the equivalent of de-leveraging, i.e. by having more idle "inefficient" capital on the side. But a a second look at such funds can reveal that there may be a direct expected value from being able to benefit from opportunities in the event of asset deflation, and hence "idle" capital needs to be analyzed as an option.

7.2. *Solutions by mitigating forecasting errors*

7.2.1. *Optimization vs. redundancy*

The optimization paradigm of the economics literature meets some problems in the fourth quadrant: what if we have a consequential forecasting error? Aside from the issue that the economic agent is optimizing on the future states of the world, with a given probability distribution, nowhere[23] have the equations taken into account the possibility of a large deviation that would allow *not* optimizing consumption and having idle capital. Also, the psychological literature on well-being (Kahneman, 1999) shows an extremely concave utility function of income—if one spends such income. But if one hides it under the mattress, one will be less vulnerable to an extreme event. So there is an enhanced survival probability for those who have additional margin.

While economics have been mired in conventional linear analysis, stochastic optimization with Bellman-style equations that fall into the category Type-1, a different point of view is provided by complex systems analysis. One of the central attributes of complex systems is redundancy (May, Levin, & Sugihara, 2008).

[23] See Merton (1992) for a discussion of the general consumption Capital Asset Pricing Market.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

Biological systems — those that have survived millions of years — include a large share of redundancies.[24, 25] Just consider the number of double organs (lungs, kidneys, ears). This may suggest an option-theoretic analysis: redundancy is like an option. One certainly pays for it, but it may be necessary for survival. And while redundancy means similar functions used by identical organs or resources, biological systems have, in addition, recourse to "degeneracy", the possibility of one organ to perform more than one function, which is the analog of redundancy at a functional level (Edelman & Gally, 2001).

When institutions such as banks optimize, they often do not realize that a simple model error can blow through their capital (as it just did) (see Fig. 11).

Examples: In one day in August 2007, Goldman Sachs experienced 24 times the average daily transaction volume[26]—would 29 times have blown up the clearing system? Another severe instance of an extreme "spike" lies in an event of September 18, 2008, in the aftermath of the Lehman Bothers Bankruptcy. According to congress documents, only made public in February 2009:

> On Thursday (Sept 18), at 11 am the Federal Reserve noticed a tremendous draw-down of money market accounts in the US, to the tune of $550 billion[27] was being drawn out in the matter of an hour or two.
>
> If they had not done that [add liquidity], their estimation is that by 2 pm that afternoon, $5.5 trillion would have been drawn out of the money market system of the U.S., which would have collapsed the entire economy of the U.S., and within 24 h the world economy would have collapsed. It would have been the end of our economic system and our political system as we know it.[28]

For naive economics, the best way to effectively reduce costs is to minimize redundancy, and hence avoiding the option premium of insurance. Indeed, some systems tend to optimize and therefore become more fragile. Albert and Barabasi (2002) and Barabási and Albert (1999) warned (ahead of the North Eastern power outage of August 2003) how electricity grids, for example, optimize to the point of not coping with unexpected surges—which predicted the possibility of a blackout of the magnitude of the one that took place in the North Eastern U.S. in August 2003. We cannot discuss "flat earth" globalization without realizing that it is overoptimized to the point of maximal vulnerability.

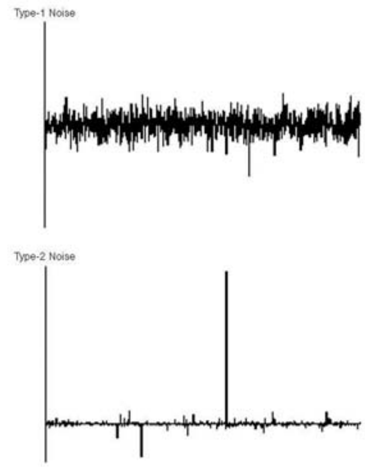


Fig. 11. Comparison between Gaussian-style noise and Type-2 noise with extreme spikes—which necessitates more redundancy (or insurance) than normally required. Policymakers and forecasters were not aware that complex systems tend to produce the second type of noise.

7.2.2. *Time and sample size*

It takes much, much longer for a fat-tailed time series to reveal its properties—in fact, many can, in short episodes, masquerade as thin-tailed. At the worst, we don't know how long it would take to know. But we can have a pretty clear idea whether *organically*, because of the nature of the payoff, the "Black Swan" can hit on the left (losses) or on the

[24] May et al. (2008).

[25] For the scalability of biological systems, see Burlando (1993), Enquist and Niklas (2001), Harte, Kinzig, and Green (1999), Ritchie and Olff (1999) and Solé, Manrubia, Benton, Kauffman, and Bak (1999).

[26] Personal communication, Pentagon Highland Forum, April meeting, 2008.

[27] Even if the number, as is possible, is off by one order of magnitude, the consequences remain extremely severe.

[28] http://www.liveleak.com/view?i=ca2_1234032281.

Please cite this article in press as: Taleb, N. N. Errors, robustness, and the fourth quadrant. International Journal of Forecasting (2009), doi:10.1016/j.ijforecast.2009.05.027

right (profits). This point can be used in climatic analysis. Things that have worked for a long time are preferable—they are more likely to have reached their ergodic states.

Likewise, portfolio diversification needs to be larger, much larger than anticipated. A mean variance Markowitz-style portfolio construction fails in the real world on several accounts. Taleb (2009) shows that, even if we assume finite variance, but fat tails and an unknown variance, the process of discovery of the variance itself makes portfolio theory totally unusable. DeMiguel, Garlappi, and Uppal (2007) show that a naive $1/n$ allocation outperforms out-of-sample any form of "optimal" portfolio—compatible with the notion that fat tails (and unknown future properties from past samples) require much broader diversification than is required by modern portfolio theory.

7.2.3. *The problem of moral hazard*

It is optimal (both economically and psychologically) to make a series of annual bonuses betting on hidden risks in the fourth quadrant, then "blow up" (Taleb, 2004). The problem is that bonus payments are made with a higher frequency (i.e. annually) than is warranted from the statistical properties (when it takes longer to capture the statistical properties).

7.2.4. *Metrics*

Conventional metrics based on type 1 randomness fail to produce reliable results—while the economics literature is grounded in them. Concepts like "standard deviation" are not stable and do not measure anything in the fourth quadrant. This is also true for "linear regression" (the errors are in the fourth quadrant), "Sharpe ratio", the Markowitz optimal portfolio,[29] ANOVA, Least squares, etc. "Variance" and "standard deviation" are terms invented years ago when we had no computers. Note that from the data shown and the instability of the kurtosis, no sample will ever deliver the true variance in a reasonable time. However, note that truncating payoffs blunts the effects of the inadequacy of the metrics.

[29] The framework of Markowitz (1952), as it is built on the L^2 norm, does not stand any form of empirical or even theoretical validity, owing to the dominance of higher moment effects, even in the presence of "finite" variance, see Taleb (2009).

8. Conclusion

To conclude, we offered a method of robustifying payoffs from large deviations and making forecasts possible to perform. The extensions can be generalized to a larger notion of society's safety—for instance how we should build systems (internet, banking structure, etc.) to be impervious to random effects.

Acknowledgements

A longer literary version of the ideas of this paper was posted on the web on the EDGE website at www.edge.org, eliciting close to 600 comments and letters, which helped in the elaboration of this version. The author thanks the commentators and various reviewers, and Yossi Vardi for the material on the events of Sept 18, 2008.

References

Albert, R., & Barabasi, A.-L. (2002). Statistical mechanics of complex network. *Review of Modern Physics*, *74*, 47–97.

Albert, R., Jeong, H., & Barabási, A.-L. (2000). Error and attack tolerance of complex networks. *Nature*, *406*, 378–382.

Amaral, L. A. N., Buldyrev, S. V., Havlin, S., Leschhorn, H., Maass, P., Salinger, M. A., et al. (1997). Scaling behavior in economics: I. Empirical results for company growth. *Journal de Physique I (France)*, *7*, 621.

Bak, P. (1996). *How nature works*. New York: Copernicus.

Bak, P., Tang, C., & Wiesenfeld, K. (1987). Self-organized criticality: An explanation of 1/f noise. *Physical Review Letters*, *59*, 381–384.

Bak, P., Tang, C., & Wiesenfeld, K. (1988). Self-organized criticality. *Physical Review A*, *38*, 364–374.

Barabási, A.-L., & Albert, R. (1999). Emergence of scaling in random networks. *Science*, *286*, 509–511.

Baz, J., & Chacko, G. (2004). *Financial derivatives: Pricing, applications, and mathematics*. Cambridge University Press.

Bouchaud, J.-P., & Potters, M. (2003). *Theory of financial risks and derivatives pricing, from statistical physics to risk management* (2nd ed.). Cambridge University Press.

Bundt, T., & Murphy, R. P. (2006). *Are changes in macroeconomic variables normally distributed? Testing an assumption of neoclassical economics*. Preprint, NYU Economics Department.

Burlando, B. (1993). The fractal geometry of evolution. *Journal of Theoretical Biology*, *163*(2), 161–172.

DeMiguel, Garlappi, & Uppal, (2007). *Optimal versus naive diversification: How inefficient is the 1/n portfolio strategy?* Working Paper.

Duffie, D., Pan, J., & Singleton, K. (2000). Transform analysis and asset pricing for affine jump diffusions. *Econometrica*, *68*, 1343–1376.

Edelman, G. M., & Gally, J. A. (2001). Degeneracy and complexity in biological systems. *Proceedings of the Nationall Academy of Sciences of the United States of America*, *98*(24), 13763–13768.

Ellsberg, D. (1961). Risk, ambiguity, and the savage axioms. *Quarterly Journal of Economics*, *75*, 643–669.

Embrechts, P., Klüppelberg, C., & Mikosch, T. (1997). *Modelling extremal events for insurance and finance*. Berlin: Spring Verlag.

Engle, R. F. (1982). Autoregressive conditional heteroscedasticity with estimates of the variance of United Kingdom. *Econometrica*, *50*(4), 987–1007.

Enquist, B. J., & Niklas, K. J. (2001). Invariant scaling relations across tree-dominated communities. *Nature*, *410*(6829), 655–660.

Freedman, D. (2007). *Statistics* (4th ed.). W.W. Norton & Company.

Gabaix, X., Gopikrishnan, P., Plerou, V., & Stanley, H. E. (2003a). A theory of power law distributions in financial market fluctuations. *Nature*, *423*, 267–230.

Gabaix, X., Gopikrishnan, P., Plerou, V., & Stanley, H. E. (2003b). *Are stock market crashes outliers?* Mimeo 43.

Gabaix, X., Ramalho, R., & Reuter, J. (2003c). *Power laws and mutual fund dynamics*. MIT mimeo.

Gardenfors, P., & Sahlin, N. E. (1982). Unreliable probabilities, risk taking, and decision making. *Syn-these*, *53*, 361–386.

Goldstein, D. G., & Taleb, N. N. (2007). We don't quite know what we are talking about when we talk about volatility. *Journal of Portfolio Management*, *33*(4), 84–86.

Gopikrishnan, P., Meyer, M., Amaral, L., & Stanley, H. E. (1998). Inverse cubic law for the distribution of stock price variations. *European Physical Journal B*, *3*, 139–140.

Gopikrishnan, P., Plerou, V., Amaral, L., Meyer, M., & Stanley, H. E. (1999). Scaling of the distribution of fluctuations of financial market indices. *Physical Review E*, *60*, 5305–5316.

Gopikrishnan, P., Plerou, V., Gabaix, X., & Stanley, H. E. (2000). Statistical properties of share volume traded in financial markets. *Physical Review E*, *62*, R4493–R4496.

Harte, J., Kinzig, A., & Green, J. (1999). Self-similarity in the distribution and abundance of species. *Science*, *284*(5412), 334–336.

Haug, E. G. (2007). *Derivatives models on models*. New York: John Wiley & Sons.

Jorion, P. (2001). *Value-at-risk: The new benchmark for managing financial risk*. McGraw Hill.

Kahneman, D. (1999). Objective happiness. In D. Kahneman, E. Diener, & N. Schwartz (Eds.), *Well being: Foundations of hedonic psychology*. New York: Russell Sage Foundation.

Levi, I. (1986). The paradoxes of Allais and Ellsberg. *Economics and Philosophy*, *2*, 23–53.

Makridakis, S., & Hibon, M. (2000). The M3-competition: Results, conclusions and implications. *International Journal of Forecasting*, *16*, 451–476.

Makridakis, S., Chatfield, C., Hibon, M., Lawrence, M., Mills, T., Ord, K., et al. (1993). The M2-competition: A real-time judgmentally based forecasting study (with commentary). *International Journal of Forecasting*, *9*, 5–29.

Mandelbrot, B. (1963). The variation of certain speculative prices. *The Journal of Business*, *36*(4), 394–419.

Mandelbrot, B. (1997). *Fractals and scaling in finance*. Springer-Verlag.

Mandelbrot, B. (2001). *Quantitative Finance*, *1*, 113–123.

Mandelbrot, B., & Taleb, N. N. (2009). Mild vs. wild randomness: Focusing on risks that matter. In F. Diebold, N. Doherty, R. Herring (Eds.), *The known, the unknown and the unknowable in financial institutions*. Princeton, NJ: Princeton University Press (in press).

Markowitz, H. (1952). Portfolio Selection. *Journal of Finance*, *7*, 77–91.

May, R. M., Levin, S. A., & Sugihara, G. (2008). Complex systems: Ecology for bankers. *Nature*, *451*, 893–895.

Merton, R. C. (1976). Option pricing when underlying stock returns are discontinuous. *Journal of Financial Economics*, *3*, 125–144.

Merton, R. C. (1992). *Continuous-time finance* (revised edition). Blackwell.

Ritchie, M. E., & Olff, H. (1999). Spatial scaling laws yield a synthetic theory of biodiversity. *Nature*, *400*(6744), 557–560.

Solé, R. V., Manrubia, S. C., Benton, M., Kauffman, S., & Bak, P. (1999). Criticality and scaling in evolutionary ecology. *Trends in Ecological Evolution*, *14*(4), 156–160.

Sornette, D. (2004). *Critical phenomena in natural sciences: Chaos, fractals, self-organization and disorder: Concepts and tools* (2nd ed.). Berlin, Heidelberg: Springer.

Stanley, H. E., Amaral, L. A. N., Gopikrishnan, P., & Plerou, V. (2000). Scale invariance and universality of economic fluctuations. *Physica A*, *283*, 31–41.

Taleb, N. N. (2004). The practitioner's perspective: Bleed or blowup? Why do we prefer asymmetric payoffs? *Journal of Behavioral Finance*, *5*(1), 2–7.

Taleb, N. (2007a). *The black swan: The impact of the highly improbable*. US: Random House, Penguin (UK).

Taleb, N. N. (2007b). Black swans and the domains of statistics. *The American Statistician*, *61*(3), 198–200.

Taleb, N. N. (2009). Finiteness of variance is irrelevant in the practice of quantitative finance. *Complexity*, *14*(3), 66–76.

Weron, R. (2001). Levy-stable distributions revisited: Tail index >2 does not exclude the Levy-stable regime. *International Journal of Modern Physics*, *12*(2), 209–223.

BIOGRAPHY FOR NASSIM N. TALEB

Nassim N. Taleb is currently Distinguished Professor in Risk Engineering at New York University Polytechnic Institute and Principal at Universa Investments. He spent close to 21 years as a senior trader on Wall Street before becoming a full time scholar. He is a combination of a scholar of risk and model error, literary essayist, and derivatives trader. He is known for a multi-disciplinary approach to the role of the high-impact rare event—across economics, philosophy, finance, engineering, and history. He also runs experiments on human errors in the assessment of probabilities of rare events as part of the Decision Science Laboratory. His current program is to design ways to live in a world we don't quite understand and help "robustify" the world against the Black Swan.

Taleb is, among other books and research papers, the author of the NYT Bestseller *The Black Swan: The Impact of the Highly Improbable* which was according to *The Times* as one of the 12 most influential books since WW-II. His books have close to two and a half million copies in print in 31 languages.

Taleb has an MBA from Wharton and a Ph.D. from the University of Paris.

Among other activities, he is currently on the King of Sweden advisory committee for climate risks and modeling. The British Tory opposition is using Black Swan thinking as part of their platform.

Chairman MILLER. Thank you, Dr. Taleb.

Dr. Bookstaber for five minutes.

STATEMENT OF DR. RICHARD BOOKSTABER, FINANCIAL AUTHOR

Dr. BOOKSTABER. Mr. Chairman and Members of the Committee, I thank you for the opportunity to testify today. My oral testimony will begin with a discussion of the limitations of VaR. I will then discuss the role of VaR in the recent market meltdown and conclude with suggestions for filling the gap left by the limitations of VaR.

The limitations of VaR are readily apparent by looking at the critical assumptions behind it. For the standard construction of VaR, these assumptions are, first, that all portfolio positions are included; secondly, that the sample history used in VaR is a reasonable representation of things that are likely to occur going forward; and third, that the normal distribution function that it uses is a reasonable representation of the statistical distribution underlying the returns. These assumptions are often violated, leading VaR estimates to be misleading. So let me discuss each of these in turn.

First of all, in terms of incomplete positions, obviously, for risk to be measured, all the risky positions must be included in the analysis, but for larger institutions, it is commonplace for some positions to be excluded. This can happen because the positions are held off a balance sheet beyond the purview of those doing the risk analysis, because they are in complex instruments that have not been sufficiently modeled, or because they are in new so-called innovative products that have yet to be added into the risk process. This provides a compelling reason to have what I call 'flight to simplicity' in financial products, to move away from complex and customized innovative products and towards standardization.

In terms of unrepresentative sample periods, VaR gives a measure of risk that assumes tomorrow is drawn from the same distribution as the sample data used to compute the VaR. If the future does not look like the past—in particular, if a crisis emerges, VaR will no longer be a good measure of risk, which is to say that VaR is a good measure of risk except when it really matters.

Third, in terms of fat tails and normal distribution, largely because of crisis events, security returns tend to have fatter tails than what is represented by a normal distribution. That is, there tend to be more outliers and extreme events than a normal distribution would imply. Now, one way to address this well-known inaccuracy is to modify the distribution allowing for fatter tails, but this adds complication to VaR analysis while contributing little insight in terms of risk.

A better approach is to accept the limitations of VaR, and then try to understand the market crises where VaR fails. If we understand the dynamics of market crises, we may be able to improve risk management to make it work when it is of the greatest importance. A starting point for understanding financial market crises is leverage and the crowding of trades. These lead to the common crisis dynamic—what I call a liquidity crisis cycle. Such a cycle begins when there is some exogenous shock that causes a drop in a market that is crowded with leveraged investors. The highly leveraged investors are forced to sell to meet their margin requirements. Their selling drops prices further, which in turn forces yet more selling, resulting in a cascading cycle downward in prices. Now, the investors that are under pressure discover there is no longer any liquidity in the stressed market, so they start to liquidate their positions in other markets to generate the required margin. And if many investors that are in the first market also have high exposure in a second one, the downward spiral propagates to this second market.

This phenomenon explains why a crisis can spread in surprising and unpredictable ways. The contagion is primarily driven by what other securities are owned by the funds that need to sell. For example, a simple example of this is what happened with the silver bubble back in 1980. The silver market became closely linked with the market for cattle. Why? Because the Hunt family had margin calls on their silver position, and so they sold whatever else they could, and what else they had to sell happened to be cattle. So thus there was a contagion based not on any economic linkage but based on who was under pressure and what else they owned.

Now, this cycle evolves unrelated to historical relationships, out of the reach of VaR-type models. But that doesn't mean it is beyond analysis. But if we want to analyze it, we need to know the leverage and the positions of the major market participants. Gathering these critical data is the first step in measuring and managing crisis risk, and should be the role of a market regulator.

Now, let me talk specifically about the role of VaR in the current crisis. Whatever the limitations of VaR models, they were not the key culprits in the case of the multi-billion dollar write-downs central to the current crisis. The large bank inventories were there to be seen. You didn't need to have any models or sophisticated detective or forensic work to see them. Furthermore, it was clear that these inventories were illiquid and that their market values were uncertain. It is hard to understand how this elephant in the room was missed, how a risk manager could see inventory grow from a few billion dollars to 10 billion dollars and then to 30 or 40 billion dollars, and not take action to bring that inventory down.

One has to look beyond VaR to sheer stupidity or collective management failure. The risk managers missed the growing inventory, or did not have the courage of their conviction to insist on its being reduced, or the senior management was not willing to heed their demands. Whatever the reason, VaR was not central to the crisis. Focus would be better placed on failures in risk governance than failures of risk models, whatever the flaws of VaR are.

Now, in summary, let me first emphasize, I believe that VaR does have value. If one were forced to pick a single number for the risk of a portfolio in the near future, VaR would be a good choice for the job. VaR illuminates most of the risk landscape, but, unfortunately, the places its light fails to reach are the canyons, crevices and cliffs.

So we can do two things to try to improve on and address the limitations of VaR. One is to employ coarser measures of risk, measures that have fewer assumptions and that are less dependent on the future looking like the past. The use of the leverage ratio mandated by U.S. regulators is an example of such a measure. The leverage ratio does not overlay assumptions about the correlation or the volatility of the assets, and does not assume any mitigating effects from diversification. It does, however, have its own limitations as a basis for capital adequacy. The second is to add other risk methods that are better at illuminating the areas VaR does not reach. So in addition to measuring risk using a standard VaR approach, develop scenarios for crises and test capital adequacy under those scenarios. Critical, of course, to the success of this approach is the ability to ferret out potential crises and describe them adequately for risk purposes. We can go a long way toward this goal by having regulators amass and aggregate data on the positions and leverage of large financial institutions. These data are critical because we cannot manage what we cannot measure, and we cannot measure what we cannot see. With these data, we will be better able to measure the crowding and leverage that leads to crisis, and shed light on risks that fail to be illuminated by VaR.

Let me close my oral comments by responding to comments by both the Chairman and the Ranking Member. The analogy of VaR and the models related to risk to models used in other engineering and physical systems—I think there is a critical distinction between financial systems and other engineering systems, because financial systems are open to gaming. If I discover a valve that is poorly designed in a nuclear power plant and design a new valve to replace it, and install that valve, the valve doesn't sit there and try to figure out if it can fool me into thinking it is on when it is really off. But in the financial markets, that is what happens. So any engineering solution or any analogy to physical processes is going to be flawed when they are applied to the financial markets, because those in the financial markets can game against the system to try to find ways around any regulation, and to find other ways to do what they want to do. And I believe that one of the key tools for this type of gaming are sophisticated, innovative, complex products that can often obfuscate what people are doing.

So, I think, parenthetical to the issues of VaR and other models is, number one, the recognition that no model can work completely in the financial markets the way they can in other physical systems, and number two, that if we want to curb or diminish the issues of gaming, we have to have more simplicity and transparency in the financial instruments.

Thank you. I look forward to your questions.

[The prepared statement of Dr. Bookstaber follows:]

PREPARED STATEMENT OF RICHARD BOOKSTABER

Mr. Chairman and Members of the Committee, I thank you for the opportunity to testify today. My name is Richard Bookstaber. Over the past decade I have worked as the risk manager in two of the world's largest hedge funds, Moore Capital Management and, most recently, Bridgewater Associates. In the 1990s I oversaw firm-wide risk at Salomon Brothers, which at the time was the largest risk-taking firm in the world, and before that was in charge of market risk at Morgan Stanley.

I am the author of *A Demon of Our Own Design—Markets, Hedge Funds, and the Perils of Financial Innovation.* Published in April, 2007, this book warned of the potential for financial crisis resulting from the growth of leverage and the proliferation of derivatives and other innovative products.

Although I have extensive experience on both the buy-side and sell-side, I left my position at Bridgewater Associates at the end of 2008, and come before the Committee in an unaffiliated capacity, representing no industry interests.

My testimony will discuss what VaR is, how it can be used and more importantly, how it can be misused. I will focus on the limitations of VaR in measuring crisis risk. I will then discuss the role of VaR in the recent market meltdown, concluding with suggestions for ways to fill the gaps left by the limitations of VaR.

What is VaR?

VaR, or Value-at-Risk, measures the risk of a portfolio of assets by estimating the probability that a given loss might occur. For example, the dollar VaR for a particular portfolio might be expressed as "there is a ten percent probability that this portfolio will lose more than $VaR over the next day."

Here is a simplified version of the steps in constructing a VaR estimate for the potential loss at the ten percent level:

1. Identify all of the positions held by the portfolio.
2. Get the daily returns for each of these positions for the past 250 trading days (about a one-year period).
3. Use those returns to construct the return to the overall portfolio for each day over the last 250 trading days.
4. Order the returns for those days from the highest to the lowest, and pick the return for the day that is the 25th worst day's return. That will be a raw estimate of the daily VaR at the ten percent level.
5. Smooth the results by fitting this set of returns to the Normal distribution function.[1]

Limitations of VaR

The critical assumptions behind the construction of VaR are made clear by the process described above:

1. All of the portfolio positions are included.
2. The sample history is a reasonable representation of what things will look like going forward.
3. The Normal distribution function is a reasonable representation of the statistical distribution underlying the returns.

The limitations to VaR boil down to issues with these three assumptions, assumptions that are often violated, leading VaR estimates to be misleading.

[1] The risk for a Normal distribution is fully defined by the standard deviation, and the results from Step 3 can be used to estimate the standard deviation of the sample. If the estimated standard deviation is, say, five percent, then the VaR at the ten percent level will be a loss of eight percent. For a Normal distribution the ten percent level is approximately 1.6 standard deviations.

Incomplete positions

Obviously, risk cannot be fully represented if not all of the risky positions are included in the analysis. But for larger institutions, it is commonplace for this to occur. Positions might be excluded because they are held off-balance sheet, beyond the purview of those doing the risk analysis; they might be in complex instruments that have not been sufficiently modeled or that are difficult to include in the position database; or they might be in new products that have not yet been included in the risk process. In the recent crisis, some banks failed to include positions in collateralized debt obligations (CDOs) for all three of these reasons.[2] And that exclusion was not considered an immediate concern because they were believed to be low risk, having attained a AAA rating.

The inability to include all of the positions in the VaR risk analysis, the most rudimentary step for VaR to be useful, is pervasive among the larger institutions in the industry. This provides a compelling reason to have a 'flight to simplicity' in financial products, to move away from complex and customized innovative products and toward standardization.[3]

Unrepresentative sample period

VaR gives a measure of risk that assumes tomorrow is drawn from the same distribution as the sample data used to compute the VaR. If the future does not look like the past, in particular if a crisis emerges, then VaR will no longer be a good measure of risk.[4] Which is to say that VaR is a good measure of risk except when it really matters.[5]

It is well known that VaR cannot measure crisis risk. During periods of crisis the relationship between securities changes in strange and seemingly unpredictable ways. VaR, which depends critically on a set structure for volatility and correlation, cannot provide useful information in this situation. It contains no mechanism for predicting the type of crisis that might occur, and does not consider the dynamics of market crises. This is not to say that VaR has no value or is hopelessly flawed. Most of the time it will provide a reasonable measure of risk—indeed the vast majority of the time this will be the case. If one were forced to pick a single number for the risk of a portfolio in the near future, VaR would be a good choice for the job. VaR illuminates most of the risk landscape. But unfortunately, the places its light fails to reach are the canyons, crevices and cliffs.

Fat Tails and the Normal Distribution

Largely because of crisis events, security returns tend to have fatter tails than what is represented by a Normal distribution. That is, there tend to be more outliers and extreme events than what a Normal distribution would predict. This leads to justifiable criticism of VaR for its use of the Normal distribution. However, sometimes this criticism is overzealous, suggesting that the professionals who assume a Normal distribution in their analysis are poorly trained or worse. Such criticism is unwarranted; the limitations of the Normal distribution are well-known. I do not know of anyone working in financial risk management, or indeed in quantitative finance generally, who does not recognize that security returns may have fat tails. It is even discussed in many investment textbooks, so it is a point that is hard to miss.[6]

---

[2] Regulatory capital on the trading assets that a bank does not include in VaR—or for which the bank's VaR model does not pass regulatory scrutiny—is computed using a risk-rating based approach. However, the rating process itself suffers from many of the difficulties associated with calculating VaR, as illustrated by the AAA ratings assigned to many mortgage-backed CDOs and the consequent severe underestimation of the capital required to support those assets.

[3] I discuss the complexity and related risk issues surrounding derivatives and related innovative products in Testimony of Richard Bookstaber, Submitted to the Senate of the United States, Committee on Agriculture, Nutrition, and Forestry for the Hearing: "Regulatory Reform and the Derivatives Markets," June 4, 2009.

[4] One way to try to overcome the problem of relying on the past is to use a very long time period in the VaR calculation, with the idea that a longer period will include many different regimes, crises and relationships. Such a view misses the way different regimes, essentially different distributions, mix to lead to a final result. A long time period gives muddied results. To see this, imagine the case where in half of the past two assets were strongly positively correlated and the other half they were strongly negatively correlated. The mixing of the two would suggest the average of little correlation, thus giving a risk posture that did not exist in either period, but that also incorrectly suggests diversification opportunities.

[5] As a corollary to this, one could also say that diversification works except when it really matters.

[6] For example, Investments, by Bodie, Kane, and Marcus, 8th edition (McGraw-Hill/Irwin), has a section (page 148) entitled "Measurement of Risk with Non-normal Distributions."

The issue is how this well-known inaccuracy of the Normal distribution is addressed. One way is knowingly to misuse VaR, to ignore the problem and act as if VaR can do what it cannot. Another is to modify the distribution to allow for fatter tails.[7] This adds complication and obfuscation to the VaR analysis, because any approach employing a fat-tailed distribution increases the number of parameters to estimate, and this increases the chance that the distribution will be mis-specified. And in any case, simply fattening up the tails of the distribution provides little insight for risk management.

I remember a cartoon that showed a man sitting behind a desk with a name plate that read 'Risk Manager.' The man sitting in front of the desk said, "Be careful? That's all you can tell me, is to be careful?" Stopping with the observation that extreme events can occur in the markets and redrawing the distribution accordingly is about as useful as saying "be careful." A better approach is to accept the limitations of VaR, and then try to understand the nature of the extreme events, the market crises where VaR fails. If we understand the dynamics of market crisis, we may be able to improve risk management to make it work when it is of the greatest importance.

Understanding the Dynamics of Market Crises

A starting point for understanding financial market crises is leverage and the crowding of trades, both of which have effects that lead to a common crisis dynamic, the liquidity crisis cycle.

Such a cycle begins when an exogenous shock causes a drop in a market that is crowded with leveraged investors. The highly leveraged investors are forced to sell to meet their margin requirements. Their selling drops prices further, which in turn forces yet more selling, resulting in a cascading cycle downward in prices. Those investors that are under pressure discover there is no longer liquidity in the stressed market, so they start to liquidate their positions in other markets to generate the required margin. If many of the investors that are in the first market also have high exposure in a second one, the downward spiral propagates to this second market.[8]

This phenomenon explains why a crisis can spread in surprising and unpredictable ways. The contagion is driven primarily by what other securities are owned by the funds that need to sell.[9] For example, when the silver bubble burst in 1980, the silver market became closely linked to the market for cattle. Why? Because when the Hunt family had to meet margin calls on their silver positions, they sold whatever else they could. And they happened also to be invested in cattle. Thus there is contagion based not on economic linkages, but based on who is under pressure and what else they are holding.

This cycle evolves unrelated to historical relationships, out of the reach of VaR-type models. But that does not mean it is beyond analysis. Granted it is not easy to trace the risk of these potential liquidity crisis cycles. To do so with accuracy, we need to know the leverage and positions of the major market participants. No one firm, knowing only its own positions, can have an accurate assessment of the crisis risk. Indeed, each firm might be managing its risk prudently given the information it has at its disposal, and not only miss the risk that comes from crowding and leverage, but also unwittingly contribute to this risk. Gathering these critical data is the first step in measuring and managing crisis risk. This should be the role of a market regulator.[10]

---

[7] Extreme value theory is the bastion for techniques that employ distributions with a higher probability of extreme events.

[8] The use of VaR-based capital can actually contribute to this sort of cycle. VaR will increase because of the higher volatility—and also possibly because of the higher correlations—leading potential liquidity providers and lenders to pull back. This was a likely exacerbating effect during the 1997 Asian crisis.

[9] As an illustration, the proximate cause of Long Term Capital Management's (LTCM's) demise was the Russian default in August, 1998. But LTCM was not highly exposed to Russia. A reasonable risk manager, aware of the Russian risks, might not have viewed it as critical to LTCM. But the Russian default hurt LTCM because many of those who did have high leverage in Russia also had positions in other markets where LTCM was leveraged. When the Russian debt markets failed and these investors had to come up with capital, they sold their more liquid positions in, among other things, Danish mortgage bonds. So the Danish mortgage bond market and these other markets went into a tail spin, and because LTCM was heavily exposed in these markets, the contagion took LTCM with it.

[10] I discuss the need for firm-level position and leverage data in crisis risk management in previous testimony before both the House and the Senate. For example, Testimony of Richard Bookstaber, Submitted to the Congress of the United States, House Financial Services Committee, for the Hearing: "Systemic Risk: Examining Regulators Ability to Respond to Threats

Continued

The Role of VaR in the Current Crisis

The above discussion provides part of the answer to the question of the role of VaR in the current market crisis: If VaR was used as the source of risk measurement, and thus as the determinant of risk capital, then it missed the potential for the current crisis for the simple reason that VaR is not constructed to deal with crisis risk. And if VaR was applied as if it actually reflected the potential for crisis, that is, if it was forgotten that VaR is only useful insofar as the future is drawn from the same distribution as the past, then this led to the mis-measurement of risk. So if VaR was the sole means of determining risk levels and risk capital coming into this crisis, it was misused. But this does not present the full story.

Whatever the limitations of VaR models, they were not the key culprits in the case of the multi-billion dollar write-downs during the crisis. The large bank inventories were there to be seen; no models or detective work were needed. Furthermore, it was clear the inventories were illiquid and their market values uncertain.[11] It is hard to understand how this elephant in the room was missed, how a risk manager could see inventory grow from a few billion dollars to ten billion dollars and then to thirty or forty billion dollars and not react by forcing that inventory to be brought down.

Of course, if these inventories were not properly included in the VaR analysis, the risk embodied by these positions would have been missed, but one has to look beyond VaR, to culprits such as sheer stupidity or collective management failure: The risk managers missed the growing inventory, or did not have the courage of their conviction to insist on its reduction, or the senior management was not willing to heed their demands. Whichever the reason, VaR was not central to this crisis.[12] Focus would be better placed on failures in risk governance than failures of risk models.

Summary: VaR and Crisis Risk

There are two approaches for moving away from over-reliance on VaR.

The first approach is to employ coarser measures of risk, measures that have fewer assumptions and that are less dependent on the future looking like the past.[13] The use of the Leverage Ratio mandated by U.S. regulators and championed by the FDIC is an example of such a measure.[14] The leverage ratio does not overlay assumptions about the correlation or the volatility of the assets, and does not assume any mitigating effect from diversification, although it has its own limitations as a basis for capital adequacy.[15]

The second approach is to recognize that while VaR provides a guide to risk in some situations, it must be enhanced with other measures that are better at illuminating the areas it does not reach. For example, Pillar II of Basel II has moved to include stress cases for crises and defaults into its risk capital process. So in addition to measuring risk using a standard VaR approach, firms must develop sce-

---

to the Financial System," October 2, 2007, and Testimony of Richard Bookstaber, Submitted to the Senate of the United States, Senate Banking, Housing and Urban Affairs Subcommittee on Securities, Insurance and Investment, for the Hearing: "Risk Management and Its Implications for Systematic Risk," June 19, 2008.

[11] This is especially true when one considers the business of the banks, which is to package the securities and sell them. The growth of inventory was outside the normal business of the banks. That the securities were not moving out the door should have been an immediate indication they were not correctly priced.

[12] Indeed, in some important cases, VaR was not even employed in the risk process. A case in point is the 'super senior' mortgage CDO positions which caused huge trading losses at a number of banks. There is a common misconception that regulatory capital for trading assets is automatically computed using VaR. In fact, trading assets are eligible for VaR-based capital only if the bank can demonstrate to its supervisor that its model is robust. Absent this, a coarser method is applied. Many of the highly complex securities at the heart of the recent crisis were not regarded as being suitable for VaR treatment, and received a simpler ratings-based treatment, which proved to severely underestimate the capital required to support the assets.

[13] I believe coarse measures—measures that are not fine tuned to be ideal in any one environment, but are robust across many environments—are a key to good risk management.

[14] The Leverage Ratio is the ratio of Tier 1 capital, principally equity and retained earnings, to total assets.

[15] The Leverage Ratio is inconsistent with Basel II because it is not sensitive to the riskiness of balance sheet assets and it does not capture off-balance sheet risks. By not taking the relative risk of assets into account, it could lead to incentives for banks to hold riskier assets, while on a relative basis penalizing those banks that elect to hold a low-risk balance sheet. In terms of risk to a financial institution, the time horizon of leverage is also important, which the Leverage Ratio also misses. The problems with Bear Stearns and Lehman was not only one of leverage per se, but of funding a sizable portion of leverage in the short-term repo market. They thus were vulnerable to funding drying up in the face of a crisis.

narios for crises and test their capital adequacy under those scenarios. Critical to the success of this approach is the ability to ferret out potential crises and describe them adequately for risk purposes.

This means that for crisis-related stress testing to be feasible, we first must believe that it is indeed possible to model financial crisis scenarios, i.e., that crises are not 'black swans.' This is not to say that surprises do not occur. Though recently popularized, the recognition that we are beset by unanticipatable risk, by events that seemingly come out of nowhere and catch us unawares, has a long history in economics and finance, dating back to Frank Knight in the 1920s.[16] The best defense against such risks is to maintain a coarse, simple and robust financial structure. Rather than fine-tuning for the current environments, we need risk measures and financial instruments which, while perhaps not optimal for the world of today, will be able to operate reasonably if the world changes in unexpected ways. VaR as currently structured is not such a risk measure.

However, although surprises do occur, crisis scenarios are not wholly unanticipatable; they are not in the realm of Knightian uncertainty. We have had ample experience with financial crises. We know a thing or two about them.[17] And we can further anticipate crisis risk by amassing data on the positions and leverage of the large investment firms. The regulator is best suited to take on this task, because these are data that no one firm can or should fully see.[18] With these critical data we will be better able to measure the crowding and leverage that lead to liquidity crisis cycles and begin to shed light on the areas of financial risk that fail to be illuminated by VaR.[19]

---

[16] Knight makes the distinction between risks we can identify and measure and those that are unanticipatable and therefore not measurable in *Risk, Uncertainty, and Profit.* (1921), Boston, MA: Houghton Mifflin Company.

[17] For example, even beyond the insights to be gained from a detailed knowledge of firm-by-firm leverage and market crowding, there are some characteristics of market crisis that can be placed into a general scenario. When a crisis occurs, equity prices drop, credit spreads rise, and the volatility of asset returns increases. The yield curve flattens and gold prices rise. Furthermore, the correlation between individual equities rises, as does the correlation between equities and corporate bonds. The riskier and less liquid assets fare more poorly, so, for example, emerging markets take a differentially bigger hit than their G–7 cousins. More broadly, anything that is risky or less liquid becomes more common and negative in its return; the subtleties of pricing between assets becomes overshadowed by the assets' riskiness. However, short-term interest rates and commodity prices are less predictable; in some cases, such as in the case of the inflation-laden crisis of 1973–1974, they rise, while in other cases, such as in the current crisis, they drop.

Each of these effects can occur with a ferocity far beyond what is seen in normal times, so if these crisis events are overlaid on the distribution coming out of the VaR model based on those normal times one will come away saying the crisis is a 100-year flood event, a twenty standard deviation event, a black swan. But it is none of these things. It is a financial crisis, and such crises occur frequently enough that to be understood without such shock and awe.

[18] Financial firms will be justifiably reticent to have their position and leverage information made public, so the collection and analysis of the data will have to reside securely in the regulator.

[19] With these data, the regulator is also in a position to run risk analysis independent of the firms. Under Basel II, the regulator still depends on the internal processes of the banks for the measurement of risk and the resulting capital requirements.

Appendix

Related Blog Posts on VaR and Risk Management

The Fat-Tailed Straw Man

See *http://rick.bookstaber.com/2009/03/fat-tailed-straw-man.html*

My *Time* article about the quant meltdown of August, 2007 started with "Looks like Wall Street's mad scientists have blown up the lab again." Articles on Wall Street's mad scientist blowing up the lab seem to come out every month in one major publication or another. The *New York Times* has a story along these lines today and had a similar story in January.

There is a constant theme in these articles, invariably including a quote from Nassim Taleb, that quants generally, and quantitative risk managers specifically, missed the boat by thinking, despite all evidence to the contrary, that security returns can be modeled by a Normal distribution.

This is a straw man argument. It is an attack on something that no one believes.

Is there anyone well trained in quantitative methods working on Wall Street who does not know that security returns have fat tails? It is discussed in most every investment text book. Fat tails are apparent—even if we ignore periods of crisis—in daily return series. And historically, every year there is some market or other that has suffered a ten standard deviation move of the "where did that come from" variety. I am firmly in the camp of those who understand there are unanticipatable risks; as far back as an article I co-authored in 1985, I have argued for the need to recognize that we face uncertainty from the unforeseeable. To get an idea of how far back the appreciation of this sort of risk goes in economic thought, consider the fact that it is sometimes referred to as Knightian uncertainty.

Is there any risk manager who does not understand that VaR will not capture the risk of market crises and regime changes? The conventional VaR methods are based on historical data, and so will only be an accurate view of risk if tomorrow is drawn from the same population as the sample it uses. VaR is not perfect, it cannot do everything. But if we understand its flaws—and every professional risk manager does—then it is a useful guide for day-to-day market risk. If you want to add fat tails, fine. But as I will explain below, that is not the solution.

So, then, why is there so much currency given to a criticism of something that no one believes in the first place?

It is because quant methods sometimes fail. We can quibble with whether 'sometimes' should be replaced with 'often' or 'frequently' or 'every now and again,' but we all know they are not perfect. We are not, after all, talking about physics, about timeless and universal laws of the universe when we deal with securities. Weird stuff happens. And the place where the imperfection is most telling is in risk management.

When the risk manager misses the equivalent of a force five hurricane, we ask what is wrong with his methods. By definition, what he missed was a ten or twenty standard deviation event, so we tell him he ignored fat tails. There you have it, you failed because you did not incorporate fat tails. This is tautological. If I miss a large risk—which will occur on occasion even if I am fully competent; that is why they are called risks—I will have failed to account for a fat tailed event. I can tell you that ahead of time. I can tell you now—as can everyone in risk management—that I will miss something. If after the fact you want to castigate me for not incorporating sufficiently fat tailed events, let the flogging begin.

I remember a cartoon that showed a man sitting behind a desk with a name plate that read 'risk manager.' The man sitting in front of the desk said, "Be careful? That's all you can tell me, is to be careful?" Observing that extreme events can occur in the markets is about as useful as saying "be careful." We all know they will occur. And once they have occurred, we will all kick ourselves and our risk managers and our models, and ask "how could we have missed that?"

The flaw comes in the way we answer that question, a question that can be stated more analytically as "what are the dynamics of the market that we failed to incorporate." If we answer by throwing our hands into the air and saying, "well, who knows, I guess that was one of them there ten standard deviation events," or "what do you expect; that's fat tails for you," we will be in the same place when the next crisis arrives. If instead we build our models with fatter and fatter tailed distributions, so that after the event we can say, "see, what did I tell you, there was one of those fat tailed events that I postulated in my model," or "see, I told you to be careful," does that count for progress?

So, to recap, we all know that there are fat tails; it doesn't do any good to state the mantra over and over again that securities do not follow a Normal distribution. Really, we all get it. We should be constructive in trying to move risk management beyond the point of simply noting that there are fat tails, beyond admonitions like "hey, you know, shit happens, so be careful." And that means understanding the dynamics that create the fat tails, in particular, that lead to market crisis and unexpected linkages between markets.

What are these dynamics?

One of them, which I have written about repeatedly, is the liquidity crisis cycle. An exogenous shock occurs in a highly leveraged market, and the resulting forced selling leads to a cascading cycle downward in prices. This then propagates to other markets as those who need to liquidate find the market that is under pressure no longer can support their liquidity needs. Thus there is contagion based not on economic linkages, but based on who is under pressure and what else they are holding. This cycle evolves unrelated to historical relationships, out of the reach of VaR-types of models, but that does not mean it is beyond analysis.

Granted it is not easy to trace the risk of these potential liquidity crisis cycles. To do so with accuracy, we need to know the leverage and positions of the market participants. In my previous post, "Mapping the Market Genome," I argued that this should be the role of a market regulator. But even absent that level of detail, perhaps we can get some information indirectly from looking at market flows.

No doubt there are other dynamics that lead to the fat tailed events currently frustrating our efforts to manage risk in the face of market crises. We need to move beyond the fat-tail critiques and the 'be careful' mantra to discover and analyze them.

The Myth of Non-correlation

See *http://rick.bookstaber.com/2007/09/myth-of-noncorrelation.html*

[This is a modified version of an article I wrote that appeared in the September, 2007 issue of Institutional Investor.]

With the collapse of the U.S. sub-prime market and the after-shocks that have been felt in credit and equity markets, there has been a lot of talk about fat tails, 20 standard deviation moves and 100-year event. We seem to hear such descriptions fairly frequently, which suggests that maybe all the talk isn't really about 100-year events after all. Maybe it is more a reflection of investors' market views than it is of market reality.

No market veteran should be surprised to see periods when securities prices move violently. The recent rise in credit spreads is nothing compared to what happened in 1998 leading up to and following the collapse of hedge fund Long-Term Capital Management or, for that matter, during the junk bond crisis earlier that decade, when spreads quadrupled.

What catches many investors off guard and leads them to make the "100 year" sort of comment is not the behavior of individual markets, but the concurrent big and unexpected moves among markets. It's the surprising linkages that suddenly appear between markets that should not have much to do with one other and the failed linkages between those that should march in tandem. That is, investors are not as dumbfounded when volatility skyrockets as when correlations go awry. This may be because investors depend on correlation for hedging and diversifying. And nothing hurts more than to think you are well hedged and then to discover you are not hedged at all.

Surprising Market Linkages

Correlations between markets, however, can shift wildly and in unanticipated ways—and usually at the worst possible time, when there is a crisis with volatility that is out of hand. To see this, think back on some of the unexpected correlations that have haunted us in earlier market crises:

- *The 1987 stock market crash.* During the crash, Wall Street junk bond trading desks that had been using Treasury bonds as a hedge were surprised to find that their junk bonds tanked while Treasuries strengthened. They had the double whammy of losing on the junk bond inventory and on the hedge as well. The reason for this is easy to see in retrospect: Investors started to look at junk bonds more as stock-like risk than as interest rate vehicles while Treasuries became a safe haven during the flight to quality and so were bid up.
- *The 1997 Asian crisis.* The financial crisis that started in July 1997 with the collapse of the Thai baht sank equity markets across Asia and ended up en-

veloping Brazil as well. Emerging-markets fund managers who thought they had diversified portfolios—and might have inched up their risk accordingly—found themselves losing on all fronts. The reason was not that these markets had suddenly become economically linked with Brazil, but rather that the banks that were in the middle of the crisis, and that were being forced to reduce leverage, could not do so effectively in the illiquid Asian markets, so they sold off other assets, including sizable holdings in Brazil.

- *The fall of Long-Term Capital Management in 1998.* When the LTCM crisis hit, volatility shot up everywhere, as would be expected. Everywhere, that is, but Germany. There, the implied volatility dropped to near historical lows. Not coincidentally, it was in Germany that LTCM and others had sizable long volatility bets; as they closed out of those positions, the derivatives they held dropped in price, and the implied volatility thus dropped as well. Chalk one up for the adage that markets move to inflict the most pain.

And now we get to the crazy markets of August 2007. Stresses in a minor part of the mortgage market—so minor that Federal Reserve Board Chairman Ben Bernanke testified before Congress in March that the impact of the problem had been "moderate"—break out not only to affect other mortgages but also to widen credit spreads worldwide. And from there, sub-prime somehow links to the equity markets. Stock market volatility doubles, the major indexes tumble by 10 percent and, most improbable of all, a host of quantitative equity hedge funds—which use computer models to try scrupulously to be market neutral—are hit by a "100-year" event.

When we see this sort of thing happening, our not very helpful reaction is to shake our heads as if we are looking over a fender bender and point the finger at statistical anomalies like fat tails, 100-year events, black swans, or whatever. This doesn't add much to the discourse or to our ultimate understanding. It is just more sophisticated ways of saying we just lost a lot of money and were caught by surprise. Instead of simply stating the obvious, that big and unanticipated events occur, we need to try to understand the source of these surprising events. I believe that the unexpected shifts in correlation are caused by the same elements I point to in my book as the major cause of market crises: complexity and tight coupling.

Complexity

Complexity means that an event can propagate in nonlinear and unanticipated ways. An example of a complex system from the realm of engineering is the operation of a nuclear power plant, where a minor event like a clogged pressure-release valve (as occurred at Three Mile Island) or a shift in the combination of steam production and fuel temperature (as at Chernobyl) can cascade into a meltdown.

For financial markets, complexity is spelled d-e-r-i-v-a-t-i-v-e-s. Many derivatives have nonlinear payoffs, so that a small move in the market might lead to a small move in the price of the derivative in one instance and to a much larger move in the price in another. Many derivatives also lead to unexpected and sometimes unnatural linkages between instruments and markets. Thanks to collateralized debt obligations, this is what is at the root of the first leg of the contagion we observed from the sub-prime market. Sub-primes were included in various CDOs, as were other types of mortgages and corporate bonds. Like a kid who brings his cold to a birthday party, the sickly sub-prime mortgages mingled with these other instruments.

The result can be unexpected higher correlation. Investors that have to reduce their derivatives exposure or hedge their exposure by taking positions in the underlying bonds will look at them as part of a CDO. It doesn't matter if one of the underlying bonds is issued by a AA-rated energy company and another by a BB financial; the bonds in a given package will move in lockstep. And although sub-prime happens to be the culprit this time around, any one of the markets involved in the CDO packaging could have started things off.

Tight Coupling

Tight coupling is a term I have borrowed from systems engineering. A tightly coupled process progresses from one stage to the next with no opportunity to intervene. If things are moving out of control, you can't pull an emergency lever and stop the process while a committee convenes to analyze the situation. Examples of tightly coupled processes include a space shuttle launch, a nuclear power plant moving toward criticality and even something as prosaic as bread baking.

In financial markets tight coupling comes from the feedback between mechanistic trading, price changes and subsequent trading based on the price changes. The mechanistic trading can result from a computer-based program or contractual requirements to reduce leverage when things turn bad.

In the '87 crash tight coupling arose from the computer-based trading of those running portfolio insurance programs. On Monday, October 19, in response to a nearly 10 percent drop in the U.S. market the previous week, these programs triggered a flood of trades to sell futures to increase the hedge. As those trades hit the market, prices dropped, feeding back to the computers, which ordered yet more rounds of trading.

More commonly, tight coupling comes from leverage. When things start to go badly for a highly leveraged fund and its collateral drops to the point that it no longer has enough assets to meet margin calls, its manager has to start selling assets. This drops prices, so the collateral declines further, forcing yet more sales. The resulting downward cycle is exactly what we saw with the demise of LTCM.

And it gets worse. Just like complexity, the tight coupling born of leverage can lead to surprising linkages between markets. High leverage in one market can end up devastating another, unrelated, perfectly healthy market. This happens when a market under stress becomes illiquid and fund managers must look to other markets: If you can't sell what you want to sell, you sell what you can. This puts pressure on markets that have nothing to do with the original problem, other than that they happened to be home to securities held by a fund in trouble. Now other highly leveraged funds with similar exposure in these markets are forced to sell, and the cycle continues. This may be how the sub-prime mess expanded beyond mortgages and credit markets to end up stressing quantitative equity hedge funds, funds that had nothing to do with sub-prime mortgages.

All of this means that investors cannot put too much stock in correlations. If you depend on diversification or hedges to keep risks under control, then when it matters most it may not work.

BIOGRAPHY FOR RICHARD BOOKSTABER

Richard Bookstaber has worked in some of the largest buy-side and sell-side firms, in capacities ranging from risk management to portfolio management to derivatives research.

Over the past decade he has worked as a risk manager at Bridgewater Associates in Westport, Connecticut, Moore Capital Management and Ziff Brothers Investments. He also ran the FrontPoint Quantitative Fund, a market neutral long/short equity fund, at FrontPoint Partners.

From 1994 through 1998, Mr. Bookstaber was the Managing Director in charge of firm-wide risk management at Salomon Brothers. In this role he oversaw both the client and proprietary risk-taking activities of the firm, and served on that firm's powerful Risk Management Committee. He remained in these positions at Salomon Smith Barney after the firm's purchase by Traveler's and the merger that formed Citigroup.

Before joining Salomon, Mr. Bookstaber spent ten years at Morgan Stanley in quantitative research and as a proprietary trader. He also marketed and managed portfolio hedging programs as a fiduciary at Morgan Stanley Asset Management. With the creation of Morgan Stanley's risk management division, he was appointed as the Firm's first Director of Market Risk Management.

He is the author of four books and scores of articles on finance topics ranging from option theory to risk management. He has received various awards for his research, including the Graham and Dodd Scroll from the Financial Analysts Federation and the Roger F. Murray Award from the Institute of Quantitative Research in Finance.

Mr. Bookstaber's most recent book is *A Demon of Our Own Design—Markets, Hedge Funds and the Perils of Financial Innovation* (Wiley, 2007).

He received a Ph.D. in economics from MIT.

DISCUSSION

Chairman MILLER. Thank you very much. We will now have rounds of questions of five minutes for each Member, and I will begin by recognizing myself for five minutes.

CAN ECONOMIC EVENTS BE PREDICTED?

Dr. Bookstaber, what you just described, what I have heard you describe as gaming, I have heard celebrated on the Financial Services Committee, on which I also serve, as innovation—that a lot of innovation seems to be simply a way to evade existing regulations. And I think both of you got it—Dr. Bookstaber, in that last bit of testimony you certainly got at it, but the supporters of the VaR now they say want a do-over, that the VaR model was perhaps flawed but it can be fixed, and they can now develop a more reliable model that will predict fat tail events, the unlikely events. Do you think that it is a failure of that model, or do you think the failure is in the idea that economic events can be predicted with the same precision that the movement of the planets can be predicted? Do you think that it is inherently flawed to think that we can develop models that will be unfailingly reliable? Dr. Taleb.

Dr. TALEB. This is my life story. From the beginning—and I heard, Dr. Bookstaber and I share a lot of opinions, you know, on things like gaming, like the numbers that are going to be gamed on the interaction between model and participants. However, there are two things or three things that I heavily disagree with, and the first one is, he said that we can use different distribution to model tail events. Well, that is the story of my life. This is why I provided this paper forthcoming in which I look at 20 million pieces of data, every single economic variable I could find, and tried to see if there is regularity in the data helping to predict itself, you know, outside that sample from which it was derived. Unfortunately, it is impossible, and that is my first argument, that the more remote the event, the less we can predict it, and that's my first point. And the second one is, we know which variables are more unpredictable than others, and it is very easy to protect against that. And the third one is that I agree with Dr. Bookstaber; if I were, you know, an omnipotent person seeing all the leverage and everything in the system, and equipped with heavy, you know, equations, I could probably figure it out. However, this is Soviet-style thinking, that someone, some regulator, some unit out there can see what is going on and be able to model it, because unfortunately when we model in complex systems, we have non-linearity. Even if I gave you all the data and you missed something by $1 million, okay—your probabilities will change markedly.

Chairman MILLER. I will get to you, Dr. Bookstaber, but your solution then is just higher liquidity requirements?

Dr. TALEB. No, my solution is figuring out—it is very simple. I was a trader in the 1980s. There were some products we could really risk manage on a napkin. Options, instruments, futures, all these we could risk manage on a napkin. Once we started having these toxic products—to me, the sole purpose of these products is to create bonuses, like complex derivatives. I was a complex derivatives trader. I have a textbook on complex derivatives, and I tell you, these products, okay, can hide massive amounts of tail risks. They are not needed for anyone. A lot of these products should not be there. If you eliminate some of the products, some of the exposure, it would not change anything to economic life and it would make things a lot more measurable. So my solution is to ban some products that have a toxic exposure to tail events.

Chairman MILLER. Dr. Bookstaber.

Dr. BOOKSTABER. Let me just correct one point. I do not advocate trying to fix VaR by fattening the tails. I am simply arguing that some people make that as a suggestion. I think VaR is what it is, it does what it does, and the best thing to do is recognize the limitations of VaR, which I stated, and use it for what it is good for but not try to oversell it, not to think that it represents all possible risk, because any attempts to somehow make it more sophisticated are just going to obfuscate it all the more. So you take VaR as one tool for risk management, and then extend out from there.

The second point, just addressing what you are saying, is that, number one, I don't think that you can use VaR and have a 'do-over' to try to expand it and have it solve these crisis-type problems. I also don't think that we will ever be at the point of being able to know all the risks. But I do think that we can move somewhat in the direction of understanding crisis risk more. But to do it, you need the data, and the data that you really need to start with is: how highly leveraged are the people in the market, and what are their positions—so that if there is a shock in a particular market, will there be so much leverage there that people will be forced to liquidate? What other positions do they have, so how could that propagate out? It is not a panacea. You can't have a silver bullet because of the feedback and gaming capabilities but I think you can move more in the direction of dealing with these crisis risks.

REGULATION OF FINANCIAL PRODUCTS

Chairman MILLER. My time has expired but I have a question that is sort of in hot pursuit of what you both just said, and I will be similarly indulgent to the other Members here.

Dr. Taleb, you said there should be something like a Food and Drug Administration (FDA) to look at financial products, to see if they actually do something useful, or if they simply create additional risks that create short-term profits. Apparently about 90 percent of derivatives—I was only half kidding when I asked you if your mother knew you designed derivatives. But in about 90 percent of derivatives, no party to the transaction has any interest in the underlying, whatever it was, that the derivative is derived from—credit default swaps. Do you agree that some financial products should simply be banned as having no readily discernible usefulness, utility for society, for the economy—and creating a risk that we cannot begin to understand? Should credit default swaps be banned? Should they be limited to—have a requirement that is equivalent to an insurable interest requirement in insurance law? Dr. Taleb or Dr. Bookstaber?

Dr. TALEB. I cannot—I don't—I am not into regulation to know whether we should be allowed to ban people based on uses but—based on risk, okay, because society doesn't bear the risk. I have here what I call the 'fourth quadrant,' and we should ban financial products—and when I call it the fourth, it is a little technical, but it is a very simple rule of thumb that takes minutes to check if a given financial product belongs or doesn't belong to the fourth quadrant. In other words, does it have any explosive toxic effects on either the user or the issuer, or both, you know, so it is very easy. So these products—and this is how I have my fourth quadrant—these are the exposures we should not just compute, you know, but eliminate. And there are a lot of things we can measure.

I mean, I may agree with Dr. Bookstaber, VaR may work for some products, and we know which ones, but not for these products that have open-ended, toxic, geometric—what I call geometric—in other words, escalating payoffs.

Chairman MILLER. Dr. Bookstaber.

Dr. BOOKSTABER. For reference, I refer the Committee to testimony that I gave in June to the Agricultural Committee of the Senate on the topic of derivatives, and there I pointed out that, over time, derivatives have moved more and more towards being used for gaming. In fact, I said that derivatives are the weapon of choice for gaming the system. They are used to allow you to hedge when you are not supposed to hedge, to avoid taxes, to lever when you are not supposed to lever. There is vested interest on both the sell and the buy side to have derivatives that are complex and obfuscating, that are customized. I believe, number one, that many derivative instruments that exist today are used more for either gaming or gambling purposes as opposed to having true economic function. And I believe that there are many customized and complex instruments that could easily be transformed into a set of standardized instruments that would be easier to track, more transparent, and possibly even put on an exchange. So I certainly agree with the concept that derivatives is a point to focus on, because it is one of the ways that we find risk coming in these tail events in surprising ways.

Chairman MILLER. Thank you, Dr. Bookstaber.

I now recognize Dr. Broun for nine minutes and 45 seconds.

'TOO BIG TO FAIL'?

Mr. BROUN. Thank you, Mr. Chairman. I want to make a quick statement. I believe, first thing, that there is no such thing as an entity that is too big to fail, particularly when we look at businesses, even large businesses such as the investment banks, and I believe in holding people personally accountable and responsible, and I believe that when you take away the taxpayer safety net that people are utilizing to gamble away other people's future, then people will be held more accountable and will make better decisions. I think greed and lust are two tremendous blinding factors when people start making decisions.

Having said that, I also want to state that I think that there were a lot of warning signs about this current economic crisis that we found ourselves in, and many people sounded the horn of warning saying that we needed to change federal law and regulation to prevent what has happened, and those warnings were unheeded by Congress and by people who were in the decision-making process. Having said that, I am real concerned too because investment banks took excessive risk based on these models and commercial banks are also now forced to rein in risk, even though they are not taking risky positions to begin with, those commercial banks. What can we do to ensure that small commercial banks around the country are not punished by the risky behavior of large investment banks? Either or both, who wants to go first?

Dr. BOOKSTABER. That is a difficult question, and I don't know that I can illuminate it too much, but I can go in a particular direction. You can correct me if I am going the wrong way. I think there is a distinction between the larger banks, which de facto actually are the investment banks, and the smaller banks, because the larger banks end up quasi-market makers in the sense that they take on positions of risk for clients. They become market makers in the fixed-income market. They issue and support derivatives. They also have proprietary trading desks so they are also quasi-hedge funds. So I think you can look at the various functions of banks, and look at smaller banks, and they typically have a pure banking function. Larger banks are not really just bigger versions of smaller banks. They are actually institutions that take different types of risk that smaller banks don't take, that can have some of these tail events of their own creation—that are demons of their own design—that they have created because they have elected to go into the derivatives markets, or take market-making functions.

So I think the question for a regulator is, do you have a different set of regulations and requirements for the banks that—it is not an issue of being too big to fail, but banks that are taking on types of risk that make them distinct from their smaller cousins.

Mr. BROUN. Isn't it greed that drives that as far as the large institutions, though?

Dr. BOOKSTABER. Well, you know, greed has a little bit of spin to it. I mean, there are incentives, and people act based on their incentives; and if we give somebody a set of incentives that, as Dr. Taleb has mentioned, lead them to say, 'I want to take risks which might blow the bank up, with small probability, but with very high probability will give me a large bonus,' you are going to have people acting accordingly. So I think the way to think of it is, not that they are acting on the basis of greed, but they are acting on the basis of incentives that lead to behavior that, for the market overall, may be unduly risky.

Mr. BROUN. Isn't it so particularly when you have somebody else who is going to be held responsible for that decision-making process?

Dr. BOOKSTABER. Right.

Mr. BROUN. Like the taxpayer is going to be on the hook if they make a bad decision.

Dr. BOOKSTABER. That is right. There is no doubt that incentives have played a large role in what we have observed. You know, had you had, for example, somebody like Mr. Prince saying—apparently recognizing the riskiness of what they are doing—and saying, well, as long as the music is playing, we are going to keep dancing. Why is he going to keep dancing? Because his incentive is based on next quarter's earnings, and he can't walk away from that dance floor while his competitors are still on it, because his incentives are structured to make that incorrect decision.

Mr. BROUN. And he has everything to gain and nothing to lose in that process, correct?

Dr. BOOKSTABER. Yes.

Mr. BROUN. Dr. Taleb.

Dr. TALEB. Yes. Well, I just wrote a paper with my colleague (Charles Tapiero) in which we showed why—I don't know if you have heard about the case of Société Generale, the French bank that lost $7 billion, $8 billion on a rogue trader, and we showed that it came from too big a size. Size has effect in compounding risk, and let me give you the intuition. If you have a bank a tenth of the size of Société Generale, and they had a rogue trader that had a tenth of the size of the position of that rogue trader, the losses would have been close to zero. The fact that they had to liquidate, they discovered that that rogue trader had 50 billion euros in hidden position and they had to liquidate that, and liquidating 50 billion euros rapidly costs a lot more than liquidating five billion euros. You liquidate five billion euros at no transaction cost almost, or a very small transaction cost, compared to liquidating 50 billion. So that would generalize to risks of unexpected events tend to affect large size more.

And I have here another comment to make about banks. Banks, of course, have done so far—I mean, we have evidence they have done, so far, very little for society, except generate bonuses for themselves, from the data, and that is not from recent events that I am deriving that. When I wrote *"The Black Swan"* it was before these events. But look at hedge funds. Hedge funds, I heard the number, 1,800 hedge fund failed in the last episode. I don't know if many of them made the front page of any Washington paper. So the hedge funds seem to be taking risks without endangering society, or at least not taxpayers directly. And this model of hedge fund corresponds to my norm, okay—what is a complex system that is robust? The best one is Mother Nature. Mother Nature has a lot of interdependence. We have an ecosystem, a lot of interdependence. But if you went and shot the largest mammal, a whale, or the largest land mammal, an elephant, you would not destroy the ecosystem. If you shot Lehman Brothers, well, you know what happened, okay. You destroyed the system—too much interdependence means you should not have large units. But hedge funds have shown us the way to go. They are born and they die every day, literally every day. Today I am sure that many hedge funds are born and many hedge funds have died. So this is a model that replicates how nature works with interdependence. But of course we have to listen to Dr. Bookstaber's advice to make sure that they don't all have the same positions you have to put the exclusionary system, but they have a lot more diversity than banks.

WALL STREET'S DEPENDENCY ON GOVERNMENT BAILOUTS

Mr. BROUN. Isn't it though that the implied or even outright safety net of the taxpayers picking up the pieces if there is a failure, isn't that the thing that is driving the derivatives and all these other complex financial instruments that cause people to make these risky behavior judgments?

Dr. TALEB. Well, I am under oath and I will say exactly something that I want to be on the record. I was a trader for 21 years, and every time I said what if we blow up, he said, who cares, the government bails us out. And I heard that so many times throughout my career, that, "don't worry about extreme risks, worry about down five percent, ten percent, don't worry about extreme risks, they are not your problem anymore, it is not our problem." I heard that so many times, and here I am under oath and I say it.

Dr. BOOKSTABER. If I may add to that, there is the notion, well known, of what is called the trader's option. The trader's option is, I get X percent of the upside and limited or zero of the downside, but that trader's option extends also in many cases to the management of the firms. They get the upside and so you would much rather, you know, construct a position that makes a little, makes a little, makes a little and makes a little and with small probability loses everything, because that increases the chance that you have consistent earnings, consistent bonuses, and in the extreme events, your downside is limited because of the option characteristic of your compensation.

Mr. BROUN. So in the ten seconds I have left, I just want to state that taking away the government safety net is going to make people more responsible and they will make better decisions on a real risk management basis, and I thank you all. It is my opinion that that is what I am getting from you all, correct?

Dr. TALEB. In my opinion as well.

Dr. BOOKSTABER. If I may, I would just say, it is not just the safety net. If I am an individual in a firm, I don't care about the safety net, I care about my own bonus, so with or without the safety net for the firm overall, if my incentives are, I make money if things go up, I get a new job if things blow up, I don't know that the safety net matters to me personally.

Dr. TALEB. May I respond to this point?

Chairman MILLER. Dr. Taleb.

Dr. TALEB. I agree that if I am a trader, I don't care who is going to bail me out. The problem is that the shareholders don't care when society can bail them out because there is unlimited liability, that shareholders are protected so society bears the rest. So we have three layers: a trader, the shareholder and thirdly, society. So in the end, the free option comes from society.

Mr. BROUN. Thank you, Mr. Chairman.

Chairman MILLER. Thank you.

I think something like 90 percent of American households have a household income of less than $105,000 a year, so for a trader to make $100 million, $120 million does not seem like make a little, lose a lot.

Ms. Dahlkemper for five minutes.

Ms. DAHLKEMPER. Thank you, Mr. Chairman.

I wanted to go back to your statement in terms of some—that maybe some financial products should be banned, and there are some that may argue that banning any financial product is an excessive intrusion into the free market. So if you could just give me your response to that claim.

Dr. TALEB. I believe in free markets but I do not believe in state socialism, okay, and I don't believe—I believe the situation we have had so far is not free markets. It is socialism for losses and capitalism for profits. So if the taxpayer is involved ultimately in bailing out, which the taxpayer should be able to say, I want this product or that product, the risk, okay? You know, my opinion, I am in favor of free markets but that is not my definition of free markets, okay, state-sponsored socialism for the losses and capitalism for the profit—I mean, free market for the profit. That I don't—as a taxpayer, and I am paying taxes.

Ms. DAHLKEMPER. Dr. Bookstaber.

Dr. BOOKSTABER. I think even in a capitalist system, the argument that some products should not go forward or should be banned is a reasonable one for the following reason: that if I construct some new product, and let us say it is a fairly complex product or has a fat tail and it can inflict problems for society, there is a negative externality to that product that is not priced—that is, I sell it, I create it, somebody wants to buy it, but the negative externality is the increased probability of crisis that it causes, and any time that you have a non-price-negative externality is a time that I think even a libertarian would argue you can have government intervention.

THE RISKS OF DIFFERENT TUPES OF INSTITUTIONS

Ms. DAHLKEMPER. Thank you. I also wanted to go back a little bit to the 'too big to fail' subject in terms of the institutions. When we look at the surviving large banks, they are bigger than ever, so where do you know when an institution is 'too big to fail' and how do we restructure these firms?

Dr. TALEB. 'Too big to fail,' you can see it. If anything in nature is bigger than an elephant, it won't survive, and you can see, I am sure anything bigger than a large hedge fund, to me, is too big. But there is one thing here associated with the problem. The reason we depend so much on banks is because the economy has been over-financialized over the past 25 years, over-financialized. The level of debt today in relation to GDP is three times, according to some numbers, even more or less, but three times the level of debt to GDP that we had in the 1980s. So that is rather worrisome. This is why we have 'too big' banks, all right, because it comes with the system. It is a process, you know, that feeds on itself, that is a recursive process. And if we definancialize the economy more, the debt level will come down. Then the discussion about 'too big to fail,' about banks, will be less relevant. I mean, banks' role is not so—you know, banks where I can withdraw money when, you know, when I go to Atlanta and then there is a bank that is used for letter of credit, very useful things for society. And there are banks that trade for speculative reason, banks that issue paper that nobody needs and there are banks, the banking that corresponds to lending, you know, increased lending because a lender makes a bonus based on the size of loans. So if you brought this down, the size of banks would drop dramatically. Particularly, the balance sheets would shrink dramatically, and particularly if we moved the risk away from banks. The banks are more of a utility in the end, and they are hijacking us because a utility with a bonus structure, it doesn't work. As I said here, don't give someone managing a nuclear plant a bonus based on cost savings, okay, or based on profitability. You don't, all right? So the problem is, they are hijacking us because of the dual function of a utility that we need them to have, a letter of credit or something as basic as withdrawing cash, and at the same time they take risks with bonuses. So if we brought down the level of banking, moved the risks more and more to hedge funds, these people are adults, they don't endanger anyone, just make sure they don't get big and have Dr. Bookstaber's rules on, you know, leverage and stuff like that well enforced . . . then the level of—then that problem would disappear. So let us worry more about the cancer rather than worry about the symptoms.

Ms. DAHLKEMPER. Dr. Bookstaber.

Dr. BOOKSTABER. You know, the Treasury came out with some principles for regulation of capital on September 3, and one of the key issues that they mentioned is dealing with 'too big to fail.' I think one of the difficulties is, I don't think we can measure too big to fail. I don't think we know. It is not just a matter of the capital that you have or the leverage that you have. For example, LTCM was a hedge fund and it was a relatively small firm and had $3 billion capital, yet in a sense it was 'too big to fail' because it almost brought down, actually, Lehman along with it, and the Fed had to step in. What matters is how what you are doing weaves in with what other people, what other funds or firms are doing within the economy. So you could have a 'too big to fail' that is not predicated on one institution and what that institution is doing, but it could be based on some strategy or some new instrument, where for anyone from that instrument that strategy is relatively small, but if the exogenous shock occurs in the market and it affects that strategy, it affects so many firms in the same way that it has a substantial systemic effect. And I get back to the point that we don't have the information to even know right now what type of positions or leverage or strategies might have that threading across different institutions.

Ms. DAHLKEMPER. Thank you. My time is expired.

Chairman MILLER. We are about to have 40 minutes of votes shortly so I would like for both Mr. Wilson and Mr. Grayson to have a chance to ask questions. I should just tell the panel that this Charlie Wilson has never had a movie made about him. So far as I know, he has never been in a hot tub. Mr. Wilson for five minutes.

INCENTIVE STRUCTURES FOR TRADES

Mr. WILSON. Thank you, Mr. Chairman.

Gentlemen, good morning. I serve on the Financial Services Committee also and I have to keep pinching myself that really I am in a Science and Technology Subcommittee and so it is hard to realize the conversations we are having. Dr. Taleb, if I could say that, you know, what you said earlier in your testimony about people not being concerned about the success or failure of a firm because they knew there would be a public bailout is frightening. That is certainly not the American way or certainly not the way we want to do business. With those things in mind, I have a couple questions I would like to ask and maybe we can get some of your feeling as to how people would get so far off track, that that would be the thought process. That concerns me.

People have been outraged at the size of the bonuses and especially when we were doing the voting for the bailout. Some of the employees were bailed out, as you all know, with government money, huge amounts of money to the Wall Street firms. Much of the conversation was about firms being 'too big to fail,' and you say that in the bonuses, that is really the motivator for everybody. I would hate to think that there was no leadership that wouldn't try to keep people on the right track rather than money being the only motivator, the true part of it. So can you explain that? And I was going to address this question, if I could, to Dr. Bookstaber if I could.

Dr. TALEB. You would like me to explain how people were handling extreme risks.

Mr. WILSON. I did. That was confusing. I am sorry. I did address that to you but I would be interested in Dr. Bookstaber also. If you would go first, Dr. Bookstaber, please.

Dr. BOOKSTABER. Thank you. I don't think—I don't mean to be cynical, but I don't think that leadership within a financial firm can overcome the incentives that exist, incentives not just including the trader's option, but to do the bidding of the people who have put you in your position, namely the shareholders whose interest is earnings and maybe even earnings quarter by quarter. So I think the way that you have to change things is through the incentive structure of the people who are taking risk in ways that has been widely discussed, and I think it is fairly clear that you don't finally get paid until whatever trade you have put on, or whatever position you put on, is off the books and has been recorded. You can't basically put on positions and get paid based on the flow of those positions until the trade is realized, that is, until the book is closed on that trade. So this is the notion of longer-term incentives. So if you have longer-term incentives, if you have incentives where you can't game the system by constructing trades or portfolios that again make a little, make a little, maybe blow up, then people will act based on those incentives. But the leadership of the firm is always going to have the following statement, that our responsibility is to the shareholders, we have to maximize shareholder value, and then the shareholders, by the way, although in theory they have a vote, in practice don't. And so, you know, you have sort of this—the management pointing towards the shareholders, the shareholders effectively being silent partners within the corporation.

Mr. WILSON. Thank you.

Dr. Talber, am I saying that right?

Dr. TALEB. Taleb.

Mr. WILSON. Taleb. I am sorry.

Dr. TALEB. There are two problems, and I gave two names, a name to each problem. The first one is called, the title of my first book, fools of randomness, *'Fooled by Randomness,'* and other people who believe their own story and actually don't know that they are engaging in these huge amount of hidden risks out of psychological, you know—as humans, we are not good at seeing negative outcomes. We are victims of overconfidence, so we make these mistakes whether or not there is a bonus, is the psychological, the first one. And the second one, I call them 'crooks of randomness,' so there is 'fools of randomness' and 'crooks of randomness,' and you always have the presence of both ailments in a system. Like, for example, when we had LTCM, Long-Term Capital Management, the problem, these people had their money in it, so, visibly, they were not gaming the system consciously, all right, they were just incapable of understanding that securities can take large variations. So there are these two problems. So the bonus, it is imperative to fix the bonus structure, and as I said here, that I don't know any place in society where people manage risk and get a bonus.

The military people, the police, they don't get a bonus. So fix, make sure that he who bears risk for society doesn't have a bonus. Fix the bonus structure that is not sufficient.

Mr. WILSON. One of the things that, you know, we have heard a lot about since the money was invested in Wall Street was that if the big bonuses didn't continue, the firms couldn't necessarily keep the talent. Do you have any comment on that, Dr. Taleb?

Dr. TALEB. I am laughing, sorry, because a lot of these people—in my book there is a gentleman who had $10 million, a big hotshot trader, and when he blew up, he couldn't even drive a car. I mean, you can find better cab drivers. I don't know what you could do with these Wall Street derivatives, high-income people other than use them as drivers but even then, I mean, you can use someone less reckless as a driver. So I don't know what to use them for, honestly. I don't know what is the—I was on Wall Street for 21 years and a lot of people I wouldn't use for anything. I don't know if you have some suggestions. So I don't know what you are competing against, all right, and you have high unemployment on Wall Street, and calling that 'talent' is a real—it is a very strange use of language, people who lost $4.3 trillion worldwide in the profession, and then calling it 'talent.' So there is talent in generating bonuses, definitely, that you cannot deny. Other than that, I don't know.

Dr. BOOKSTABER. There is—on this point, there are people who are not merely talented, but gifted, in areas like medicine and physics and other fields and they seem to get by on some amount of money, $200,000, $500,000, $1,000,000. I don't know that the talent in Wall Street is so stellar that it is worth $50 million or $100 million versus the talent in these other fields. The issue with the talent more is that the structure of Wall Street somehow allows that level of compensation, so if one firm does not allow it, people can move to another firm that does. But if there is a leveling of the field overall so that instead of $20 million people are making $1 million or $2 million, you know, then I think this issue of, you know, 'we will lose our talent' disappears. It has to be done in a uniform way, as opposed to affecting just one firm versus another.

Mr. WILSON. Thank you.

Chairman MILLER. Dr. Taleb, do you want to respond?

Dr. TALEB. Yes, I have one comment. He is making a socialistic argument to limit bonuses. I am making a capitalistic argument to limit bonuses. I am saying if people want to pay each other, they can pay whatever they want. I just don't want society to subsidize bonuses. That is it. I am making the opposite argument coming from—so this is an extreme bipartisan conclusion here where——

Mr. WILSON. We have a few of those here.

Dr. TALEB. If people want to take risk, you know, and two adults can hurt each other financially as much as they want. The problem is, as a taxpayer, okay, I don't want these bonuses.

Mr. WILSON. Thank you. Thank you both.

Mr. Chairman, just one comment if I could. It just seems that we have to try to find a way to legislate maybe some character to Wall Street.

Chairman MILLER. Thank you. I misread the note that said that we would shortly have 40 minutes of votes. We will have votes at around 11:45 and they will last 40 minutes, so I am delighted that we will be able to continue with this panel for Mr. Grayson and for a second round of questioning. Mr. Grayson.

HOLDING WALL STREET ACCOUNTABLE FOR BONUSES

Mr. GRAYSON. Thank you, Mr. Chairman.

We are talking today about what proper incentive structures we should have on Wall Street, and I am wondering if we are talking too much about carrots and not enough about sticks. In fact, people on Wall Street did lose over $4 trillion of our money, and I have seen almost no one punished for it. Don't you think that it would be likely to deter bad behavior and an overly fond view of risks if we actually punished people?

Dr. TALEB. I am not a legal scholar but there has got to be a way to—there is something called malpractice, okay. There has got to be a way where we can go after these people that I haven't seen so far, because people are scared, because Wall Street has 'talents.' These people would run away and go to Monte Carlo or something, so we are afraid of letting them, you know, of them running away, but we should be doing it immediately, find people who made [these losses]—like the Chairman of an executive committee or the firm that we had to support who made $120 million of bonuses, and supervised unfettered risk taking and made sure that that gentleman got returns of $120 million bonuses. The place where my idea was most popular was Switzerland. The first event of a clawback in any country took place in Switzerland, where the authorities went to Mr. Marcel Ospel, head of UBS, after the events of October and told him, listen, give us 12 million Swiss francs, please, and it was voluntary and he gave back almost—a large share of his—but he clawed back his bonuses.

Mr. GRAYSON. But it was voluntary only because the government intervenes by limiting people's liability. The concept of liability is determined by our law, not by the free market. In fact, if we were to say that we will not give people the right to hide behind corporate shields, wouldn't that have a dramatic effect on holding people accountable for the bad decisions that they make?

Dr. TALEB. To answer, okay, this is still the same problem, fooled by randomness or not fooled by randomness. Some people I have seen in Chicago trade their own money and lose huge amounts of money, not knowing they could lose it, so someone whose net worth is $2 million loses $2 million and had to go burn his house to collect insurance money. So I have seen that. It is not just—so people sometimes engage in crazy trades, okay, where they have liability themselves. It may not be sufficient, but it would be, for me, economically, a good way to have a bonus compensated by malice because capitalism is not just about incentives, it is about punishment.

Mr. GRAYSON. When you say it wouldn't be sufficient, all you are really saying is that it wouldn't solve the problem for all time, forever in every case, but it would certainly be a step in the right direction.

Dr. TALEB. Oh, it would be imperative, not a step.

Mr. GRAYSON. Imperative?

Dr. TALEB. It is an imperative.

Mr. GRAYSON. Okay. Now, Dr. Bookstaber, I understand that in Sweden, the bank managers have unlimited liability for the mistakes that they make, but what happened in our system with regard to blow-ups, with regard to crazy risks that people take in order to pad their own pockets, what effect would that have if we were to take that law and introduce it in America?

MALPRACTICE IN RISK MANAGEMENT

Dr. BOOKSTABER. You know, something along those lines that I have advocated is to have the potential of penalties for the risk managers within a firm similar to what are there for the CFO of a firm. You know, if a CFO knowingly allows some accounting statement to go out, where he knows it is incorrect, he is on the hook not just from a civil but from a criminal standpoint. If you had the risk managers have to sign on the dotted line, that the risk—that they have executed their function correctly, and all material risks have been duly represented—I think that could go a long way towards solving the problem, because they would then have an incentive to make sure everything is right. And there are cases, I think, as we go back to this last crisis, where the risk managers were in some sense not up to the task, or possibly in bed with the people involved in trading or with senior management, to where they were willing to have their views overridden—because they had no liability on the one side, and they didn't want to get fired on the other.

Mr. GRAYSON. But don't we have to do more than that? Don't we have to not only say to people, you have to fill out these forms properly and you have to disclose, but we have to actually hold people accountable for the mistakes that they make, and hold them personally accountable? Isn't that what we need to actually deter this kind of misconduct?

Dr. BOOKSTABER. I guess the question is what type of mistake, because everybody makes certain types of mistakes. I think that sort of mistake where you can hold people accountable is where they—obviously if they knowingly misrepresent—but where there is something material that they—on the one hand it is a malpractice where you say, you know, somebody doing this job in a reasonable way should have discovered that.

Mr. GRAYSON. But let us talk about the specific problems we have seen time and time again in the last few years. Let us talk about, for instance, AIG. In AIG, the fundamental problem is that the traders entered into literally billions upon billions of dollars of heads, I win, tails, you lose bets, bets that couldn't possibly be made good on by anybody but the U.S. Government, and that wasn't a problem of not filling out the form properly, not disclosing. Don't those people need to be punished in order to deter that conduct in the future?

Dr. BOOKSTABER. Well, this gets to Dr. Taleb's point that you would have to go into the mindset of the people. Was it, as he is saying, you know——

Dr. TALEB. Crooks or fools.

Dr. BOOKSTABER. Yeah, were they crooks or fools. If you can discern one from the other, then I agree with you, but what I am saying is, you could also go one level higher to require, which now is required, risk management oversight for those functions where it is believed to be credible, and these were supposed to be the people who know how to do their job, and they have the responsibility to represent that this type of event is not occurring.

Mr. GRAYSON. Dr. Taleb.

Dr. TALEB. Yes. Well, the problem I saw and I wrote about, actually, in one of my writings not yet published, I say it is easier to fool a million than fool a person and it is much easier to fool people with billions than to fool them with millions. Why? Because you have bandwagon effects, and you have collective—something called diffusion of collective responsibility, and I will tell you exactly why. If you have—what risk managers are doing is to make sure they do exactly what other risk managers do. If there is a mistake, it is a mistake that they did not commit individually, but committed—that had company on that. We call it 'company on a trade.' It is not like an individual doctor who is just incompetent. It is collective incompetence. We had collective risk management incompetence, but they were all doing what other people—the hedge is to do what the other guy is doing and that, I don't know if, you know——

Chairman MILLER. Well, the note I got earlier was incorrect and now it appears we are going to have votes at any moment, so I will start a round of questions and we will try to keep it—I know that everybody would like to ask questions of this panel.

CLAWBACK PROVISIONS

Just one—it is not clear to me whether you actually supported a legal requirement that there be clawback provisions in bonus contracts, that if a bonus is based upon a profit this year, that if the very same transaction results in a loss in two or three years there be requirement, a legal requirement that that bonus be repaid. Dr. Taleb?

Dr. TALEB. Indeed.

Chairman MILLER. You do——

Dr. TALEB. Indeed.

Chairman MILLER. Dr. Bookstaber.

Dr. BOOKSTABER. I don't know that I would go to the extent of having it be a legal requirement. Ideally, it should be requirements placed on the corporation by the equity holders, because it makes good economic sense. I think the issue of it being a legal requirement gets into the question of, okay, if we are ultimately the ones holding the bag if this fails, we now have a societal obligation. But I think whether it is done through the shareholders or if it is legislated, that type of structure, incentive structure, clearly makes sense for trading.

Chairman MILLER. Dr. Taleb.

Dr. TALEB. There is an additional problem other than the clawback. There is the fact that if in any given year, I take $1 million from you, okay—say I win, I get my bonus, and I lose, you keep all the losses, so that clawback situation doesn't solve the free option problem. You are solving the mistiming problem, you are not solving the free option problem.

So we have two problems with bonuses. The first one is symmetry. In other words, I make, all right, either a big bonus or nothing, whereas if he loses, I take his money, risk his money. He loses or makes [money], all right, I just make [money], I just earn. So that problem is not solved with the clawback. For example, say the TARP money we gave Goldman, all right—okay, let us forget about clawbacks. Had they lost money, all right, it would have been—we would have eaten the loss. If they made money, they kept the bonuses, okay, so that idea of having just profits and never losses, net, net . . . the clawback is about repaying previous bonuses, but it doesn't address the vicious incentive of paying someone for the profits and not charging him for the overall losses, and the clawback doesn't solve that.

Chairman MILLER. Are you suggesting that that should be prohibited by law, or should people just have better sense than to agree to that kind of compensation system?

Dr. TALEB. In other words, people should have skin in the game. Net, net, net, if I fail, I should be penalized personally some way or another. Don't have an option where I only have the profits and none of the losses.

Chairman MILLER. I am still not clear if you are suggesting that that be a legal requirement or there simply should be a change in the culture, that anyone who agrees to a hedge fund compensation of 220 is a fool, and if people stopped agreeing to it, the compensation system would change.

Dr. TALEB. No, to me, it should be only a legal requirement wherever TARP or a possible society bailout is possible. If there is no society—if someone signs no society bailout, then no.

CREDIT DEFAULT SWAPS

Chairman MILLER. I asked the question earlier but I am not sure I got a clear answer. Do you think credit default swaps should be banned? If not, do you think they should be limited to—they should have a requirement that would be comparable to the requirement of an insurable interest in insurance law?

Dr. BOOKSTABER. I agree with the latter. I don't believe that credit default swaps should be banned, because they do have economic function in the sense that—if I have the debt of a company and perhaps it is illegal, or for some reason it is difficult for me to undo my risk by selling it, I can use the swap to mitigate or hedge my risk. But I don't think that it should turn into what it has turned into—basically, a gambling parlor of side bets for people who have no economic interest at all in the underlying firm. The point you mentioned, Mr. Chairman, in your opening remarks, that the number of people doing side bets far exceeds those who actually have an economic reason to be taking that exposure.

Chairman MILLER. Dr. Taleb.

Dr. TALEB. Mr. Chairman, these products are absurd. They are class B products for me, for the simple reason that it is like someone buying insurance on the Titanic from someone on the Titanic. These credit default swaps, you buy them from a bank, so they make no sense. And I have been writing about these class B instruments that have absolutely no meaning and I don't believe that they have economic justification other than [to] generate bonuses.

Chairman MILLER. The other analogy I have heard is buying insurance against a nuclear holocaust; if you think you are going to be around to file a claim, who do you think you are going to file it with. I will give up my own time; Dr. Broun.

WERE THE BAILOUTS AND STIMULUS FUNDS NECESSARY?

Mr. BROUN. Thank you, Mr. Chairman. Do you believe that bailing out banks and transferring debt from private sources to public sources is a responsible action?

Dr. TALEB. I mean, my opinion is, I am going to be very, very, very honest—it is irresponsible because we have levels of about $60 trillion, $70 trillion worldwide in excess debt that is being slowly transformed into something for our children. If a company goes bankrupt, that debt disappears the old-fashioned way or it turns into equity. If government bails out a company, it is a debt that our children and grandchildren will have to bear. So it doesn't reduce debt in society, and this is why I have been warning against the stimulus packages and all of these. Transforming private debt into public debt is vastly more vicious than just taking the pain of reducing the level of debt.

Mr. BROUN. Dr. Bookstaber.

Dr. BOOKSTABER. In the abstract, I don't think that makes sense. In the current crisis, I think it was inevitable, because we had to adjust for problems that got us to where we are. So I would say we would want to construct a system with regulatory safeguards, with adequate capital, with correct incentives so that the event doesn't occur where we have to move into the bailout mode that we had in the recent past. But my sense is that if we hadn't taken this action, as distasteful and costly as it may be, the end results for the economy may have been far worse.

Mr. BROUN. So you believe that stimulus spending and debt accumulation and the bailouts are all necessary responses to this economic crisis, is what I am gathering.

Dr. BOOKSTABER. Yes, I believe they were for this crisis. I don't believe that as a general principle it is something that we want to occur, and hopefully we can take steps so that it doesn't occur again.

Mr. BROUN. Dr. Taleb.

Dr. TALEB. I don't believe in deficit spending for the following reason, and it comes from the very same mathematics that I used to talk about tail risks. We live in a very nonlinear world—as you know, the butterfly effects, a butterfly in India causes a rainstorm in Washington. You know, these small, little—we don't quite understand the link between action and consequences in some areas, particularly monetary policy. So if you have deficit spending, it is debt that society has to repay someday, okay? You depend a lot more on expert error and projections. I showed in *"The Black Swan,"* in my book, *"The Black Swan"* from 27,000 economic projections, that an astrologist would do better than economists, including, you know, some people here who are economists making projections. So I don't want to rely on expert projections to be able to issue a stimulus and say oh, no, no, look what will happen by 2014, we will be paying it back. These are more of the huge errors.

So what is the solution? The solution is going to be that all this, all right, may lead to what governments have been very good at doing for a long time—printing, okay. And we know the consequences of printing; everybody would like to have a little bit of inflation but you cannot. Because of non-linearities, it is almost impossible to have the 3.1 percent inflation everybody would love to have. You see, a little bit of error could cause hyperinflation, or if you do a little less, maybe it would be ineffective. So to me, deficit spending, aside from the morality of transferring, you know, private debt into my children's debt—okay, aside from that, because someone has got to buy that bond, okay, the way it may lead—you know, because of error in projection—[is] into printing of money.

Mr. BROUN. So from my previous questions as well as others', I take it that both of you all would agree, looking in the future, not only with this economic crisis but in the future, to prevent other economic crises, the real solution is to take away the taxpayer safety net which was implied and now with Freddie and Fannie is express taxpayers being on the hook for this mismanagement and their bad decisions. Would you both agree, yes or no, that taking away that safety net will help people be more responsible, and we will have more of the sticks that my colleague was talking about and that they can within their own company just to protect their own company's viability, et cetera, will put in place more responsible risk management and they will make better decisions. Would you both agree with that statement?

Dr. TALEB. I agree with the statement, remove the safety net.

Dr. BOOKSTABER. I don't know that I can say yes or no because I have to envision what the future world looks like. If we make no changes in terms of regulation and oversight, then I wouldn't agree with the notion of taking away the safety net because we have a flawed system where there is a notion of 'too big to fail' . . . where if certain institutions do fail, it has severe adverse consequences for people on Main Street. I think that we have to say, we want to get rid of the safety net, and to do that we need to get the corrective incentive structures, the correct level of oversight from regulators, the right capital requirements. So as an end result, that is where I believe we should go, but I don't think we can be there in good conscience for the typical citizen without doing a better job, you know, in the regulatory arena.

Dr. TALEB. I don't understand this logic because I don't see how—in 1983, when banks were bailed out, and even one of them was the First National Bank of Chicago. It set a bad precedent. Every time I heard the same argument, you hear the same argument, "this is necessary, society can't function, but in the future we'll make sure we don't do it again." I don't understand this argument.

Mr. BROUN. Thank you, Mr. Chairman.

Chairman MILLER. Ms. Dahlkemper? Okay, Mr. Wilson?

Mr. BROUN. I think we need to go vote.

Chairman MILLER. We have been called to our votes. Thank you very much to this panel. We will be gone for about 20 minutes, not 40 minutes as I earlier understood. But at that point it does make sense to excuse this panel, but thank you very much. It has been very helpful and even entertaining. And then when we come back, when we return we will have the second panel, although these are the last votes of the week so it is possible some Members will not come back but go straight to the airport. Thank you, and we will be at ease.

[Recess.]

Panel II:

Chairman MILLER. Other Members may return or may not, but I think we should begin the second panel, and I also mean it when I say that this panel is unusually distinguished. Our witnesses are leading experts in their respective fields. Dr. Gregg Berman is the Head of Risk Business at RiskMetrics Group, which is the present-day descendant of the group at J.P. Morgan that created the Value-at-Risk methodology. He has worked with many of the world's largest financial institutions on the development of risk models. Mr. James Rickards is the Senior Managing Director of the consulting firm Omnis Inc., is a former risk manager and investment banker who has been involved in the launch of several hedge funds. As general counsel of Long-Term Capital Management during the 1998 crisis, he was the firm's principal negotiator of a bailout plan that rescued it. And Mr. Christopher Whalen is the Managing Director at Institutional Risk Analytics, a provider of risk management tools and consulting services. He volunteers as the Regional Director of the Professional Risk Managers International Association, and edits a weekly report on global financial markets. And finally, Dr. David Colander, the Christian A. Johnson Distinguished Professor of Economics at Middlebury College, has written or edited over 40 books, more than 40 books, including a top-selling Principals of Economics textbook and more than 150 articles on various aspects of economics, including the sociology of the economics profession.

You will also have five minutes for your oral testimony, your spoken testimony. Your written testimony will be included in the record for the hearing. When you have completed your spoken testimony, when all of you have, we will have rounds of questions from the Members who are here, which may include me repeatedly. It is the practice of this subcommittee, as you saw earlier, to receive testimony under oath. Again, I don't think any of you have to worry about perjury. That would require that the prosecutor prove what the truth was, beyond a reasonable doubt, and that you knew what the truth was beyond a reasonable doubt. Do any of you have any objection to swearing an oath? Okay, and I think you may sleep easy tonight without worrying about perjury prosecution. You also have the right to be represented by counsel. Do any of you have counsel here? And all the witnesses said that they do not. If you would now please stand and raise your right hand. Do you swear to tell the truth and nothing but the truth?

The record will show that all the witnesses did take the oath. We will begin with Dr. Berman.

STATEMENT OF DR. GREGG E. BERMAN, HEAD OF RISK BUSINESS, RISKMETRICS GROUP

Dr. BERMAN. Thank you. I would like to begin by thanking the Committee for this opportunity to present our thoughts on Value-at-Risk and banking capital, especially in the context of the present financial crisis.

My name is Gregg Berman and I am currently the Head of the Risk Business at RiskMetrics Group. I joined as a founding partner 11 years ago when we first spun off from J.P. Morgan, and throughout that time I have had a number of roles, from leading research and development to leading product design, but mostly spending time with clients, and those clients include some of the world's largest hedge funds, largest asset managers and certainly the world's largest banks. During that time, and even under oath I feel I can say this, I have not traded any derivatives in any way, shape or form.

My comments today revolve around three essential points. First, Value-at-Risk, or simply 'VaR,' was created about 15 years ago to address issues faced by risk managers of large, multi-asset, complex portfolios. The purpose of VaR was to answer the question: how much can you lose? In this context, it has actually enjoyed tremendous success, ranging from revealing the hidden risks of complex strategies to communicating with investors in a consistent and transparent fashion.

Second, VaR is a framework. It is not a prescriptive set of rules. As such, it has been implemented in many different ways across a wide variety of institutions. Criticisms of VaR that focus on the use of normal distributions or poor historical data must be taken in context. These issues are often the results of specific VaR implementations that may not have kept up with the best practices in the community.

Third, most VaR methodologies utilize recent market data to estimate future short-term movements in order to allow risk managers to make proactive decisions based on rapidly changing market conditions. This is what VaR was designed to do. Research shows that these estimates are indeed quite robust, but they are not designed to predict long-term trends, and they are not designed to operate when the markets themselves stop functioning. Banks, on the other hand, must be protected against adverse long-term trends and in situations where the markets actually stop functioning. This, therefore, is not the domain of Value-at-Risk.

So how do we tackle this problem? We start by noting that the current crisis is driven by two primary factors: one, the failure of market participants and of regulators to acknowledge and prepare for large negative long-term trends, such as a decline in home prices or buildup of leveraged credit, coupled with, two, the failure of many institutions to accurately and completely model how these negative long-term trends would actually affect their financial holdings. In this context, I am using the word "model" to mean a mathematical representation of a security or derivative that shows how its value is driven by one or more underlying market factors. Since both of these issues were quite well known for quite long periods of time, it is very hard to say that this crisis was unforeseeable, unknowable or a fat-tailed event.

All market participants, including banks, must do a better job at modeling complex securities and in understanding how their strategies will fare under changing market conditions. For example, if the holders of mortgage-backed bonds would have known how sensitive these assets were to modest changes in default rates, they may not have purchased them in the first place. New rules, regulations and other types of policy changes regarding better disclosure in data must be done in order to address this critical issue.

But it is banks and regulators who must specifically focus on preparing more for the negative long-term trends that lie ahead and less on trying to predict things with probabilities. Though current VaR methodologies are designed to estimate short-term market movements under normal market conditions, regulators nevertheless try to recast these models in order to measure the probability of long-term losses under extended market dislocations. We propose that it is not the model that needs to be recast, but that regulators need to recast the question itself.

VaR is about making dynamic decisions, constructing portfolios, sizing bets and communicating risk. On the contrary, banking capital is more like an insurance policy designed to protect against worst-case events and their consequences. Instead of having banks report probabilities of short-term losses, banks should estimate the losses they would expect to sustain under a set of adverse conditions chosen by regulators. The question of 'how much can you lose' is thus changed to 'how much would you lose.'

The conditions that banks are tested against should depend on what type of events policy-makers in the public interest believe that banks should be able to withstand. In this fashion, models, probabilities, simulations and predictions are left to those making ongoing risk-reward business decisions, whereas the minimum levels of capital needed to ensure a bank's survival are based on how regulators implement the broader requirements of policy-makers. Perhaps one bank needs to survive a 100-year flood whereas an orderly liquidation is all that is required for a different bank. Perhaps all banks should be able to weather a further ten percent downturn in housing prices, but no bank is required to survive a 50 percent default rate or a 40 percent unemployment rate—not because these events are highly improbable, but because policy-makers decide that this is too onerous a burden for a bank to bear.

In summary, VaR is an excellent framework for active risk management by banks and other financial institutions and the development of risk models must continue unabated. But banking capital serves a different purpose, and a resetting of expectations will allow for the development of much better solutions driven by policy instead of by probability. Thank you.

[The prepared statement of Dr. Berman follows:]

PREPARED STATEMENT OF GREGG E. BERMAN

I'd like to begin by thanking the Committee for this opportunity to present our thoughts on Value-at-Risk and banking capital in the context of the present financial crisis. My name is Gregg Berman and I am currently the head of the risk business at RiskMetrics Group, a provider of risk and corporate governance services to the financial community. I have been at RiskMetrics since its founding 11 years ago and in the last decade have worked with many of the world's largest financial institutions on the development of risk models, their use by hedge funds, asset managers, and banks.

SIMPLE ROOTS OF A COMPLEX CRISIS

My comments today start with a rather bold assertion—the current crisis was not unpredictable, unforeseeable, or unknowable. In that sense I'm not sure it should be classified as a fat-tailed event. Rather, it was caused by the coupling of two fundamental problems, namely:

1. the inability of market participants to acknowledge and prepare for the consequences of long-term trends, such as a protracted downward spiral in home prices, or a leveraging of the credit market through the use of CDS, and
2. the inability of market participants to recognize the economic exposures they had to those trends through holdings such as asset-backed securities and derivative contracts.

The fact that these issues went unchecked for many years led directly to the creation of multiple, unsustainable market bubbles, which when burst propelled us downwards into a full-blown crisis.

But if my assertion is correct and these events were foreseeable, then what does that imply about all the financial models and risk methodologies that were supposed to monitor and protect us from such a crisis? It is the answer to this question that I'd like to explore.

THE INEVITABILITY OF VALUE-AT-RISK

In the early days of risk management size was used as a primary measure of risk. After all, intuition tells us that $10,000,000 in Ford bonds should be ten times riskier than $1,000,000 in Ford bonds. But soon the market realized that $10,000,000 of Ford bonds is probably riskier than a similar value of government bonds, but not as risky as $10,000,000 of Internet start-up equity.[1]

To address these issues practitioners switched from asking "how large is your position" to "how much can you lose." But there is not just one answer to that question since for any given security differing amounts can be lost with different probabilities. One can estimate these probabilities by polling traders, by building econometric models, by relying on intuition, or by using variations of history to observe relevant patterns of past losses. Each of these methods has their own benefits and weaknesses. And unless we consider only one security at a time, it will also be necessary to make estimates of how the movements in each security are related to the movements of every other security in a given portfolio.

These concepts are encapsulated by two well-known statistical terms: volatility and correlation. If one could measure the volatility and correlation of every security in a portfolio the question "how much can you lose" could be meaningfully addressed. This process is the basis of a popular risk methodology known as Value-at-Risk, or VaR.

HOW VaR IS COMPUTED AND HOW IT IS USED

Because security valuations are often driven by underlying market factors, such as equity prices, spreads, interest rates, or housing prices, VaR is usually calculated in a two-step process that mimics this behavior. In the first step a model for the economic exposure of each security is created that links its value to one or more of underlying market factors. In the second step future trends of these underlying factors are simulated using volatilities, correlations, and other probabilistic methods. These two steps are then combined to create a curve that plots potential profits-and-losses against the probability of occurrence. For any given curve VaR is defined to be the amount that can be lost at a specific level of probability. It is a way of describing the entire profit-and-loss curve without having to list every data point.[2]

The accuracy of any VaR number depends on how well underlying markets have been simulated, and how well each security has been modeled. There unfortunately exists a tremendous variability in current practices and different financial institutions perform each step with varying levels of accuracy and diligence.[3] Deficiencies

---

[1] The matter is further complicated by derivative contracts that do not even have a well-defined measure of size. For example, what is the size of a contract that pays the holder $1,000 for each penny-increase in the average spread throughout September between the price of natural gas for delivery in November and the price for delivery in January? Technically the answer is zero since the holder owns no natural gas, but the risk is certainly not zero.

[2] Exhibit 1 on page 10 shows the potential one-day profit-and-loss distribution of selling a short-term at-the-money put on the S&P 500. Out of 5,000 trials we see that about 50 of them have losses of 250 percent or worse. Thus VaR is 250 percent with a one percent probability. Alternatively we can ask for the worst five out of 5,000 trials (a 0.1 percent probability) and observe these losses to be 400 percent or worse.

[3] The marketplace is rife with common fallacies about VaR due to poor implementations. When VaR first became popular in the mid-1990's computing power limited how accurately instruments, especially derivatives, could be modeled. Approximations that relied on the use of

Continued

in how VaR is implemented at a particular firm should not be confused with limitations of VaR itself.[4]

But indeed there are limitations. When computed according to current best practices, VaR is most applicable for estimating short-term market volatilities under "normal" market conditions. These techniques are based on over a decade of well-tested research demonstrating that in most circumstances recent market movements are indeed a good predictor of future short-term volatility. VaR models have seen tremendous success in a wide range of applications including portfolio construction, multi-asset-class aggregation, revealing unexpected bets, investor communication, the extension of margin, and general transparency.

As such, VaR has become an essential part of risk management, and when properly integrated into an overall investment process it provides an excellent framework for deploying capital in areas that properly balance risk and reward.

VAR AND BANKING CAPITAL

So why did this not foretell the current crisis? First and foremost, many institutions and market participants did not perform step one correctly—they failed to correctly model how their securities would behave under changing market conditions. This failure is one of the leading causes of current crisis.[5]

The second issue is where banking capital comes in. Recall that our crisis stems from long-term trends, not short-term volatility. And as mentioned, most of today's VaR techniques are only applicable for estimating potential short-term movements in well-functioning markets.[6] But it is long-term trends and non-functioning markets that are the concerns of banking capital.

Nevertheless regulators rely on VaR as the basis for many bank capital calculations.[7] And even today they continue to recast VaR-like models in order to address

---

so-called normal distributions (bell-shaped curves) were often required. Also, the amount of market data that could be used, and the frequency at which this data was updated, was limited by technical and mathematical challenges resulting in further approximations. However, by the early part of this decade many of these challenges were overcome and today's simulation techniques do not rely on normal distributions and are not restricted by limited data. Unfortunately many institutions with older implementations still use somewhat outdated and approximate methods that do a poor job in estimating the risk of multi-asset, derivative-heavy portfolios.

[4] One fundamental criticism of VaR is that it can be "gamed" or manipulated since one number cannot by itself represent or reveal all possible "tail-loss" events. This is easily rectified by simply asking for VaR numbers at more than one level of probability, by computing the average of all losses comprising a tail event (often called conditional VaR or expected loss), or by examining the entire distribution of estimated future losses and their corresponding probabilities.

[5] Many institutions and market participants did not recognize nor understand how their portfolios and strategies would be affected by a fall in housing prices or a widening of credit spreads. Regulators had even less information on these effects and almost no information on how they were linked across institutions.

It could be argued that if investors had understood the nature of the mortgage-backed products they had purchased, many would not have purchased them in the first place (which would have significantly curtailed the formation of the bubble itself). If regulators had understood how CDS contracts inherently lever the credit markets they may not have allowed their unbridled expansion. And if insurance companies understood that changes in the mark-to-market values of their derivative contracts would require the posting of collateral to their counter-parties many would not have entered into those deals. None of these decisions involve predicting the future or modeling fat tails. They do involve understanding the present, spending time on the details of financial instruments, and being incented to care about their risk.

Tackling these significant shortcomings may require new regulations regarding data availability, disclosure, and the analytical capabilities of each market participant. Central oversight of the markets themselves will be needed to monitor, and sometimes even limit, actions that could trigger systemic risk and future liquidity crises.

[6] There is nothing endemic to VaR that limits its applicability to short-term estimates or functioning markets. However, current methodologies are optimized for those conditions and this is where most parameters have been tested for proper use. Research into new models that lengthen the prediction horizon and include factors like liquidity to account for non-functioning markets is underway. As development of these methodologies progresses we may see the domain of VaR extended into more areas of risk.

[7] One technique employed to "fix" the short-term aspect of VaR models is to utilize long-term historical data as the basis for "better" future estimates. This is a very common but dangerous practice since it both invalidates any estimates of short-term volatility (preventing proper use by risk managers trying to be reactive to rapid changes to the market) and it doesn't actually provide any better estimates of long-term trends. For a complete discussion on this and other related topics see included reference by Christopher Finger (*RiskMetrics Research Monthly*—April 2009) and references therein (including a March 2008 report issued by the Senior Supervisors Group on their study of how risk was implemented at a variety of large banks).

VaR's perceived shortcomings.[8] We propose that it is not the model that needs to be recast but rather the question that regulators want the model to address.

POLICY-BASED BANKING CAPITAL

We believe that the foundation of banking capital is rooted in the following two questions:

1) What are the adverse events that consumers, banks, and the financial system as a whole, need to be protected against?
2) What is required from our banks when those events occur?

This is not the domain of VaR. On the contrary, banking capital is more like an insurance policy designed to protect against worst-case events and their consequences. Instead of having banks report probabilities of short-term losses, banks should estimate the losses they would expect to sustain under a set of adverse conditions chosen by regulators. The question of "how much can you lose" is thus changed to "how much would you lose."

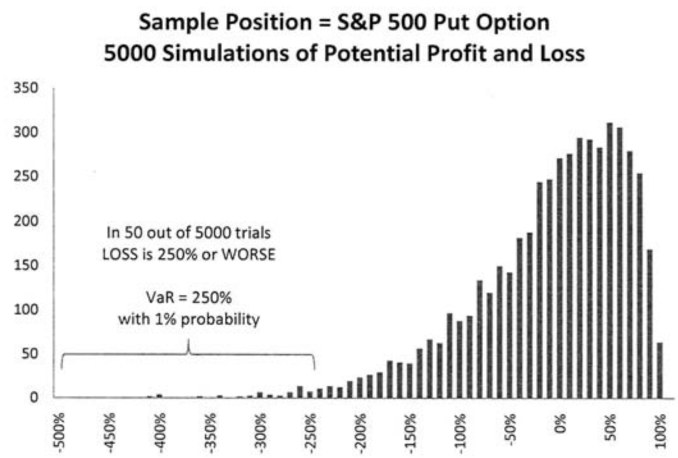
The conditions that banks are tested against should depend on what types of events policy-makers decide that, in the public interest, banks should be able to withstand. In this fashion models, probabilities, simulations, and predictions are left to those making ongoing risk-reward business decisions whereas the minimum levels of capital needed to ensure a bank's survival are based on how regulators implement the broader requirements of policy-makers. Perhaps one bank needs to survive a hundred-year flood whereas an orderly liquidation is all that is required for a different bank. Perhaps all banks should be able to weather a further 10 percent downturn in housing prices, but no bank is required to survive a 50 percent default rate or a 40 percent unemployment rate—not because these are highly improbable, but because policy-makers decide that this is too onerous a burden to expect a bank to bear.[9]

To summarize, we believe that key differences between the needs of risk management and banking capital suggest different solutions are required. And in doing so each field can separately develop to meet the ever-expanding array of challenges we face today.

[8] See included reference by Christopher Finger (*RiskMetrics Research Monthly*—February 2009) containing our comments on the Basel committee's proposed Incremental Risk Charge—an extension that uses VaR for additional types of capital charges.

[9] The recent stress-tests conducted on banks by the Federal Reserve is an excellent example of how policy, as opposed to probability, can help set capital requirements. This should not diminish the role of simulations and the use of models to explore possibilities and uncover unexpected relationships, but this should be a guide of what the future may bring, not a prediction of what it will (or will not) bring.

EXHIBIT 1: VAR AND THE PROFIT AND LOSS CURVE



RiskMetrics Group Research Monthly

IRC comments

February 2009

Christopher C. Finger
chris.finger@riskmetrics.com

Amid the financial crisis of the past year, one of the many alleged villains has been the Basel II banking capital standard. Lost in the debate is the not so subtle point that the standard is still evolving, and in fact the Basel Committee is taking steps to address many of the regulation's shortcomings. One such step is the recent publication of the third consultative document on assessing incremental capital in the trading book. Though not finalized, this new regulation represents an important step to closing a gap in the existing capital rules—credit risk in trading portfolios—that has been severely exposed by recent events.

We comment here on the new proposal, from both a practical and fundamental perspective. To do so, we begin with a statement of the Committee's goals: some of which have been explicitly stated, some which we can infer from prior documents and some which we would assert should be goals, even if the Committee is not in a position to state them outright. With a set of broad principles defined, we then proceed to question whether the specific proposal achieves the goals, and to make a few modest recommendations as to how they may be better met.

A bit of history

If the one overarching theme of the first Basel Accord was consistency, that of the second accord must be risk sensitivity, along with the acknowledgement that banks themselves are capable of measuring risks. In fact, Basel II was not the first appearance of risk sensitivity in the capital guidelines: under the 1998 Market Risk Amendment (MRA) to the original Basel Accord, banks were given the option to base part of their minimum capital requirement on their own assessment of the risk of their trading portfolio, subject to certain restrictions and standards.

For the most part, Basel II was notable for extending the internal models idea to banking portfolios. The trading book capital treatment, though updated somewhat, did not change dramatically from how it was set forth in the MRA, under which banks are assessed minimum capital in two parts: general and specific.

The general risk charge is intended to cover risks due primarily to systemic market moves; banks are permitted to use their own models to measure this risk, and capital is defined as the Value-at-Risk (at 99% confidence over a ten-day risk horizon) multiplied by a scaling factor, which is subject to supervisory discretion, but is no less than three. The specific risk charge is to cover the impact of security- or issuer-specific events: spread widening, credit migration, default, and so on. As in 1998, many risk models did not account for such events, the original specific risk charge was a standard supervisory formula. As time went on, more sophisticated banks were able to secure approval to expand their internal model coverage to specific risk.

While Basel II left the general and specific capital charges largely unchanged, it recognized the need to better cover the default risks inherent in trading portfolios, and laid out a number of high level principles for treating incremental default risk. As the principles were presented in 2005, they were not (yet) a reaction to significant trading book losses, but rather a recognition of the amount traded credit risk had grown since the MRA.

In October 2007, the Basel Committee published a consultative document defining an Incremental Default Risk Charge. In response to both industry comments and market developments, the Committee expanded the charge to include migration risks (and consequently renamed it the Incremental Risk Charge, or IRC). The third consultative document, published in January 2009, refines the definition of the IRC, and formalizes the total trading book capital requirement as the sum of the general and specific charges from the MRA and the new IRC.

The IRC itself is defined as the VaR of the unsecuritized credit products in the trading book, at a 99.9% confidence level over a one-year horizon. Recognizing that imposing a one-year holding period is unrealistic for many trading positions, the IRC permits a liquidity horizon for each instrument, with the notion that at this horizon, an instrument may be rebalanced in order to maintain a constant level of risk. Other than the restriction that no instrument have a liquidity horizon shorter than three months, the definition of the model and parameters for the IRC is left largely to the banks.

A set of principles

Though tempting to jump straight to the specifics, it is important to first establish the motivations for the IRC specifically and model-based capital generally. We should begin with the very principle of risk sensitivity: required bank capital should depend on the risks of the bank. Moreover, it is probably inevitable that a risk sensitive capital regime will depend on bank internal models. Taken together, these notions are under fire, with the perceptions that through this crisis, banks were undercapitalized (a fair point) and incapable of modeling their own risks (an overreaction). These perceptions have prompted cries for a "back to basics" approach, but this is dangerous.

If by "back to basics" we mean a return to the simple capital requirements of the original Basel Accord, we should recall what motivated Basel II in the first place. The criticism of the original accord was that it was basic and inflexible, and as such produced an incentive for banks to remove low yielding (but safe) credits from their balance sheet, while retaining higher yielding (but riskier) credits. Such an incentive was no more than an academic point in a world of buy-and-hold credit, but with the rise of collateralized debt obligations (CDOs) and credit default swaps not long after, banks had a ready mechanism to act on those incentives. It is fair to say that the original development of CDOs, the descendants of which wound up at the heart of this crisis, owes to the skewed incentives of the old, basic capital regime. That is not something we should aim to go back to.

If, on the other hand, by "back to basics", we mean a turn to more transparent capital requirements, that is a principle to hold to.

So while we should agree that a transparent, risk sensitive capital regime is still desirable, it is impossible to neglect the point that banks were undercapitalized. So the next basic principle is that we should find ways to increase capital standards, focusing in particular on the trading book, where the significant losses have occurred. At the same time, it is important to be mindful of the rules for banking book capital, as we do not want the capital standards to produce skewed incentives for where particular positions are placed.

An issue that has lurked in the background, but has come to the front in the IRC discussion is the tension between the regulatory risk horizon and the horizon at which banks manage their trading portfolios. From the regulatory point of view, the concern is the bank as a "going concern". The aim of minimum regulatory capital, then, is that banks can withstand losses, and are able to continue their normal activities, without the need to raise additional capital. The dispute between industry and the regulators over the appropriate regulatory risk horizon, in fact, has been essentially a proxy battle over the frequency at which banks are able to raise capital. Whereas back in 2007, early industry responses to the IRC proposals argued for a shorter risk horizon, asserting that banks could raise additional capital if they suffered capital depleting losses, the events of the last year has rendered such arguments void, and it is the regulators with the upper hand in pushing for a longer horizon.

With their going concern, long horizon view, the regulators have put themselves firmly in the camp of seeking models for banking (or trading) *practices*. Regulatory capital, then, is intended to support not just possible losses on existing positions, but on all of the positions throughout the next year, as a bank is assumed to continue its normal operations.

Internal trading risk management, on the other hand, focuses on *positions*. As such, the risk horizon is not the frequency over which the bank can raise capital, but rather the frequency at which positions can be traded. This horizon, particularly with trading portfolios and even accounting for stressed levels of liquidity, is markedly shorter than the capital horizon. It is this problem—short horizon risk on constant portfolios—that market risk managers have focused on for over a decade. And even if the models may be imperfect or incomplete, their short horizons at least allow for empirical validation.

The MRA rules represent a compromise between the regulatory and trading horizons. The regulations recognize that banks are capable of modeling short horizon risks, yet also that the purpose of capital is more than absorbing 99% worst case, ten-day losses. Thus, banks are permitted to estimate their short horizon VaR, and the regulator applies two mechanisms to convert this measure, at least qualitatively, into a long horizon one.

The first mechanism is that, while the bank calculates its VaR daily, it is assessed capital on a sixty-day moving average of the VaR measure; this adjustment is an admission that while the short term risk of a trading portfolio may indeed be quite volatile, the capital required to support longer term trading activities should be smoother. The second mechanism is the multiplier: the short horizon VaR is scaled by a regulatory factor, which is typically between three and five. The MRA rules, then, display a principle of compromise, and are a balance between quantitative risk sensitivity, as measured by the banks themselves, and qualitative prudential standards, as imposed by the regulator.

A consequence of this balance is that capital is not defined in precise statistical terms. Basel defines neither the horizon nor the confidence level they consider prudent, but enforce prudence through their subjective choices of multipliers. The new rules tilt this balance, attempting to define capital in a more rigorous statistical framework. And they impose on banks the task of modeling not just their positions, but their practices, over a horizon long enough to suit the regulator.

Finally, some models, as the saying goes, are useful. But with a capital regime encouraging new model developments, it is not just models that should be useful. We should also require that discussions about models be useful, and that the byproducts of these discussions, be they other models or simply heightened awareness of other issues, be useful as well.

Recipe for trouble

In the industry responses to the prior IRC consultative documents, a consistent complaint has been the lack of an overarching conceptual framework to the new rules. The confusion stems foremost from the definition of capital as the sum of the old MRA capital, based on a ten-day 99% VaR, and the new IRC capital, based on a one-year 99.9% VaR and covering a slightly different set of sources of risk. As with the MRA, there is still no precise statistical definition of capital, but the new rules are more frustrating in that they tease with the illusion of precision (all of the components are defined statistically), and yet the final aggregated number is no more than a recipe, with no conceptual framework to justify it. What in the end does it really mean to take the sum of ten-day 99% and one-year 99.9% VaR?

While only the most ardent modelers (still) believe in precise estimates of such things as a 99.9% one-year VaR, it is not just these zealots crying for a conceptual basis for capital, the better to apply yet more complex techniques. Even the more rational among us should recognize that the lack of a conceptual framework is dangerous, and not just aesthetically bothersome. When we have no conceptual framework, capital in the end is a recipe. When a recipe is simple, such as multiplying VaR by three, it is easy to anticipate how the recipe will behave in new circumstances. When a recipe is more complicated, such as the sum of two inconsistently defined VaR figures, we do not know what we can really expect.

The examples in the paper by Breuer et al (2007) should serve as warning. Just adding the risk measures arising from different sorts of risks does not, as popular wisdom would argue, necessarily produce a conservative answer. Too many of us have gotten too accustomed to the behavior of the simplest of models, where risks add in simple ways: so we propose a recipe that under the simplest of models is guaranteed to behave as we desire, but under realistic models, we have no idea what will happen.

Industry is not blameless here either. One of the early industry counterproposals to the IRC was to add these distinct risks via the standard formula for adding standard deviations, with an "effective correlation" calibrated to match actual model results in select cases. This proposal was no better than Basel's, in that it replaced one recipe for adding risks with no conceptual basis with another more complicated, but equally unfounded recipe. Basel, while not coming up with something richer, was wise at least to discard this industry proposal.

What what else to do?

A better industry proposal sought to bring some consistency to the standard market risk and IRC charges. This proposal was based on the observation that three times the ten-day 99% VaR is close to the VaR for 60 days at 99.9% confidence, assuming a Gaussian distribution and the standard time scaling. Sixty days being a plausible time horizon for overall trading book risk, and 99.9% being consistent with the solvency standard initially proposed by Basel, the industry proposed using a 60-day, 99.9% VaR for all trading book capital. The important consequence of this is that it allows for a single model, encompassing all risk sources, rather than the arbitrary division of risks of the IRC, and the resulting specter of falsely conservative capital rules.

The latest proposal from Basel sticks to the separate capital charges and risk horizons, an apparent blow to the industry proposal. One rationale for the persistent one-year horizon is that Basel wishes to avoid too large a discrepancy between the capital for an asset in the banking book (which is based on a one-year horizon) and that for the same asset in the trading book (the IRC). A second rationale is clearly that Basel desires to raise capital levels by more than the sixty-day horizon would imply.

With the discussions focusing on the risk horizon and solvency standard, it is disappointing that the choice of risk statistic has never been seriously reconsidered. In the consultative document, the Committee makes it clear that a shortcoming of the MRA capital rules is that they are insensitive to the sorts of rare but large events that the IRC is intended to address. Their reaction to this is to push out both the horizon and the confidence level, seemingly raising both the overall capital and the risk sensitivity of the rule.

This approach is misguided. A measure such as VaR—a specific quantile of a probability distribution—is insufficient to distinguish the risk of portfolios with differently shaped distributions. In a 1998, MRA era view, where all distributions are Gaussian (or close) the VaR is really no more than a scaled version of the standard deviation. And if all portfolios are distributed with the same shape, this is enough to differentiate more from less risky portfolios. In this setting, VaR was a sensible choice on which to base a risk sensitive capital regime.

But we are in a different setting now. If our focus is on rare but large events, then the new capital regime should be able to differentiate between two portfolios that differ only in that one is exposed to a rare but catastrophic event. As long as the likelihood of the rare event is significantly less than the VaR confidence level, the VaR of these portfolios is almost equal. One remedy is to make our confidence level stringent enough to cover what we think will be the likelihood of such rare events. But this is a fragile regime, and only risk sensitive in the particular range of probabilities that are defined a priori. A different statistic—in particular expected shortfall, or the expected loss, conditional on experiencing a loss in the tail—produces a measure that is at once sensitive to the risks we desire to measure, and not as sensitive to the arbitrary choice of confidence level.

Constant level of frustration

By far the most controversial element of the Basel proposals has been the notion of an assumed constant level of risk. The constant level of risk and its companion notion, the liquidity horizon, are in a sense concessions by the Committee that a one-year risk horizon with fixed positions is unrealistic for most trading portfolios. This author has been torn on the proposal, in fact praising it in an earlier column.[1]

To rationalize somewhat, our praise at the time was for the constant level of risk notion as the first explicit acknowledgement that with long risk horizons, it is imperative to address the problem of portfolio aging. The assumption of constant positions, while adequate for short trading horizons, is unrealistic in the context of the "going concern" outlook that Basel aims to apply for regulatory capital. We maintain that Basel is due praise for promoting what is the first serious industry discussion of this topic.

Praise is due as well for the emphasis on the liquidity horizon. While its role in this particular modeling framework is debatable, the assessment of a liquidity horizon for trading positions is a worthy goal in its own right.[2] That the liquidity horizon for trading positions is a quantity that can materially impact capital requirements is a guarantee that these will be the subject of significant scrutiny and debate. Regardless of the ultimate application of the liquidity horizon, this debate will necessarily raise awareness and promote transparency of liquidity risks. While not a stated goal of the IRC proposal, this transparency of liquidity is a desirable side benefit, and discussions thereof most definitely useful.

The constant level of risk framework, on the other hand, is an invitation to arbitrary recipes, and for the more cynical, to manipulation of the minimum capital through unneeded complexity. In a utopian world, we would build a risk model that simulated each asset through time. For each asset, at the appropriate liquidity horizon, we would sell the asset, realizing a gain or loss, and reinvest the proceeds. The reinvestment would be into a new hypothetical asset with the same characteristics as the original position at initiation. The motivation for such a rebalancing strategy is that it is expected to reduce risk relative to the strategy of holding all positions for an entire year.

But the effects of the rebalancing strategy are subtle, and it is unclear when the strategy actually does reduce risk. Among other things, the outcome depends on three features of the utopian simulation:

- Structure of credit (or risk) curves. It is a common that an issuers' credit curve is upward sloping, that is, that if a default event is to occur, it is more likely to occur in the long term than in the near future. If this is true for most issuers, then rebalancing reduces our risk by exposing us to a succession of "near futures" rather than a single long term.
- Inter-period correlations. In efficient markets, we can typically assume (and validate) that market moves in one period are uncorrelated with market moves in the next. But with ratings migrations and defaults, it is much more difficult to argue market efficiency, and ef-

[1] See Finger (2008).

[2] Indeed, in a parallel consultative process, Basel has issued a set of principles for sound liquidity risk management.

fects such as ratings momentum are well documented.

- Risk dynamics. It is acknowledged that both default probabilities and volatility vary with time. Thus, it is not certain that the risk of a BBB position in six months, after my initial BBB position has downgraded, will in fact be a constant level of risk.

In liquid markets, where risk factors evolve continuously (or approximately so), it is plausible that we might defend such a set of assumptions. But ratings migrations and defaults do not evolve continuously, and what is more, are subject to very human, discrete processes: when does a firm choose to file for bankruptcy, or an analyst to review a rating. When migrations and default are the risk factors, clarifying a model with rebalancing will always come down to some articles of faith. And when capital depends in complex ways on articles of faith, it is naive not to believe that faith could well be corrupted.

It is naive as well to believe practical a utopian simulation with asset-by-asset rebalancing; any implementation of an IRC model will be a mere approximation to this ideal. At least in the special case where there is a single liquidity horizon, the consensus emerging in the industry is to obtain the one-year distribution by summing successive short-horizon portfolio losses: for a liquidity horizon of three months, we define the three month portfolio loss distribution, then sample four times from this. Embedded in this operation is the assumption that in our succession of "near futures", each period is independent of the next. Mathematically, this procedure amounts to computing a convolution of the short horizon loss distribution.

The convolution approach, while tractable and seemingly consistent with the spirit of the IRC definitions, is problematic. Most importantly, the approach does not define a model in terms of a process; we have not specified how the portfolio evolves from one period to the next. As such, there is no natural way to generalize the approach to a portfolio with a mix of liquidity horizons, a situation sure to arise in practice. Consider a portfolio composed of three-month and six-month liquidity horizon positions. Applying the convolution to either portion of the portfolio is straightforward, but how do we make sense of the dependence between the six month loss experience on the less liquid subportfolio and the month four to month six loss experience (including hypothetical rebalanced positions) of the more liquid portfolio?

In the end, the convolution approach, which appears to be the only tractable way to address the constant level of risk proposal, is in truth no more than a scaling rule, albeit an opaque and complicated one.

A humble proposal

Acknowledging the basic principles and goals of the IRC proposal, we offer a recommendation for a slight modification. We have in a sense a cap on what the IRC should be, in the form of the Basel II internal ratings-based (IRB) capital charge—the capital that would be required were the assets in question to sit in the banking book. In the limit where our trading positions are highly illiquid, these positions should be treated exactly as banking book positions.

We should establish a limit on the other end of the spectrum: what the IRC should be for very liquid positions. Basel has established that they view three months as a minimal liquidity horizon, and so we should define this liquid-IRC based on a three-month holding period. While it is appealing to establish a risk measure (VaR) and confidence level (99.9%) to be consistent with the IRB charge, we are faced with the shortcomings of the VaR measure discussed previously. It would be sensible, then, to base the definition on an expected shortfall, with confidence level set to match the prudential standard of the IRB charge, at least in some special cases. The Committee should reserve the right to apply a capital multiplier in this case, in order to raise overall capital levels without compromising the desirable risk sensitivity of the measure.

With our two limits established, it remains to specify how capital should be computed for portfolios of an arbitrary liquidity horizon. Here, we must admit that the constant level of risk approach, while appealing in spirit, is unattractive in practice, and will in all likelihood lead to obfuscation without any real modeling advances. The liquidity horizon, on the other hand, is a worthwhile endeavor on its own. We propose, then, that banks establish and defend liquidity horizons for all positions, subject to a prudential regulatory floor, and calculate the average liquidity horizon across the portfolio. Moreover, the liquidity horizon definition should play a role in the needed definition of "illiquid positions" within the specific IRC rules.

The final IRC charge should then be obtained by a simple interpolation rule (not necessarily linear, but still transparent) between the two limiting cases, based on the portfolio average liquidity horizon. Granted this proposal is itself but a recipe, and not a coherent statistical definition of solvency. But in the spirit of the old MRA compromise, it is a recipe that makes fully transparent the dependence of capital on the liquidity horizon, while at the same time preserving an appropriate level of capital for the most and least liquid portfolios. Most importantly, it is a recipe that encourages the industry to focus its research efforts on items that are valuable in their own right—short-term default probabilities, liquidity horizons, basis risks—and limits the most onerous operational requirements and least valuable modeling discussions at the same time.

Further reading

- Basel Committee on Banking Supervision (2008). Principles for sound liquidity risk management and supervision. September.
- Basel Committee on Banking Supervision (2009). Guidelines for computing capital for incremental risk in the trading book. Consultative document. January.
- Breuer, T. et al (2007). Regulatory capital for market and credit risk interaction: Is current regulation always conservative? Conference on the Interaction of Market and Credit Risk, Berlin, December.
- Finger, C. (2008). The once holy grail, RiskMetrics Research Monthly, January.

Research Monthly

VaR Is From Mars, Capital Is From Venus

April 2009

Christopher C. Finger
chris.finger@riskmetrics.com

In February, we commented on a Basel Committee proposal to implement a capital charge (the Incremental Risk Charge, or IRC) to cover default and migration losses in the trading book. Last month, the Financial Services Authority (FSA) of the UK released the Turner Review, an analysis of the origins of our financial crisis and recommendations for the future of financial services regulation, along with a discussion paper stating a more official FSA policy stance. Unlike the detailed proposal for the IRC, the FSA documents represent a broader review of regulation and capital generally.

Neither FSA document is guarded in its assessment of financial supervisors in the runup to the crisis. The discussion paper summarizes:

> ...prudential regulation was too weak at a micro-prudential level, and almost completely lacking at a macro-prudential level. (Paragraph 4.2)

Central to the calls for macro-prudential regulation is the reaffirmation of the supervisors' role in ensuring the stability of the financial system. The papers propose that regulators take an active role in identifying systemic risks—easing of credit standards, rising systemic leverage, falling risk premia—through both macroeconomic analysis and knowledge of the actions of specific institutions. This is an ambitious step, broadening the mandate for regulation generally, but also requiring cooperation between the different agencies that today perform macroeconomic and institution specific analyses.

For the micro-prudential level—the supervision of individual institutions—the strongest call is for a "fundamental review" of the existing minimum capital rules. The tone of the papers make clear that the FSA holds strong views on where this review should take us. As for Value-at-Risk (VaR) models, the FSA states that the burden of proof lies with the industry to show that VaR is appropriate for capital rules.

But such a challenge sets up the review as a (potentially one-sided) discussion focused on the merits (or demerits) of VaR. This is far less productive than starting with a review of what trading book capital should be, with the notion that if a short-term measure of risk is appropriate, then VaR should prove its place. Our aim in this note, then, is to address some of the wrong reasons the FSA has put forth to cast VaR aside, offer some good reasons of our own, and comment on where short-term risk measures could contribute in a future trading book capital framework.

What went wrong?

Both FSA documents begin with the same chapter entitled "What went wrong?" The chapter reviews the macroeconomic environment and the growth of the securitized credit model, and then moves to the now obligatory section on "Misplaced reliance on sophisticated maths". Central to this section is an exhibit (Box 1A in the Turner Review) summarizing in three main points the FSA views on the deficiencies of VaR-based estimates of risk.

One of the three VaR deficiencies is the "failure to capture fat-tail risks", which is further elaborated:

> Short-term observation periods plus assumption of normal distribution can lead to large underestimation of probability of extreme loss events.

While the symptom here (extreme loss events were underestimated) is uncontroversial, both aspects of the diagnosis are misguided.

To be clear, VaR models are intended to forecast (in a statistical sense) the possible loss on today's portfolio, based on today's market, over a short (one day to one month, for instance) risk horizon. Almost always, these models rely on historical price moves and some statistical inference to perform forecasting. In regulatory language, VaR by definition is a point-in-time measure.

One implication is that VaR models are verifiable: we can track both our forecasts of possible loss and the actual loss experience over time, and validate whether these two have the statistical relationship that they should. Banks perform this backtesting as part of their regulatory and disclosure requirements. Among the standard metrics is the number of VaR exceptions, that is, the number of days on which actual trading losses exceeded the VaR forecast.

Campbell (2009) recently surveyed bank VaR disclosure for 2008. Even in such an eventful year, there was a wide range of model performance. One of the better performers, Bank of America, reported two VaR exceptions at 99% confidence, almost precisely what one would expect. This was an improvement on the bank model's performance in 2007, which the bank attributed to their move to more frequent data updates in an effort to react more rapidly to higher volatility. In fact, this move to more reactive volatility measures was one of the best practices cited by the Senior Supervisors Group (SSG) in March 2008. This example also shows that good backtesting results do not guarantee that a bank had a peaceful year.

At the other end of the spectrum was UBS, which recorded 50 exceptions at 99% confidence. In other words, UBS observed what they thought was a one-in-a-hundred event on average once per week, a spectacular failure in risk forecasting. One of the bank's comments on this poor performance was to assert that their results "highlight the limitations of VaR". Such a broad dismissal of risk forecasting based is especially feeble in light of other banks' success.

A closer look at the UBS disclosure is illuminating. In its annual report, the bank discloses that it utilizes five years of historical returns, with equal weighting, to produce its VaR forecasts. It warns that this method "does not respond quickly to periods of heightened volatility". Indeed.

Thus, consistent with the SSG's recommendations, but at odds with the FSA evaluation, it was *long* observation periods that led to underestimation of risks. In the short term, risk changes, and risk models must react. The use of short[1] observation periods for VaR

[1] Or effectively short, in the case that past data is weighted unequally

forecasting is necessary to make good forecasts, and not just a convenient choice that lets us save money on data storage.

False relics

This brings us to the second part of the diagnosis—the use of the infamous normal distribution—which has been fodder for a thousand popular press vilifications of risk models. While it is true that the normal distribution is an oversimplification of empirical loss experience, the continued burning in effigy of Karl Friedrich Gauss has distracted the dialogue from numerous other points. And while we can ignore the counterproductive dialogue in the tabloid press or chat forums, the regulatory dialogue is more important. Unfortunately, to be heard above the din, a bit of yelling is in order.

First, any discussion of probability distribution (the description of our ignorance) must come only after using the available information to forecast what we can, in this case volatility. If asked to propose a distribution of the heights of a group of school children, it would be absurd to start deriving tail exponents before asking how old the children were.

Second, criticisms of the normal distribution are not new, and there exist numerous VaR model implementations that use alternatives. Not all of these are improvements, however. Those that use non-normal distributions but do not forecast changes in volatility perform categorically worse than those that stick with the beleaguered normal distribution but react appropriately to changing market conditions.

Third, the search for the right fat-tailed distribution at best gives us a more accurate view of the static part of our risks. As such, it is in some ways equivalent to shrugging our shoulders and saying "Shit happens". Maybe we recognize that market moves of ten times our volatility forecast are significantly more likely than we thought, not one-in-a-billion events but one-in-a-thousand events. But this teaches us nothing about the dynamics of the market we are trading, and gives us no warning signals of when those events are becoming even more plausible. The plea for the right fat-tailed distribution is the medieval response to the Plague, not to understand hygiene, germs or contagion, but to blame misfortune on the unknowable and immeasurable. This reaction is dangerous, as it presupposes not only that we don't know, but that we can't know. Our only hope is to trust in someone endowed with divine knowledge of the unknowably improbable.

Bigger distractions

The most dangerous distraction then is from an Enlightenment response to the Plague—a real analysis of what could have made underestimations of risk better, not just by making them larger, but by making them more timely. There are two crucial areas which have been all too often ignored. To their credit, the FSA does focus on one of these.

The second of the VaR deficiencies is "Failure to capture systemic risk", in which the FSA cites the assumption that "each institution is an individual agent whose actions do not themselves affect the market", and asserts that "interconnected market events can produce self-reinforcing cycles which models do not capture". This is potentially a much more productive discussion than that of fat-tailed distributions, as it leads us away from a view purely based on historical returns data, and holds the promise of more than just larger risk estimates overall, but more timely signals of when risk has increased.

In fact, the language here lets us resolve one of the damaging rhetorical corners that the risk community has painted itself into. It is common to state that VaR models work in "normal markets", but without ever defining what "normal markets" are. The lack of a good definition turns what could be a useful guidepost into a useless circular statement: models work in normal markets, and normal markets are defined as those within which models work.

A better definition of a normal market is one in which the assumption mentioned above holds: institutions' actions do not themselves affect the market, and all market participants are purely victims of a set of exogenous price processes. In this regime, it is reasonable to expect that the historical data is sufficient as a basis for forecasting, and that VaR models will provide timely and accurate indicators of risk.[2]

To illustrate what we would call a non-normal market, consider the dislocation in tranched credit derivatives in 2005. The first two tranches of the North American credit derivatives index (CDX) typically exhibit a very tight relationship, with correlations in the 80-90% range. There is a fundamental reason for this, as the two tranches represent protection on the same underlying portfolio, albeit against different levels of loss. In the first two weeks of May 2005, however, the tranches moved significantly against each other, without the price of the underlying index moving much, causing heavy losses for investors with (seemingly) hedged positions across the two tranches.

This was a case where no model based on historical data would have foreseen the losses to come. Though a savvy risk manager would have known that the relationship between the two tranches was not perfect, and that such a dislocation was possible in theory, no historical precedent existed for the magnitude of the losses. This is not, however, a case where we shrug our shoulders and mumble about fat tails.

In the aftermath, it became clear that there had been a large buildup in positions that were short protection on the first tranche and long protection on the second. In such a position, a trader would reap a net quarterly premium and be hedged against moves in the underlying portfolio, or so it seemed. But with many investors in the same trade, the inevitable happened. Sparked by an event external to the tranche market (likely the earlier downgrades of Ford and GM), an initial set of investors closed their positions, pushing the prices of the two tranches apart and causing mark-to-market losses for those still holding the position; this sparked more position closing, which led to greater losses, and so on. In the end, this was a classic example of a crowded trade.[3]

But with no historical precedent, was there anything risk managers could do? Possibly. Market makers in these derivatives, themselves advocates for the "hedged" trade, could see the market flows leading to the crowded trade, prompting perhaps an investi-

[2]To be clear, we use the word normal here in its generic sense, meaning typical or regular. Market returns could well be non-normally distributed; all we are claiming is that useful statistical inference from historical data is possible.

[3]See Finger (2005) for more detailed analysis.

gation of their own positioning and susceptibility to a rush-to-the-exits scenario. Outside of flow desks, in 2005, there may have been nothing but market rumors to provide such insight. Today, however, the situation has improved.

The Depository Trust Clearing Corporation (DTCC) began in the fall of 2008 to provide weekly snapshots of the market exposures in credit default swaps, credit indices and tranches. For some of these products, the reports are granular enough to note changes in exposure on individual contracts. For tranches, this is not yet the case, but it is not far fetched to believe the members of the DTCC will agree to make this level of detail available in the future. Notably, this is true for an over-the-counter market which does not (yet) operate with a central counterparty.

In short, the FSA is correct to criticize the current generation of VaR models for their inability to uncover systemic risk or contagion effects. The data and tools to uncover such effects are closer than we might think, and should be the subject of significant attention in any regulatory review.

A new backtest

One year ago, we wrote in praise of the SSG for recognizing the description of positions in a risk model as just as crucial as the volatility model or distributional assumption. Neither FSA document makes enough mention of this point. This is the second casualty of the fat-tailed distraction.

The most overlooked source of bad VaR forecasts is the failure to adequately describe trading positions. This can take the form of a missing risk factor—assuming the basis between two similar instruments or the spread on a risky bond is constant—or a poor proxy choice—utilizing corporate bond yields to describe the risk of a securitization with comparable rating. As the SSG pointed out in early 2008, and as continued to be the case, it was modeling the wrong (or no) risk factors that was the root of the worst understatements of risk, not the choice of the wrong statistical distribution. The lack of coverage of credit risk in the trading book—what the IRC proposes to address—is a version of this same problem.

There are statistical arguments to tell us how many exceptions we should see in theory, assuming VaR models work as advertised. And there is a history of disclosure on simple, standard backtesting measures. We have a sense for how many exceptions the industry experiences, and know for instance that at 99% confidence, two VaR exceptions in a year is expected, eight is fair but slightly concerning and fifty is outrageous.

We propose that the description of instruments for risk purposes be backtested in their own right, independently of the statistical models used to forecast changes, and that the risk community establish a set of simple benchmarks for these tests. One candidate for such a benchmark is to compare over time the actual market price changes on an instrument to the price changes that would appear in the risk model, with whatever assumptions (proxy factors, constant spreads, linear price relationships) that might be entailed. Simple correlations of these changes, averaged across asset classes, would provide a first indicator of the quality of instrument representation.

Of course, this sort of backtesting is only possible if we have actual market prices to compare. The absence of such prices, and the inability to perform the proposed backtesting, could be used as criteria to exclude instruments from a model-based capital framework, or from regulatory trading book consideration.

This is essentially the argument used (lack of prices, unclear pricing models and risk factors) for excluding securitizations from trading book consideration, both in the FSA's proposed capital review and the Basel Committee's IRC proposal. We are in agreement with this specific decision at the present time, but recommend that regulators and industry formalize what is expected for securitizations, or any instrument, to earn trading book treatment in the future.

Moving to procylicality

The last of the VaR deficiencies is that the procyclicality of VaR contributed to excessive risk taking in the period prior to the crisis. A procyclical capital framework is one that reinforces business cycles, requiring less capital when times appear good—encouraging greater risk taking—and more capital as the economy contracts—constraining banks' ability to lend and working against economic growth. Alongside this point, there is a demonstration that VaR based on short histories will produce such procyclical capital requirements. The solution is to use longer historical periods, exactly as UBS did. Once again, the symptom is correct, but not the diagnosis.

The conflict between the needs of capital and the output of VaR has existed since VaR first became part of the capital regime. The regulators have responded to this by placing restrictions on VaR models, in an effort to embed into VaR the properties that are desirable of regulatory capital. The call for longer observation periods in order to eliminate procyclicality is a continuation of this mindset. So is the recent proposal of the Basel Committee to apply risk forecasts from a turbulent period to the positions of today, in order to calculate a so-called stressed VaR.

The restrictions on VaR have not worked in the past—trading book capital under the current regime is still flawed—and further restrictions are unlikely to make it work in the future. At the same time, the UBS example demonstrates that VaR models that hew closely to regulatory desires perform poorly as risk forecasters. The effort to make a desirable capital rule out of a good risk forecasting model has resulted in something that is neither.

Looking to the horizon

The bottom line is that short-horizon risk forecasts should be procyclical, and efforts to dampen this produce worse forecasts. But two weeks is too short a horizon over which to set prudential capital. The horizons over which procyclicality matters are measured in quarters or years, not days or weeks. For an institution or a system to build up risks over this type of horizon, it is not enough for a portfolio at a specific point in time to go bad (something VaR might warn against); rather, the systemic risks are a result of institutions' reactions to an evolving market.

So the fundamental question for the fundamental review is how to define minimum capital for trading books such that the regime is risk sensitive, is countercyclical (or at least, not procyclical) and protects not just a fixed portfolio, but rather an institution as a going concern. While VaR on its own satisfies the first of these requirements, it is incompatible with the second, and by construction inappropriate for the third. There is a strong argument, then, for moving away from a capital requirement defined as a multiple of VaR, not because VaR fails to perform as advertised, but because it succeeds.

If we accept VaR for what it is, there may be a role for the measure in a new capital regime, even if not the central role it plays today. And if not, the regulators should still emphasize that institutions should measure, manage and disclose the short-term risks in their trading portfolios. In either case, regulators should allow banks the freedom to build models that best forecast short-term risk. In return, banks should not only demonstrate adequate statistical performance of their risk forecasts, but also an acceptable link between any position's representation in the risk model and its actual market prices.

Finally, some short-term events can in fact produce systemic risks: crowded trades, contagion, liquidity shocks, the transition to non-normal markets. Signals for these should become part of the supervisory arsenal. But far from giving up and bucketing all of these as unknowable fat tails, we should be seeking indicators of when market imbalances have made these dynamics more likely. Regulators, banks and the rest of the financial community should share in this task.

Further reading

- Campbell (2009). The Risk of Value-at-Risk. *Risk*, **22**(4): 42–46.
- Financial Services Authority (2009). A Regulatory Response to the Global Banking Crisis. Discussion Paper 09/2. March.
- Financial Services Authority (2009). The Turner Review: A Regulatory Response to the Global Banking Crisis. March.
- Finger (2005). Eating Our Own Cooking, RiskMetrics Research Monthly, June.
- Finger (2008). The SSG and My Two Brains, RiskMetrics Research Monthly, March.

BIOGRAPHY FOR GREGG E. BERMAN

Gregg E. Berman, 43, is currently head of RiskMetrics Risk Business covering institutional and wealth management offerings that serve Hedge Funds, Asset Managers, Prime Brokers, Banks, Financial Advisors, Insurance Companies, and Corporates. Mr. Berman joined RiskMetrics as a founding member during the time of its spin-off from J.P. Morgan in 1998 and has held a number of roles from research to head of product management, market risk, and of business management.

Prior to joining RiskMetrics Group, Mr. Berman co-managed a number of multi-asset Hedge Funds within New York-based ED&F Man. His start in the Hedge Fund space began in 1993, researching and developing multi-asset trading strategies as part of Mint Investment Management Corporation, a $1bn CTA based in New Jersey.

Mr. Berman is a physicist by training and holds degrees from Princeton University (Ph.D. 1994, M.S. 1989), and the Massachusetts Institute of Technology (B.S. 1987).

Chairman MILLER. Thank you, Dr. Berman.

Mr. Rickards for five minutes.

STATEMENT OF MR. JAMES G. RICKARDS, SENIOR MANAGING DIRECTOR FOR MARKET INTELLIGENCE, OMNIS, INC., MCLEAN, VA

Mr. RICKARDS. Mr. Chairman, my name is James Rickards and I appreciate the opportunity to speak to you on a subject of the utmost importance to global capital markets.

The world is two years into the worst financial crisis since the Great Depression. The list of culprits is long, including mortgage brokers, investment bankers and rating agencies. The story sadly is, by now, well known. What is less well known is that behind these actors were quantitative risk models which said that all was well even as the bus was driving over a cliff.

Unfortunately, we have been here before. In 1998, capital markets came to the brink of collapse due to the failure of a hedge fund, Long-Term Capital Management. The amounts involved seem small compared to today's catastrophe. However, it did not seem that way at the time. I know, I was general counsel of LTCM. What is most striking to me now as I look back is how nothing has changed and how no lessons were learned. The lessons should have been obvious. LTCM used fatally flawed VaR models, too much leverage, and the solutions were clear. Risk models needed to be changed or abandoned, leverage needed to be reduced, and regulatory oversight needed to be increased.

Amazingly, the United States Government did the opposite. They repealed Glass-Steagall in 1999 and allowed banks to act like hedge funds. The *Commodity Futures Modernization Act of 2000* allowed more unregulated derivatives. SEC regulations in 2004 allowed increased leverage. It was as if the United States had looked at the catastrophe of LTCM and decided to double down. None of this would have happened without the assurance and comfort provided to regulators and Wall Street by VaR models. But all models are based on assumptions. If the assumptions are flawed, no amount of mathematics will compensate. Therefore, the root of our inquiry into VaR should be an examination of the assumptions behind the models.

The key assumptions are the following: one, the efficient market hypothesis, which assumes that investors behave rationally; two, the random walk, which assumes that no investor can beat the market consistently, because future prices are independent of the past; three, normally distributed risk. This says that since future price movements are random, the relationship of the frequency and the severity of the events will also be random, like a coin toss or roll of the dice. The random distribution is represented as a bell curve. Value-at-Risk would be a fine methodology but for the fact that all three of these assumptions are wrong. Markets are not efficient, future prices are not independent of the past, risk is not normally distributed. As the saying goes, “Besides that, Mrs. Lincoln, how was the play?”

Behavioral economics has done a masterful job of showing that investors do not behave rationally and are guided by emotion. Similarly, prices do not move randomly but are dependent on past prices. In effect, news may be ignored for sustained periods of time until a kind of tipping point is achieved, at which point investors will react en masse. The normal distribution of risk has been known to be false since the early 1960s, when studies showed price distributions to be shaped in what is known as a power curve. A power curve has fewer low-impact events than the bell curve but has far more high-impact events. In short, a power curve corresponds to market reality while a bell curve does not.

Power curves have low predictability but can offer other valuable insights. One lesson is that as you increase the scale of the system, the size of the largest possible catastrophe grows exponentially. An example will illustrate the relationship between the scale of the system and the greatest catastrophe possible. Imagine a vessel with a large hold divided into three sections, separated by watertight bulkheads. If a hole is punched in one section and that section fills with water, the vessel will still float. Now imagine the bulkheads are removed and the same hole is punched into the vessel. The entire hold will fill with water and the vessel will sink. In this example, the hold can be thought of as the system. The sinking of the vessel represents the catastrophic failure of the system. When the bulkheads are in place, we have three small systems. When the bulkheads are removed, we have one large system. By removing the bulkheads, we increase the scale of the system by a factor of three, but the likelihood of failure did not increase by a factor of three. It went from practically zero to practically 100 percent. The system size tripled, but the risk of sinking went up exponentially.

If scale is the primary determinant of risk in complex systems, it follows that descaling is the most effective way to manage risk. This does not mean that the totality of the system needs to shrink—merely that it be divided into subcomponents with limited interaction. This has the same effect as installing the watertight bulkheads referred to above. In this manner, severe financial distress in one sector does not result in contagion among all sectors.

This descaling can be accomplished with three reforms: number one, the enactment of a modernized version of Glass-Steagall with a separation between bank deposit taking on the one hand, and market risk on the other; two, strict requirements for all derivative products to be traded on exchanges subject to margin position limits, price transparency and netting; three, higher regulatory capital requirements and reduced leverage for banks and brokers. Traditional ratios of eight to one for banks and 15 to one for brokers seem adequate, provided off-balance sheet positions are included.

Let us abandon VaR and the bell curve once and for all and accelerate empirical research into the actual metrics of event distributions. Even if predictive value is low, there is value in knowing the limits of our knowledge. Understanding the way risk metastasizes with scale might be lesson enough. It would offer a proper dose of humility to those trying to supersize banks and regulators.

Thank you for this opportunity to testify.

[The prepared statement of Mr. Rickards follows:]

PREPARED STATEMENT OF JAMES G. RICKARDS

The Risks of Financial Modeling: VaR and the Economic Meltdown

Introduction

Mr. Chairman, Mr. Ranking Member and Members of this subcommittee, my name is James Rickards, and I want to extend my deep appreciation for the opportunity and the high honor to speak to you today on a subject of the utmost importance in the management of global capital markets and the global banking system. The Subcommittee on Investigations and Oversight has a long and distinguished history of examining technology and environmental matters which affect the health and well-being of Americans. Today our financial health is in jeopardy and I sincerely applaud your efforts to examine the flaws and misuse in financial modeling which have contributed to the impairment of the financial health of our citizens and the country as a whole.

As a brief biographical note, I am an economist, lawyer and author and currently work at Omnis, Inc. in McLean, VA where I specialize in the field of threat finance and market intelligence. My colleagues and I provide expert analysis of global capital markets to members of the national security community including military, intelligence and diplomatic directorates. My writings and research have appeared in numerous journals and I am an Op-Ed contributor to the *Washington Post* and *New York Times* and a frequent commentator on CNBC, CNN, Fox and Bloomberg. I was formerly General Counsel of Long-Term Capital Management, the hedge fund at the center of the 1998 financial crisis, where I was principal negotiator of the Wall Street rescue plan sponsored by the Federal Reserve Bank of New York.

Summary: The Problem with VaR

The world is now two years into the worst financial crisis since the Great Depression. The IMF has estimated that the total lost wealth in this crisis so far exceeds $60 Trillion dollars, more than the cost of all of the wars of the 20th century combined. The list of causes and culprits is long including mortgage brokers making loans borrowers could not afford, investment bankers selling securities while anticipating their default, rating agencies granting triple-A ratings to bonds which soon suffered catastrophic losses, managers and traders focused on short-term profits and bonuses at the expense of their institutions, regulators acting complacently in the face of growing leverage and imprudence and consumers spending and borrowing at non-sustainable rates based on a housing bubble which was certain to burst at some point. This story, sadly, is by now well known.

What is less well-known is that behind all of these phenomena were quantitative risk management models which told interested parties that all was well even as the bus was driving over a cliff. Mortgage brokers could not have made unscrupulous loans unless Wall Street was willing to buy them. Wall Street would not have bought the loans unless they could package them into securities which their risk models told them had a low risk of loss. Investors would not have bought the securities unless they had triple-A ratings. The rating agencies would not have given those ratings unless their models told them the securities were almost certain to perform as expected. Transaction volumes would not have reached the levels they did without leverage in financial institutions. Regulators would not have approved that leverage unless they had confidence in the risk models being used by the regulated entities. In short, the entire financial edifice, from borrower to broker to banker to investor to rating agency to regulator, was supported by a belief in the power and accuracy of quantitative financial risk models. Therefore an investigation into the origins, accuracy and performance of those models is not ancillary to the financial crisis; it is not a footnote; it is the heart of the matter. Nothing is more important to our understanding of this crisis and nothing is more important to the task of avoiding a recurrence of the crisis we are still living through.

Unfortunately, we have been here before. In 1998, western capital markets came to the brink of collapse, owing to the failure of a hedge fund, Long-Term Capital Management, and a trillion dollar web of counter-party risk with all of the major banks and brokers at that time. Then Fed Chairman Alan Greenspan and Treasury Secretary Robert Rubin called it the worst financial crisis in over 50 years. The amounts involved and the duration of the crisis both seem small compared to today's catastrophe, however, it did not seem that way at the time. Capital markets really did teeter on the brink of collapse; I know, I was there. As General Counsel of Long-Term Capital Management, I negotiated the bail out which averted an even greater disaster at that time. What is most striking to me now as I look back is how nothing changed and how no lessons were applied.

The lessons were obvious at the time. LTCM had used fatally flawed VaR risk models. LTCM had used too much leverage. LTCM had transacted in unregulated over-the-counter derivatives instead of exchange traded derivatives. The solutions were obvious. Risk models needed to be changed or abandoned. Leverage needed to be reduced. Derivatives needed to be moved to exchanges and clearinghouses. Regulatory oversight needed to be increased.

Amazingly the United States Government did the opposite. The repeal of Glass-Steagall in 1999 allowed banks to act like hedge funds. The *Commodities Futures Modernization Act of 2000* allowed more unregulated derivatives. The Basle II accords and SEC regulations in 2004 allowed increased leverage. It was as if the United States had looked at the near catastrophe of LTCM and decided to double-down.

What reason can we offer to explain this all-in approach to financial risk? Certainly the power of Wall Street lobbyists and special interests cannot be discounted. Alan Greenspan played a large role through his belief that markets could self-regulate through the intermediation of bank credit. In fairness, he was not alone in this belief. But none of this could have prevailed in the aftermath of the 1998 collapse without the assurance and comfort provided by quantitative risk models. These models, especially Value-at-Risk, cast a hypnotic spell, as science often does, and assured bankers, investors and regulators that all was well even as the ashes of LTCM were still burning.

What are these models? What is the attraction that allows so much faith to be placed in them? And what are the flaws which lead to financial collapse time and time again?

The term "Value-at-Risk" or VaR is used in two senses. One meaning refers to the assumptions, models and equations which constitute the risk management systems most widely used in large financial institutions today. The other meaning refers to the output of those systems, as in, "our VaR today is $200 million" which refers to the maximum amount the institution is expected to lose in a single day within some range of probability or certainty usually expressed at the 99 percent level. For purposes of this testimony, we will focus on VaR in the first sense. If the models are well founded then the output should be of some value. If not, then the output will be unreliable. Therefore the proper focus of our inquiry should be on the soundness of the models themselves.

Furthermore, any risk management system is only as good as the assumptions behind it. It seems fair to conclude that based on a certain set of assumptions, the quantitative analysts and computer developers are able within reason to express those assumptions in equations and to program the equations as computer code. In other words, if the assumptions are correct then it follows that the model development and the output should be reasonably correct and useful as well. Conversely, if the assumptions are flawed then no amount of mathematical equation writing and computer development will compensate for this deficiency and the output will always be misleading or worse. Therefore, the root of our inquiry into models should be an examination of the assumptions behind the models.

In broad terms, the key assumptions are the following:

The Efficient Market Hypothesis (EMH): This assumes that investors and market participants behave rationally from the perspective of wealth maximization and will respond in a rational manner to a variety of inputs including price signals and news. It also assumes that markets efficiently price in all inputs in real time and that prices move continuously and smoothly from one level to another based on these new inputs.

The Random Walk: This is a corollary to EMH and assumes that since markets efficiently price in all information, no investor can beat the market consistently because any information which an investor might rely on to make an investment decision is already reflected in the current market price. This means than future market prices are independent of past market prices and will be based solely on future events that are essentially unknowable and therefore random.

Normally Distributed Risk: This is also a corollary to EMH and says that since future price movements are random, their degree distribution (i.e., relationship of frequency to severity of events) will also be random like a coin toss or roll of the dice. This random or normal degree distribution is also referred to as Gaussian and is most frequently represented as a bell curve in which the large majority of outcomes are bunched in a region of low severity with progressively fewer outcomes shown in the high severity region. Because the curve tails off steeply, highly extreme events are so rare as to be almost impossible.

Value-at-Risk would be a fine methodology but for the fact that all three of these assumptions are wrong. Markets are *not* efficient. Future prices are *not* independent of the past. Risk is *not* normally distributed. As the saying goes, "Besides that, Mrs. Lincoln, how was the play?" Let's take these points separately.

Behavioral economics has done a masterful job of showing experimentally and empirically that investors do not behave rationally and that markets are not rational but are prone to severe shocks or mood swings. Examples are numerous but some of the best known are risk aversion (i.e., investors put more weight on avoiding risk than seeking gains), herd mentality (i.e., investors buy stocks when others are buying and sell when others are selling leading to persistent losses) and various seasonal effects. Prices do not smoothly and continuously move from one price level to the next but have a tendency to gap up or down in violent thrusts depriving investors of the chance to get out before large losses are incurred.

Similarly, prices to not move randomly but are highly dependent on past price movements. In effect, relevant news will be discounted or ignored for sustained periods of time until a kind of tipping point is achieved at which point investors will react *en masse* to what is mostly old news mainly because other investors are doing likewise. This is why markets exhibit periods of low and high volatility in succession, why markets tend to overshoot in response to fundamental news and why investors can profit consistently by momentum trading which exploits an understanding of these dynamics.

Finally, the normal distribution of risk has been known to be false at least since the early 1960's when published studies of time series of prices showed price distributions to be shaped in what is known as a power curve rather than a bell curve. This has been borne out by many studies since. A power curve has fewer low impact events than the bell curve but has far more high impact events. This corresponds exactly to the actual market behavior we have seen including frequent extreme events such as the stock market crash of 1987, the Russian-LTCM collapse of 1998, the dot corn bubble collapse of 2000 and the housing collapse of 2007. Statistically these events should happen once every 1,000 years or so in a bell curve distribution but are expected with much greater frequency in a power curve distribution. In short, a power curve corresponds to market reality while a bell curve does not.

How is it possible that our entire financial system has come to the point that it is risk managed by a completely incorrect system?

The Nobelist, Daniel Kahneman, tells the story of a Swiss Army patrol lost in the Alps in a blizzard for days. Finally the patrol stumbles into camp, frostbitten but still alive. The Commander asks how they survived and the patrol leader replies, "We had a map." The Commander looks at the map and says, "This is a map of the Pyrenees; you were in the Alps." "Yes," comes the reply; "but we had a map." The point is that sometimes bad guidance is better than no guidance; it gives you confidence and an ability to function even though your system is flawed.

So it is with risk management on Wall Street. The current system, based on the idea that risk is distributed in the shape of a bell curve, is flawed and practitioners know it. Practitioners treat extreme events as outliers and develop mathematical fixes. They call extreme events fat tails and model them separately from the rest of the bell curve. They use stress tests to gauge the impact of extreme events. The problem is they never abandon the bell curve. They are like medieval astronomers who believe the sun revolves around the earth and are furiously tweaking their geocentric math in the face of contrary evidence. They will never get this right; they need their Copernicus.

But the right map exists. It's called a power curve. It says that events of any size can happen and extreme events happen more frequently than the bell curve predicts. There is no need to treat fat tails as a special case; they occur naturally on power curves. And power curves are well understood by scientists because they apply to extreme events in many natural and man-made systems from power outages to earthquakes.

Power curve analysis is not new. The economist, Vilfredo Pareto, observed in 1906 that wealth distributions in every society conform to a power curve; in effect, there is one Bill Gates for every 100 million average Americans. Benoit Mandelbrot pioneered empirical analysis in the 1960's that showed market prices move in power curve patterns.

So why have we gone down the wrong path of random walks and normal distributions for the past 50 years? The history of science is filled with false paradigms that gained followers to the detriment of better science. People really did believe the sun revolved around the earth for 2,000 years and mathematicians had the equations to prove it. The sociologist, Robert K. Merton, called this the Matthew Effect from a New Testament verse that says, "For to those who have, more will be given . . ." The idea is that once an intellectual concept attracts a critical mass of supporters it becomes entrenched while other concepts are crowded out of the marketplace of ideas.

Another reason is that practitioners of bell curve science became infatuated with the elegance of their mathematical solutions. The Black-Scholes options formula is based on bell curve type price movements. The derivatives market is based on variations of Black-Scholes. Wall Street has decided that the wrong map is better than no map at all—as long as the math is neat.

Why haven't scientists done more work in applying power curves to capital markets? Some excellent research has been done. But one answer is that power curves have low predictive value. Researchers approach this field to gain an edge in trading and once the edge fails to materialize they move on. But the Richter Scale, a classic power curve, also has low predictive value. That does not make earthquake science worthless. We know that 8.0 earthquakes are possible and we build cities accordingly even if we cannot know *when* the big one will strike.

We can use power curve analysis to make our financial system more robust even if we cannot predict financial earthquakes. One lesson of power curves is that as you increase the scale of the system, the risk of a mega-earthquake goes up exponentially. If you increase the value of derivatives by a factor of 10, you may be increasing risk by a factor of 10,000 without even knowing it. This is not something that Wall Street or Washington currently comprehend.

Let's abandon the bell curve once and for all and accelerate empirical research into the proper risk metrics of event distributions. Even if predictive value is low, there is value in knowing the limits of our knowledge. Understanding the way risk metastasizes with scale might be lesson enough. It would offer a proper dose of humility to those trying to supersize banks and regulators.

Detailed Analysis—History of VaR Failures

The empirical failures of the Efficient Market Hypothesis and VaR are well known. Consider the October 19, 1987 stock market crash in which the market fell 22.6 percent in one day; the December 1994 Tequila Crisis in which the Mexican Peso fell 85 percent in one week; the September 1998 Russian-LTCM crisis in which capital markets almost ceased to function; the March 2000 dot corn collapse during which the NASDAQ fell 80 percent over 30 months, and the 9–11 attacks in which the NYSE first closed and then fell 14.3 percent in the week following its reopening. Of course, to this list of extreme events must now be added the financial crisis that began in July 2007. Events of this extreme magnitude should, according to VaR, either not happen at all because diversification will cause certain risks to cancel out and because rational buyers will seek bargains once valuations deviate beyond a certain magnitude, or happen perhaps once every 1,000 years (because standard deviations of this degree lie extremely close to the x-axis on the bell curve which corresponds to a value close to zero on the y-axis, i.e., an extremely low frequency event). The fact that all of these extreme events took place in just over 20 years is completely at odds with the predictions of VaR in a normally distributed paradigm.

Practitioners treated these observations not as fatal flaws in VaR but rather as anomalies to be explained away within the framework of the paradigm. Thus was born the "fat tail" which is applied as an embellishment on the bell curve such that after approaching the x-axis (i.e., the extreme low frequency region), the curve flattens to intersect data points representing a cluster of highly extreme but not so highly rare events. No explanation is given for what causes such events; it is simply a matter of fitting the curve to the data (or ignoring the data) and moving on without disturbing the paradigm. This process of pinning a fat tail on the bell curve reached its apotheosis in the invention of generalized auto-regressive conditional heteroskedasicity or GARCH and its ilk, which are analytical techniques for modeling the section of the degree distribution curve containing the extreme events as a separate case and feeding the results of this modeling into a modified version of the curve. A better approach would have been to ask the question: if a normal distribution has a fat tail, is it really a normal distribution?

A Gaussian distribution is not the only possible degree distribution. One of the most common distributions in nature, which accurately describes many phenomena, is the power curve which shows that the severity of an event is inversely proportional to its frequency with the proportionality expressed as an exponent. When graphed on a double logarithmic scale, the power law describing financial markets risk is a straight line sloping downward from left to right; the negative exponent is the slope of the line.

This difference is not merely academic. Gaussian and power curve distributions describe two *entirely different phenomena.* Power curves accurately describe a class of phenomena known as nonlinear dynamical systems which exhibit scale invariance, i.e., patterns are repeated at all scales.

The field of nonlinear dynamical systems was enriched in the 1990s by the concept of self-organized criticality. The idea is that actions propagate throughout systems in a *critical* chain reaction. In the critical state, the probability that an action will propagate is roughly balanced by the probability that the original action will dissipate. In the subcritical state, the probability of extensive effects from the initial action is low. In the super-critical state, a single minor action can lead to a catastrophic collapse. Such states have long been observed in physical systems, e.g., nuclear chain reactions in uranium piles, where a small amount of uranium is relatively harmless (*subcritical*) and larger amounts can either be carefully controlled to produce desired energy (*critical*), or can be shaped to produce atomic explosions (*supercritical*).

The theory of financial markets existing in a critical state cannot be tested in a laboratory or particle accelerator in the same fashion as theories of atomic physics. Instead, the conclusion that financial markets are a nonlinear critical state system rests on two non-experimental bases; one deductive, one inductive. The deductive basis is the ubiquity of power curves as a description of the behavior of a wide variety of complex systems in natural and social sciences, e.g., earthquakes, forest fires, sunspots, polarity, drought, epidemiology, population dynamics, size of cities, wealth distribution, etc. This is all part of a more general movement in many natural and social sciences from 19th and early 20th century equilibrium models to non-equilibrium models; this trend has now caught up with financial economics.

The inductive basis is the large variety of capital markets behavior which has been empirically observed to fit well with the nonlinear paradigm. It is certainly more robust than VaR when it comes to explaining the extreme market movements described above. It is consistent with the fact that extreme events are not necessarily attributable to extreme causes but may arise spontaneously in the same initial conditions from routine causes.

While extreme events occur with much greater than normal frequency in nonlinear critical state systems, these events are nevertheless limited by the scale of the system itself. If the financial system is a self-organized critical system, as both empirical evidence and deductive logic strongly suggest, the single most important question from a risk management perspective is: *what is the scale of the system?* Simply put, the larger the scale of the system, the greater the potential collapse with correlative macroeconomic and other real world effects.

The news on this front is daunting. There is no normalized scale similar to the Richter Scale for measuring the size of markets or the size of disruptive events that occur within them, however, a few examples will make the point. According to recent estimates prepared by the McKinsey Global Institute, the ratio of world financial assets to world GDP grew from 100 percent in 1980 to 200 percent in 1993 to 316 percent in 2005. Over the same period, the absolute level of global financial assets increased from $12 trillion to $140 trillion. The drivers of this exponential increase in scale are globalization, derivative products, and leverage.

Globalization in this context is the integration of capital markets across national boundaries. Until recently there were specific laws and practices that had the effect of fragmenting capital markets into local or national venues with little interaction. Factors included withholding taxes, capital controls, protectionism, non-convertible currencies, licensing, regulatory and other restrictions that tilted the playing field in favor of local champions and elites. All of these impediments have been removed over the past 20 years to the point that the largest stock exchanges in the United States and Europe (NYSE and Euronext) now operate as a single entity.

Derivative products have exhibited even faster growth than the growth in underlying financial assets. This stems from improved technology in the structuring, pricing, and trading of such instruments and the fact that the size of the derivatives market is not limited by the physical supply of any stock or commodity but may theoretically achieve *any* size since the underlying instrument is notional rather than actual. The total notional value of all swaps increased from $106 trillion to $531 trillion between 2002 and 2006. The notional value of equity derivatives increased from $2.5 trillion to $11.9 trillion over the same period while the notional value of credit default swaps increased from $2.2 trillion to $54.6 trillion.

Leverage is the third element supporting the massive scaling of financial markets; margin debt of U.S. brokerage firms more than doubled from $134.58 billion to $293.2 billion from 2002 to 2007 while the amount of total assets per dollar of equity at major U.S. brokerage firms increased from approximately $20 to $26 in the same period. In addition, leveraged investors invest in other entities which use leverage to make still further investments. This type of layered leverage is impossible to unwind in a panic.

There can be no doubt that capital markets are larger and more complex than ever before. In a dynamically complex critical system, this means that the size of the maximum possible catastrophe is *exponentially* greater than ever. Recalling that systems described by a power curve allow events of all sizes and that such events can occur at any time, particularly when the system is super-critical, the conclusion is inescapable that progressively greater financial catastrophes of the type we are experiencing today should be expected frequently.

The more advanced risk practitioners have long recognized the shortcomings of using VaR in a normally distributed paradigm to compute risk measured in standard deviations from the norm. This is why they have added stress testing as an alternative or blended factor in their models. Such stress testing rests on historically extreme events such as the market reaction to 9–11 or the stock market crash of 1987. However, this methodology has its own flaws since the worst outcomes in a dynamically complex critical State system are not bounded by history but are only bounded by the scale of the system itself. Since the system is larger than ever, there is nothing in historical experience that provides a guide to the size of the largest catastrophe that can arise today. The fact that the financial crisis which began in July 2007 has lasted longer, caused greater losses and been more widespread both geographically and sectorally than most analysts predicted or can explain is because of the vastly greater scale of the financial system which produces an exponentially greater catastrophe than has ever occurred before. This is why the past is not a guide and why the current crisis may be expected to produce results as severe as the Great Depression of 1929–1941.

Policy Approaches and Recommendations

A clear understanding of the structures and vulnerabilities of the financial markets points the way to solutions and policy recommendations. These recommendations fall into the categories of limiting scale, controlling cascades, and securing informational advantage.

To explain the concept of limiting scale, a simple example will suffice. If the U.S. power grid east of the Mississippi River were at no point connected to the power grid west of the Mississippi River, a nationwide power failure would be an extremely low probability event. Either the “east system” or the “west system” could fail catastrophically in a cascading manner but both systems could not fail simultaneously except for entirely independent reasons because there are no nodes in common to facilitate propagation across systems. In a financial context, governments should give consideration to preventing mergers that lead to globalized stock and bond exchanges and universal banks. The first order efficiencies of such mergers are outweighed by the risks of large-scale failure especially if those risks are not properly understood and taken into account.

Another example will help to illustrate the relationship between the scale of a system and extent of the greatest catastrophe possible in that system. Imagine a vessel with a large hold. The hold is divided into three equal sections separated by watertight bulkheads. If a hole is punched in one section and that section is completely filled with water, the vessel will still float. Now imagine the watertight bulkheads are removed and the same hole is punched into the vessel. In this case, the entire hold will fill with water and the vessel will sink. In this example, the area of the hold can be thought of as the relevant dynamic system. The sinking of the vessel represents the catastrophic failure of the system. When the bulkheads are in place we have three small systems. When the bulkheads are removed we have one large system. By removing the bulkheads we increased the scale of the system by a factor of three. But the likelihood of failure did not increase by a factor of three; it went from practically zero to practically 100 percent. The system size tripled but the risk of sinking went up exponentially. By removing the bulkheads we created what engineers call a “single point of failure,” i.e., one hole is now enough to sink the entire vessel.

Something similar happened to our financial system between 1999 and 2004. This began with the repeal of Glass-Steagall in 1999 which can be thought of as removing the watertight bulkheads separating commercial banks and investment banks.

This was exacerbated by the *Commodities Futures Modernization Act of 2000* which removed the prohibition on many kinds of derivatives. This allowed banks to increase the scale of the system through off-balance sheet transactions. Finally, in 2004, the SEC amended the broker-dealer net capital rule in such a way that allowed brokers to go well-beyond the traditional 15:1 leverage ratio and to use leverage of 30:1 or more. All three of these events increased the scale of the system by allowing regulated financial institutions to enter new markets, trade new products and use increased leverage. Using a power curve analysis, we see that while the scale of the system was increased in a linear way (by a factor of three, five, ten or fifty depending on the product) the risk was increasing in a nonlinear way (by a factor of 100, 1000, or 10,000 depending on the slope of the power curve). VaR models based on normal distributions were reporting that risk was under control and sounding the all clear signal because so much of the risk was offsetting or seen to cancel out in the models. However, a power curve model would have been flashing a red alert sign because it does not depend on correlations, instead it sees risk as an emergent property and an exponential function of scale.

The fact that government opened the door to instability does not necessarily mean that the private sector had to rush through the door to embrace the brave new world of leveraged risk. For that we needed VaR. Without VaR models to tell bankers that risk was under control, managers would not have taken so much risk even if government rules allowed them to do so. Self-interest would have constrained them somewhat as Greenspan expected. But with VaR models telling senior management that risk was contained the new government rules became an open invitation to pile on massive amounts of risk which bankers promptly did.

Our financial system was relatively stable from 1934–1999 despite occasional failures of institutions (such as Continental Illinois Bank) and entire sectors (such as the S&L industry). This 65-year period can be viewed as the golden age of compartmented banking and moderate leverage under Glass-Steagall and the SEC's original net capital rule. Derivatives themselves were highly constrained by the *Commodity Exchange Act*. In 1999, 2000 and 2004 respectively, all three of these watertight bulkheads were removed. By 2006 the system was poised for the most catastrophic financial collapse in history. While subprime mortgage failures provided the catalyst, it was the scale of the system itself which caused the damage. The catalyst could just as well have come from emerging markets, commercial real estate or credit default swaps. In a dynamically critical system, the catalyst is always less important than the chain reaction and the reaction in this case was a massive collapse.

The idea of controlling cascades of failure is, in part, a matter of circuit breakers and pre-rehearsed crisis management so that nascent collapses do not spin into full systemic catastrophes before regulators have the opportunity to prevent the spread. The combination of diffuse credit and layered leverage makes it infeasible to assemble all of the affected parties in a single room to discuss solutions. There simply is not enough time or condensed information to respond in real time as a crisis unfolds.

One significant circuit breaker which has been discussed for over a decade but which has still not been fully implemented is a clearinghouse for all over-the-counter derivatives. Experience with clearinghouses and netting systems such as the Government Securities Clearing Corporation shows that gross risk can be reduced 90 percent or more when converted to net risk through the intermediation of a clearinghouse. Bearing in mind that a parametric decrease in scale produces an exponential decrease in risk in a nonlinear system, the kind of risk reduction that arises in a clearinghouse can be the single most important step in the direction of stabilizing the financial system today; much more powerful than bail outs which do not reduce risk but merely bury it temporarily.

A clearinghouse will also provide informational transparency that will allow regulators to facilitate the failure of financial institutions without producing contagion and systemic risk. Such failure (what Joseph Schumpeter called "creative destruction") is another necessary step on the road to financial recovery. Technical objections to clearinghouse implementation based on the non-uniformity of contracts can be overcome easily through consensual contractual modification with price adjustments upon joining the clearinghouse enforced by the understanding that those who refuse to join will be outside the safety net. Only by eliminating zombie institutions and creating breathing room for healthy institutions with sound balance sheets can the financial sector hope to attract sufficient private capital to replace government capital and thus re-start the credit creation process needed to produce sound economic growth.

Recently a number of alternative paradigms have appeared which not only do not rely on VaR but rather assume its opposite and build models that are more robust to empirical evidence and market price patterns. Several of these approaches are:

Behavioral Economics—This field relies on insights into human behavior derived from social science and psychology, in particular, the "irrational" nature of human decision-making when faced with economic choices. Insights include risk aversion, herding, the presence or absence of cognitive diversity and network effects among others. While not summarized in a general theory and while not always amendable to quantitative modeling, the insights of behavioral economics are powerful and should be considered in weighing reliance on VaR-style models which do not make allowance for subjective influences captured in this approach.

Imperfect Knowledge Economics—This discipline (under the abbreviation IKE) attempts to deal with uncertainty inherent in capital markets by using a combination of Bayesian networks, link analysis, causal inference and probabilistic hypotheses to fill in unknowns using the known. This method is heavily dependent on the proper construction of paths and the proper weighing of probabilities in each hypothesis cell or evidence cell, however, used properly it can guide decision-making without applying the straitjacket of VaR.

Econoahysics—This is a branch of financial economics which uses insights gained from physics to model capital markets behavior. These insights include nonlinearity in dynamic critical state systems the concept of phase transitions. Such systems exhibit an unpredictably deterministic nonlinear relationship between inputs and outputs (the so-called "Butterfly Effect") and scale invariance which accords well with actual time series of capital markets prices. Importantly, this field leads to a degree distribution characterized by the power curve rather than the bell curve with implications for scaling metrics in the management of systemic risk.

It may be the case that these risk management tools work best at distinct scales. For example, behavioral economics seems to work well at the level of individual decision-making but has less to offer at the level of the system as a whole where complex feedback loops cloud its efficacy. IKE may work best at the level of a single institution where the hypothesis and evidence cells can be reasonably well defined and populated. Econophysics may work best at the systemic level because it goes the furthest in its ability to model highly complex dynamics. This division of labor suggests that rather than replacing VaR with a one-size-fits-all approach, it may be best to adopt a nested hierarchy of risk management approaches resembling the following:

Econophysics
Normalized metrics for understanding *systemic* risk

|

Imperfect Knowledge Economics
Processes to aid risk and resource allocation at the *enterprise* level

|

Behavioral Economics
Experimentally derived tools to aid *individual* decision making

While all of these approaches and others not mentioned here require more research to normalize metrics and build general theories, they are efficacious and robust alternatives to EMH and VaR and their development and use can serve a stabilizing function since they have a strong empirical basis unlike EMH and VaR.

In summary, Wall Street's reigning risk management paradigm consisting of VaR using a normally distributed model combined with GARCH techniques applied to the non-normal region and stress testing to account for outliers is a manifest failure. It should be replaced at the systemic level with the empirically robust model based on nonlinear complexity and critical state dynamics as described by the power curve. This method also points the way to certain solutions, most importantly the creation of an over-the-counter derivatives clearinghouse which will de-scale the system and lead to an exponential decrease in actual risk. Such a clearinghouse can also be used to improve transparency and manage failure in ways that can leave the system far healthier while avoiding systemic collapse.

Importantly, if scale is the primary determinant of risk, as appears to be the case in complex systems such as the financial markets, then it follows that de-scaling the system is the simplest and most effective way to manage risk. This does not mean that the totality of the system needs to shrink, merely that it be divided into subcomponents with limited interaction. This has the same effect as installing the watertight bulkheads referred to in the example above. In this manner, severe financial distress in one sector does not automatically result in contagion among all sectors.

This effective de-scaling can be accomplished with three reforms:

1. The enactment of a modernized version of Glass-Steagall with a strict separation between commercial banking and deposit taking on the one hand and principal risk taking in capital markets on the other.
2. Strict requirements for all derivative products to be traded on exchanges subject to credit tests for firm memberships, initial margin, variation margin, position limits, price transparency and netting.
3. Higher regulatory capital requirements and reduced leverage for banks and broker-dealers. Traditional ratios of 8:1 for banks and 15:1 for brokers seem adequate provided off-balance sheet positions (other than exchange traded contracts for which adequate margin is posted) be included for this purpose.

These rules can be implemented directly and do not depend on the output of arcane and dangerous models such as VaR. Instead, they derive from another proven model, the power curve, which teaches that risk is an exponential function of scale. By de-scaling, we radically reduce risk and restore stability to individual institutions and to the system as a whole.

BIOGRAPHY FOR JAMES G. RICKARDS

James G. Rickards is Senior Managing Director for Market Intelligence at Omnis, Inc., a scientific consulting firm in McLean, VA. He is also Principal of Global–I Advisors, LLC, an investment banking firm specializing in capital markets and geopolitics. Mr. Rickards is a seasoned counselor, investment banker and risk manager with over thirty years experience in capital markets including all aspects of portfolio management, risk management, product structure, regulation and operations. Mr. Rickards's market experience is focused in alternative investing and derivatives in global markets.

Mr. Rickards was a first hand participant in the formation and growth of globalized capital markets and complex derivative trading strategies. He held senior executive positions at sell side firms (Citibank and RBS Greenwich Capital Markets) and buy side firms (Long-Term Capital Management and Caxton Associates) and technology firms (OptiMark and Omnis). Mr. Rickards has participated directly in many of the most significant financial events over the past 30 years including the release of U.S. hostages in Iran (1981), the Stock Market crash of 1987, the collapse of Drexel (1990), the Salomon Bros. bond trading scandal (1991) and the LTCM financial crisis of 1998 (in which Mr. Rickards was the principal negotiator of the government-sponsored rescue). He has founded several hedge funds and fund-of-funds. His advisory clients include private investment funds, investment banks and government directorates. Since 2001, Mr. Rickards has applied his financial expertise to missions for the benefit of the U.S. national security community.

Mr. Rickards is licensed to practice law in New York and New Jersey and the Federal Courts. Mr. Rickards has held all major financial industry licenses including Series 3 (National Commodities Futures), Series 7 (General Securities Representative), Series 24 (General Securities Principal), Series 30 (Futures Manager) and Series 63.

Mr. Rickards has been a frequent speaker at conferences sponsored by bar associations and industry groups in the fields of derivatives and hedge funds and is active in the International Bar Association. He has been the interviewed in *The Wall Street Journal* and on CNBC, Fox, CNN, NPR and C-SPAN and is an OpEd contributor to the *New York Times* and the *Washington Post.*

Mr. Rickards is a graduate school visiting lecturer in finance at the Kellogg School and the School of Advanced International Studies. He has delivered papers on econophysics at the Applied Physics Laboratory and the Los Alamos National Laboratory. Mr. Rickards has written articles published in academic and professional journals in the fields of strategic studies, cognitive diversity, network science and risk management. He is a member of the Business Advisory Board of Shariah Capital, Inc., an advisory firm specializing in Islamic finance and is a member of the International Business Practices Advisory Panel to the Committee on Foreign Investment in the United States (CFIUS) Support Group of the Director of National Intelligence.

Mr. Rickards holds the following degrees: LL.M. (Taxation) from the New York University School of Law; J.D. from the University of Pennsylvania Law School; M.A. in international economics from the School of Advanced International Studies, Washington DC; and a B.A. degree with honors from the School of Arts & Sciences of The Johns Hopkins University, Baltimore, MD.

Chairman MILLER. Thank you, Mr. Rickards. I did practice repeatedly saying "Taleb." I should have practiced "Rickards" as well.

Mr. Whalen.

STATEMENT OF MR. CHRISTOPHER WHALEN, MANAGING DIRECTOR, INSTITUTIONAL RISK ANALYTICS

Mr. WHALEN. Thank you, Mr. Chairman. I am going to just summarize a couple points further to my written testimony. You will notice in my comments I focused on the distinction between subjectivity and objectivity, and I think this committee is probably better placed to understand those distinctions than most of the other panels in the Congress.

You know, we have seen over the last 100 years in this country a shift in our financial world from focusing on current performance of companies and financial institutions to focusing on predicting the future. This is very well illustrated in the Graham and Dodd volume, Securities Analysis, in chapter 38 where they talk about new era investing, and I urge you to reread that if you have never done so before.

The bottom line to me as someone who has worked in the industry as a supervisor and a trader and investment banker, is that when you use assumptions and models, you have already stepped off the deep edge, you know, the deep end of the pool, and there's no water in the pool. You essentially are in the world of speculation, and you have left the world of investing. Why do I say this? Well, if we use the same rules that govern the assumptions that go into most VaR models to design airplanes and buildings and dams, all of these physical structures would fail, because they violate the basic rules of scientific method that the Members of this committee know very, very well. I would submit to you that if we are going to allow our financial system to design products that are based on assumptions rather than hard data, than we are in big trouble. My firm has over the last seven years shunned the quantitative world. Our entire methodology is focused on benchmarking the current performance of banks, and taking observations about that current performance that may suggest what they are going to do in the future. But we don't guess, we don't speculate. We have almost 20,000 retail customers who use the bank monitor to track the safety and soundness of their institution. It is an entirely mechanical survey process. We stress-test every bank in the United States the same way, whether it is J.P. Morgan or Cullen/Frost Bank in Texas. We ask the same question, how did you do this quarter, and we compare it to 1995, which was a nice, boring year.

The second point I would like to make is that I think a big part of the problem is that we allowed the economist profession to escape from the world of social sciences, and enter into an unholy union with commission-driven dealers in the securities market. Your colleague, Mr. Broun, said earlier that economists can't make up their mind. Well, yes, they can. When they are working in the securities business they have no trouble making up their mind. They offer opinions and hypotheses and 'what if' or 'I want' in regards to the creation of a security. This is a big problem. I wouldn't let most economists park my car, and the problem is not that they are not smart people, not that they are not interesting people, but they live in the world of supposition rather than the world of fact, and again, their methodologies are not governed by the iron rules that you find in physics or chemistry or any of the other physical sciences where you have to live by those rules. You can't come up with some neat concept and say to your colleagues, hey, look at me, or hey, look at this new CDO I designed, and then go out and sell that security to the public.

I think it all comes down at the end of the day to what kind of economy do we want. There is an old-fashioned American concept called 'fair dealing' that I spent a lot of time talking in my testimony to the Senate Banking Committee earlier this year, and it comes from the Greeks, the concept of proportional requital. One person gives value, the other person receives value. The problem with products like credit default swaps, is that they are entirely speculative. There is no visible underlying market for single-name credit default swaps really. The corporate bonds that are supposedly the derivative or the basis for the derivative are fairly liquid and not a very good source of pricing information, so we use models and we then sell these securities to anyone and everyone. I would submit that that is unfair, and it goes against the basic grain of American society that we are a fair and transparent nation. So bottom line to me is, if you want to fix the problem, I think we have got to reimpose not higher capital requirements on banks that are out of control, and which take risks that no one can really quantify. I think what we have to do is reimpose restrictions on their risk taking and get them to the point where an eight percent capital assets ratio makes sense again, because it clearly doesn't now. Does anybody really think we can get the private sector to double the capital of J.P. Morgan when their equity returns are going to be falling for the next couple of years? The only entity that would fund that opportunity would be a government, so what we are really saying is that these are GSEs. I think we have got to come back almost to the Glass-Steagall-era draconian division between the utility function of a bank and the transactional function of hedge funds, broker dealers, whatever, and that latter group can do whatever they want.

So let me stop there, and I look forward to your questions.

[The prepared statement of Mr. Whalen follows:]

PREPARED STATEMENT OF CHRISTOPHER WHALEN

Chairman Miller, Congressman Broun, Members of the Committee, my name is Christopher Whalen and I live in the State of New York. I work in the financial community as an analyst and a principal of a firm that rates the performance of commercial banks.[1] Thank you for inviting my comments today on this important subject.

The Committee has asked witnesses to comment on the topic of *"The Risks of Financial Modeling: VaR and the Economic Meltdown."* The comments below reflect my own views, as well as comments from my colleague and business partner Dennis Santiago, and others in the financial and risk management community.

By way of background, our firm provides ratings for assessing the financial condition of U.S. banks and commercial companies. We build the analytical tools that we use to support these rating activities and produce reports for thousands of consumer and professional users.

We use mathematical tools such as models to explore the current financial behavior of a given subject. In the course of our work, we use these tools to make estimates, for example, as to the maximum probable loss in a bank's loan portfolio through an economic cycle or the required Economic Capital for a financial institution. Models help us understand and illustrate how the financial condition of a bank or other obliger have changed and possibly will change in the future.

But in all that we at Institutional Risk Analytics do in the world of ratings and financial analysis, we do our best to separate objective measures based upon empirical observations, and subjective analyses that employ speculative assumptions and directives which are often inserted into the very ground rules for the analysis process itself. The difference between subjectivity and objectivity in finance has significant implications for national policy when it comes to financial markets and institutions.

I strongly suggest to the Committee that they bear the distinction between objective and subjective measures in mind when discussing the use of models in finance. Obtaining a better understanding of the role of inserting subjectivity into models is critical for distinguishing between useful deployments of modeling to manage risk and situations where models are the primary failure pathway towards creating systemic risk and thus affect economic stability and public policy.

Used as both a noun and a verb, the word "model" has become the symbol for the latest financial crisis because of the use, or more precisely, the misuse of such simulations to price unregistered, illiquid securities such as sub-prime mortgage backed securities and derivatives of such securities. The anecdotal cases where errant models have led to mischief are many and are not limited to the world of finance alone.

The Trouble with Models

The problem is not with models themselves. The trouble happens when they are (a) improperly constructed and then (b) deliberately misapplied by individuals working in the financial markets.

In the physical sciences, models can be very usefully employed to help analysts understand complex systems such as disease, buildings and aircraft. These models tend to use observable data as inputs, can be scientifically validated and are codified in a manner that is transparent to all involved in the process. Models used in the physical world share one thing in common that financial models do not: they are connected to and are confirmed or refuted by the physical world they describe.

Financial models, on the other hand, are all intellectual abstractions designed to manipulate arbitrarily chosen, human invented concepts. The chief reason for this digression from the objective use of models observed in the physical sciences is the injection of economics into the world of finance. Whereas financial models were once merely arithmetic expressions of expected cash flows, today in the world of financial economics, models have become vehicles for rampant speculation and outright fraud.[2]

In the world of finance, modeling has been an important part of the decision-making toolkit of executives and analysts for centuries, helping them to understand the various components in a company or a market and thereby adjust to take advantage of the circumstances. These decision analysis models seek to measure and report on key indicators of *actual performance* and confirm the position of the entity with respect to its' competitive environment. For instance, the arithmetic calculation of cash flows adheres to the scientific method of structures and dynamics, and is the foundation of modern finance as embodied by the great theorists such as Benjamin Graham and David Dodd.

[1] Mr. Whalen is a co-founder of *Institutional Risk Analytics,* a Los Angeles unit of Lord, Whalen LLC that publishes risk ratings and provides customized financial analysis and valuation tools.

[2] See "New Hope for Financial Economics: Interview with Bill Janeway," *The Institutional Risk Analyst,* November 17, 2008.

At our firm, we employ a "measure and report" model called The IRA Bank Monitor to survey and stress test all FDIC insured banks each quarter. By bench-marking the performance of banks with a consistent set of tests, we are able to not only characterize the relative safety and soundness of each institution, but can drawn reasonable inferences about the bank's future performance.

But when the world of finance marries the world of outcome driven economics—the world of "what if" and "I want"—models cease to be mechanistic tools for validating current outcomes with hard data and assessing a reasonable range of possible future events. Instead models become enablers for speculation, for the use of skillful canards and legal subterfuge that ultimately cheat investors and cause hundreds of billions of dollars in losses to private investors and insured depository institutions.

Take the world of mortgage backed securities or MBS. For decades the investment community had been using relatively simple models to predict the cash flow of MBS in various interest rate scenarios. These predictions have been relatively simple and are validated against the monthly mortgage servicer data available to the analyst community. The MBS securitization process was simple as well. A bank would sell conforming loans to GNMA and FNMA, and sell inferior collateral to a handful of investment banks on Wall Street to turn in the loans into private MBS issues.

At the beginning of the 1990's, however, Wall Street's private MBS secret sauce escaped. A firm named Drexel, Burnham, Lambert went bankrupt and the bankruptcy court sold copies of Drexel's structured finance software to anyone and everyone. It eventually wound up in the hands of the mortgage issuers themselves. These banks and non-banks naturally began to issue private MBS by themselves and discovered they could use the mathematics of modeling to grow their mortgage conduit businesses into massive cash flow machines. When brought to market, these private MBS were frequently under-collateralized and could therefore be described as a fraud.

Wall Street, in turn, created even more complex modeling systems to squeeze even more profits from the original MBS template. The expanding bubble of financial innovation caught the eye of policy-makers in the Congress, who then created political models envisioning the possibility that "innovation" could be used to make housing accessible to more Americans.

Spurred on to chase the "policy outcome" of affordable housing, an entire range of deliberately opaque and highly leveraged financial instruments were born with the full support of Washington, the GSEs and the Congress. Their purpose now was to use the alchemy of financial modeling to create the appearance of mathematical safety out of dangerous toxic ingredients. Wall Street firms paid the major rating agencies to award "AAA" ratings to derivative assets that were ultimately based on sub-prime mortgage debt. And the stage was set for a future economic disaster.

In the case of sub-prime toxic waste, the models became so complex that all transparency was lost. The dealers of unregulated, unregistered complex structured assets used proprietary models to price and sell deals, but since the "underlying" for these derivative securities was invisible, none of the investment or independent ratings community could model the security. There was no validation, no market discipline. Buy Side customers were dependent upon the dealer who sold them the toxic waste for valuation. The dealers that controlled the model often time would not even make a market in the security.

Clearly we have now many examples where a model or the pretense of a model was used as a vehicle for creating risk and hiding it. More important, however, is the role of financial models for creating opportunities for deliberate acts of securities fraud. These acts of fraud have caused hundreds of billions of dollars in losses to depository institutions and investors.

Whether you talk about toxic mortgage assets or credit default swaps, the one common element that the misuse of models seems to contain is a lack of a visible underlying market against which to judge or "mark" the model. Indeed, the use of models in a subjective context seems to include the simulation of a nonexistent market as the primary role for the financial model.

In single-name credit default swaps or "CDS" for example, there is often insufficient trading in the supposed underlying corporate debt security to provide true price discovery. In the case of CDS on complex structured assets, there is no underlying market to observe at all. The subjective model becomes the market in terms of pricing the security.

In the spring of 2007, however, the fantasy land consensus that allowed people to believe that a model is a market came undone. We have been dealing with the consequences of the decisions that originally built the house of cards since that time.

An Objective Basis for Finance and Regulation

The term “model” as it applies to finance can be a simulation of reality in terms of predicting future financial outcomes. The author Nassim Taleb, who is appearing at this hearing, says the term “VaR” or value at risk describes a statistical estimate of “the expected maximum loss (or worst loss) over a target horizon within a given confidence interval.” [3]

VaR models and similar statistical methods pretend to estimate the largest possible loss that an investor might experience over a given period of time to a given degree of certainty. The use of VaR type models, including the version embedded in the Basel II agreement, involves a number of assumptions about risk and outcomes that are speculative. More important, the widespread use of these statistical models for risk management suggest that financial institutions are subject to occasional “Black Swans” in the form of risk events that cannot be anticipated.

We take a different view. We don’t actually believe there is such a thing as a “Black Swan.” Our observations tell us that a more likely explanation is that leaders in finance and politics simply made the mistake of, again, believing in what were in fact flawed models and blinded themselves to what should have been plainly calculable innovation risks destined to be unsustainable. Or worse, our leaders in Washington and on Wall Street decided to be short sighted and not care about the inevitable debacle.

We suggest that going forward our national interest needs to demand a higher standard of tangible proof from “outcome designers” of public policies. If financial markets and the models used to describe them are limited to those instruments that can be verified objectively, then we no longer need to fear from the ravages of Black Swans or systemic risk. The source of systemic risk in the financial markets is fear born from the complexity of opaque securities for which there is no underlying basis. The pretext for issuing these ersatz securities depends on subjectivity injected into a flawed model.

If we accept that the sudden change in market conditions or the “Black Swan” event that Taleb and other theorists have so elegantly described arises from a breakdown in prudential regulation and basic common sense, and not from some unknowable market mechanism, then we no longer need to fear surprises or systemic risk. We need to simply ensure that all of the financial instruments in our marketplace have an objective basis, including a visible, cash basis market that is visible to all market participants. If investors cannot price a security without reference to subjective models, then the security should be banned from the U.S. markets as a matter of law and regulation. To do otherwise is to adopt deception as the public policy goal of the U.S. when it comes to financial markets regulation.

As Graham and Dodd wrote nearly a century ago, the more speculative the inputs the less the analysis matters. Models only have real value to society when their workings are disciplined by the real world. When investors, legislators and regulators all mistook models for markets, and even accepted such speculations as a basis for regulating banks and governing over-the-counter or OTC markets for all types of securities, we as a nation were gambling with our patrimony. If the Committee and the Congress want to bring an end to the financial crisis, we must demand higher standards from our citizens who work in and regulate our financial markets.

As we discussed in a commentary last month, “Systemic Risk: Is it Black Swans or Market Innovations?,” published in *The Institutional Risk Analyst,* “were the failures of Bear Stearns, Lehman Brothers, Washington Mutual or the other “rare” events really anomalous? Or are we just making excuses for our collective failure to identify and manage risk? A copy of our commentary follows this testimony. I look forward to your questions.

[3] See Taleb, Nassim, “Against Value-at-Risk: Nassim Taleb Replies to Philippe Jorion,” 1997.

Systemic Risk: Is it Black Swans or Market Innovations?

AUGUST 18, 2009

"Whatever you think you know about the distribution changes the distribution."
Alex Pollock
American Enterprise Institute

In this week's issue of The IRA, our friend and colleague Richard Alford, a former Fed of New York economist, and IRA founders Dennis Santiago and Chris Whalen, ask us whether we really see Black Swans in market crises or our own expectations. Of note, we will release our preliminary Q2 Banking Stress Index ratings on Monday, August 24, 2009. As with Q1, these figures represent about 90 percent of all FDIC insured depositories, but exclude the largest money center banks (aka the "Stress Test Nineteen"), thus providing a look at the state of the regional and community banks as of the quarter ended June 30, 2009. Click here to register for The Institutional Risk Analyst.

Many popular explanations of recent financial crises cite "Black Swan" events; extreme, unexpected, "surprise" price movements, as the causes of the calamity. However, in looking at our crisis wracked markets, we might consider that the Black Swan hypothesis doesn't fit the facts as well an alternative explanation: namely that the speculative outburst of financial innovation and the artificially low, short-run interest rate environment pursued by the Federal Open Market Committee, combined to change the underlying distribution of potential price changes. This shift in the composition of the distribution made likely outcomes that previously seemed impossible or remote. This shift in possible outcomes, in turn, generated surprise in the markets and arguably led to the emergence of "systemic risk" as a metaphor to explain these apparent "anomalies."

But were the failures of Bear Stearns, Lehman Brothers, Washington Mutual or the other "rare" events really anomalous? Or are we just making excuses for our collective failure to identify and manage risk?

The choice of which hypothesis to ultimately accept in developing the narrative description of the causation of the financial crisis has strategic implications for understanding as well as reducing the likelihood of future crisis, including the effect on the safety and soundness of financial institutions. To us, the hard work is not trying to specifically limit the range of possibilities with artificial assumptions, but to model risk when you must assume as a hard rule, like the rules which govern the physical sciences, that the event distribution is in constant flux.

If we as financial and risk professional are serious in claims to model risk proactively, then change, not static assumptions, must be the rule in terms of the possible outcomes. Or "paranoid and nimble" in practical terms. After all, these modeling exercises ultimately inform and support risk assumptions for decisions that are used in value-at-risk (VaR) assessments for investors and for capital adequacy bench-marking for financial institutions.

Even before the arrival of Benoit Mandelbrot in the 1960s, researchers had observed that distributions of price changes in various markets were not normally distributed. The observed distributions of price changes had fatter tails than the normal distribution. Nassim Nicolas Taleb, author of *The Black Swan* and *Fooled by Randomness,* and others have dubbed significantly larger extreme price moves than those predicted by a normal distribution as "Black Swans." Indeed, Taleb and others have linked Black Swan price change events to the recent financial crisis, suggesting in effect that we all collectively misunderstood on which side of the distribution of possible risk outcomes we stood.

The argument is as follows: Current risk management and derivative pricing regimes are based upon normal distributions. Price movements in the recent financial crises were unpredictable/low probability events that were also greater than predicted by normal distribution models. Hence our collective failure to anticipate Black Swan events is "responsible" for the recent crises as mis-specified risk management models failed due to fatter than normal tails.

The alternative explanation, however, links the extreme price movements not to aberrations with respect to a stable, observable mean, but instead to the activation of alternate stable means as a result of jumping discontinuously through tipping points—much in the same way particles jump quantum levels in energy states when subjected to the cumulative effects of energy being added to or removed from their environments. These tipping points are as predictable as the annual migrations of ducks. Swans, alas, rarely migrate, preferring to stay in their summer feeding grounds until the water freezes, then move only far enough to find open water. Sound familiar?

Force feed a system with enough creative energy via permissive public policies and the resulting herd behaviors, and the system will change to align around these new norms, thereby erasing the advantages of the innovators and creating unforeseen hazards. "Advances" such as OTC derivatives and complex structured assets, and very accommodating Fed interest rate policy, resulted in unprecedented leverage and maturity mismatches by institutions and in markets that are the perfect quantum fuel to brew such change.

While the exact timing of each tipping point and magnitude of the crises remains somewhat inexact, the waves of change and the ultimate crisis borne shift are broadly predictable. The probabilities attached to extreme price moves are calculable as the cost of deleveraging an accumulation of innovation risk that must be shed as the system realigns. The "Black Swan" approach assumes a stable distribution of price changes with fatter than "normal" tails. The alternative posits that the distribution of possible price changes was altered by innovation and the low cost of leverage. It also posits that the new distributions allowed, indeed require, more extreme price movements. Two examples will illustrate the alternative hypothesis.

Once upon a time, the convertible bond market was relatively quiet. The buy side was dominated by real money (unleveraged) players who sought the safety of bonds, but were willing to give up some return for some upside risk (the embedded equity call option).

More recently the market has been dominated by leveraged hedge funds doing convertible bond arbitrage. They bought the bonds, hedging away the various risks. In response to the advent of the arbitrageurs, the spread between otherwise similar conventional and convertible bonds moved to more accurately reflect the value of the embedded option and became less volatile.

When the financial crises hit, however, arbitrageurs were forced to liquidate their positions as losses mounted and it became difficult to fund the leveraged positions. Prices for convertible bonds declined and for a period were below prices for similar conventional bonds—something that had been both unheard of and considered impossible as the value of an option cannot be negative.

Was this a Black Swan type event, or had the market for convertible bonds and the underlying distribution of price changes, been altered? The mean spread between otherwise similar conventional and convertible bonds had changed. The volatility of the spread had changed. Forced sales and the public perception of possible future forced sales generated unprecedented behavior of the heretofore stable spread. The emergence and then dominance of leveraged arbitrage positions altered the market in fundamental ways. What had not been possible had become possible.

Now consider bank exposures to commercial real estate. Numerous financial institutions, hedge funds (e.g., at Bear Stearns), sellers of CDS protection (e.g., AIG) and banks (many of them foreign as reflected in the Fed swap lines with foreign central banks) suffered grievous losses when the real estate bubble popped. Much of these losses remain as yet unrealized.

As investors and regulators demanded asset-write downs and loss realization, many of these institution expressed dismay. They had stressed tested their portfolios, the large banks complained, often with the support of regulators. The large banks thought their geographically diversified portfolios of MBSs immunize them from falls in real estate prices as the US had experienced regional, but never (except for the 1930s) nationwide declines in housing prices. These sophisticated banks incorporated that assumption into their stress test even as they and the securitization process were nationalizing—that is, changing—the previously regional and local mortgage markets.

Was the nationwide decline in housing prices an unpredictable Black Swan event or the foreseeable result of lower lending standards, a supportive interest rate environment, and financial innovation the led to the temporary nationalization of the mortgage market? Risk management regimes failed and banks have been left with unrealized losses that still threaten the solvency of the entire system in Q3 2009.

However useful or necessary "normal" statistical measures such as VaR might be, it will not be sufficient to insulate institutions or the system from risk arising from rapidly evolving market structures and practices. Furthermore, insofar as models such as VaR, which are now enshrined in the bank regulatory matrix via Basel II, were the binding constraint on risk taking, it acted perversely, allowing ever greater leverage as leveraged trading acted to reduce measured volatility! Remember, the convertible bond market at first looked placid as a lake as leverage grew—but then imploded in a way few thought possible. Is this a Black Swan event or a failure of the stated objectives of risk management and prudential oversight?

We all know that risk management systems based solely on analysis of past price moves will at some point fall if financial markets continue to change. The problem with current risk management systems cannot be fixed by fiddling with VaR or other statical models. Risk management regimes must incorporate judgments about the evolution of the underlying markets, distribution of possible price changes and other dynamic sources of risk.

Indeed, as we discussed last week ("Are You Ready for the Next Bank Stress Tests"), this is precisely why IRA employs quarterly surveys of bank stress tests to benchmark the US banking industry. Think of the banking industry as a school of fish, moving in generally the same direction, but not uniformly or even consistently. There is enormous variation in the past of each member of the school, even though from a distance the group seems to move in unison.

Stepping back from the narrow confines of finance for a moment, consider that the most dramatic changes in the world are arguably attributable to asymmetric confluences of energy changing the direction of human history. It's happened over and over again. The danger has and always will be the immutable law of unintended consequences, which always comes back to bite the arrogant few who believe they can control the future outcome. And it is always the many of us who pay the price for these reckless leaps of faith.

If the recent financial crises were truly highly infrequent random events, then any set of policies that can continuously prevent their reoccurrence seemingly will be very expensive in terms of idle capital and presumably less efficient markets required to avoid them. If, on the other hand, the crisis was the result of financial innovation and the ability to get leveraged cheaply, then society need not continuously bare all the costs associated with preventing market events like the bursting of asset bubbles.

Policy-makers would like everyone to believe that the recent crises were random unpredictable Black Swan events. How can they be blamed for failing to anticipate a low probability, random, and unpredictable event? If on the other hand, the crises had observable antecedents, e.g., increased use of leverage, maturity mismatches, near zero default rates, and spikes in housing price to rental rates and housing price to income ratios, then one must ask: why policy-makers did not connect the dots, attach significant higher than normal probabilities to the occurrence of severe financial disturbances, and fashion policies accordingly? Ultimately, that is a question that Ben Bernanke and the rest of the federal financial regulatory community still have yet to answer.

Questions? Comments? *info@institutionalriskanalytics.com*

BIOGRAPHY FOR CHRISTOPHER WHALEN

Christopher is co-founder of *Institutional Risk Analytics,* the Los Angeles based provider of risk management tools and consulting services for auditors, regulators and financial professionals. Christopher leads IRA's risk advisory practice and consults for global companies on a variety of financial and regulatory issues. He is a Fellow of the *Networks Financial Institute* at Indiana State University. Christopher volunteers as a regional director of Professional Risk Managers International Association (*www.prmia.org*) and is a board adviser to I-OnAsia Limited (*www.ionasia.com.hk*), a global business security and risk consultancy based in Hong Kong. Christopher currently edits *The Institutional Risk Analyst,* a weekly news report and commentary on significant developments in and around the global financial markets. Christopher has testified before the Congress and the SEC on a variety of issues and contributes articles and commentaries to publications such as *The International Economy, American Banker* and *The Big Picture.*

Chairman MILLER. Thank you.

Dr. Colander.

STATEMENT OF DR. DAVID COLANDER, CHRISTIAN A. JOHNSON DISTINGUISHED PROFESSOR OF ECONOMICS, MIDDLEBURY COLLEGE

Dr. COLANDER. Mr. Chairman, thanks for the opportunity to testify. I am Dave Colander, the Christian A. Johnson Distinguished Professor of Economics at Middlebury College. I was invited here because I was one of the authors of the Dahlem Report in which we chided the economics profession for its failure to warn society about the impending financial crisis.

Some non-economists have blamed the financial crisis on economists' highly technical models. My argument is that the problem isn't the models, the problem is the way the economic models are used, and I think a number of the other panelists have made that point. Where I am going to lead or go with that is that the issue goes much deeper than just with VaR and the various models you are looking at, and it goes very much to the general arguments about science and technology and the way in which economists approach problems, and I think, you know, Mr. Whalen had it directly right: we live in the world of supposition. Why? Because that is what our incentives are. We write articles. We advance through writing articles, we don't advance by designing something positive. If we are working for a business, we do, but within academics it is very much directed towards, you know, sort of what can we publish, and so I think Value-at-Risk models are part of a much broader economic problem, you know, sort of in terms of what economists accept and how they go about doing what they are doing.

An example I want to give is really about macroeconomics, you know, sort of in the dominant model in macroeconomics, which is the dynamic stochastic general equilibrium (DSGE) model, which is a big model designed very much along the same lines about efficient markets. It sort of took efficient markets and said, what if we had efficient markets in the entire economy? To get that model, you have to assume there is one individual, because we can't solve it unless there is only one individual. We have to assume that person is globally rational, understands everything and he has complete knowledge in looking into the infinite future, and then we can actually solve it for a very small case.

By definition, this model rules out strategic coordination problems. What would happen if somebody else did something else? That is obviously the likely cause of the recent crisis, but it was simply assumed away in the macroeconomic model and that macroeconomic model has been dominant for the last 30 years and has been funded by NSF, the research, you need to be looking into that.

If the DSGE model had been seen as an aid to common sense, it could have been a useful model. It improved some of the problems that some earlier models had. But for a variety of sociological reasons that I don't have time to go into here, a majority of macroeconomists started believing the DSGE model was useful, not just as an aid to our understanding but as the model of the macroeconomy. As that DSGE model became dominant, really important research on the whole set of broader non-linear and complex dynamic models that would have really served some foundation for thinking about these issues just wasn't done. It just wasn't allowed. You couldn't get anything published on it in the main macro journals.

Similar developments occurred with the efficient market finance models, which made assumptions very similar to the DSGE model. And so, again, at first these served a useful purpose. They led to technological advances in risk management and financial markets. But as happened in macro, the users of these financial models forgot that the models provide, at best, half-truths. They stopped using models with common sense and judgment. What that means is that warning labels should be put on models, and that should be in bold print, 'these models are based on assumptions that do not fit the real world and thus these models should be not relied on very heavily.' Those warning labels haven't been there.

How did something so stupid like this happen in economics? It didn't happen because economists are stupid, and I appreciate the people before who said we are not. We are very bright. It happened because of incentives within the economics profession and those incentives lead researchers to dot i's and cross t's of existing models. It is a lot easier to do that than to design a whole new model that nobody else, a peer, can really review. So they don't explore the wide range of alternative models, and they don't focus their research on interpreting and seeing that models are used in policy in a common sense fashion.

So let me conclude with just two brief suggestions which relate to issues under the jurisdiction of this committee that might decrease the probability of such events happening in the future, and these are far off but it has to do with, you know, sort of the incentives for economists. The first is a proposal that might add some common sense check on models. Such a check is needed because currently there is a nature of the internal to the sub-field peer review system, that works within NSF and within the system, that allows for what can only be called an incestuous mutual reinforcement of researchers' views with no common sense filter on those views. My proposal is to include a wider range of peers in the reviewing process for the National Science Foundation grants in the social sciences. For example, physicists, mathematicians, statisticians and even business and government representatives could serve on reviewing those, and it would serve as a useful common sense check, you know, about what is going on.

The second is a proposal to increase the number of researchers trained in interpreting models, rather than developing models, by providing research grants to do precisely that. In a sense, what I am suggesting is an applied science division of the National Science Foundation, a social science component. This division would fund work on the usefulness of models and would be responsible for adding the warning labels that should have been attached to those models.

The applied research would not be highly technical and would involve a quite different set of skills than the standard scientific research requires. It would require researchers to have an intricate knowledge—consumer's knowledge of the theory, but not a producer's knowledge of that theory. In addition, it would require a knowledge of institutions, methodology, previous literature and a sensibility of how the system works. These are all skills that are not taught in graduate economics today, but they are skills that underlie judgment and common sense. By providing NSF grants for this work, the NSF would encourage the development of a group of economists who specialize in interpreting models and applying models to the real world. The development of such a group would go a long way toward placing the necessary warning labels on models. Thank you.

[The prepared statement of Dr. Colander follows:]

PREPARED STATEMENT OF DAVID COLANDER

Mr. Chairman and Members of the Committee: I thank you for the opportunity to testify. My name is David Colander. I am the Christian A. Johnson Distinguished Professor of Economics at Middlebury College. I have written or edited over forty books, including a top-selling principles of economics textbook, and 150 articles on various aspects of economics. I was invited to speak because I was one of the authors of the Dahlem Report in which we chided the economics profession for its failure to warn society about the impending financial crisis, and I have been asked to expand on some of the themes that we discussed in that report. (I attach that report as an appendix to this testimony.)

Introduction

One year ago, almost to the day, the U.S. economy had a financial heart attack, from which it is still recovering. That heart attack, like all heart attacks, was a shock, and it has caused much discussion about who is to blame, and how can we avoid such heart attacks in the future. In my view much of that discussion has been off point. To make an analogy to a physical heart attack, the U.S. had a heart attack because it is the equivalent of a 450-pound man with serious ailments too numerous to list, who is trying to live as if he were still a 20-year-old who can party 24–7. It doesn't take a rocket economist to know that that will likely lead to trouble. The questions I address in my testimony are: Why didn't rocket economists recognize that, and warn society about it? And: What changes can be made to see that it doesn't happen in the future?

Some non-economists have blamed the financial heart attack on economist's highly technical models. In my view the problem is not the models; the problem is the way economic models are used. All too often models are used in lieu of educated common sense, when in fact models should be used as an aid to educated common sense. When models replace common sense, they are a hindrance rather than a help.

Modeling the Economy as a Complex System

Using models within economics or within any other social science, is especially treacherous. That's because social science involves a higher degree of complexity than the natural sciences. The reason why social science is so complex is that the basic unit in social science, which economists call agents, are strategic, whereas the basic unit of the natural sciences are not. Economics can be thought of the physics with strategic atoms, who keep trying to foil any efforts to understand them and bring them under control. Strategic agents complicate modeling enormously; they make it impossible to have a perfect model since they increase the number of calculations one would have to make in order to solve the model beyond the calculations the fastest computer one can hypothesize could process in a finite amount of time.

Put simply, the formal study of complex systems is really, really, hard. Inevitably, complex systems exhibit path dependence, nested systems, multiple speed variables, sensitive dependence on initial conditions, and other non-linear dynamical properties. This means that at any moment in time, right when you thought you had a result, all hell can break loose. Formally studying complex systems requires rigorous training in the cutting edge of mathematics and statistics. It's not for neophytes.

This recognition that the economy is complex is not a new discovery. Earlier economists, such as John Stuart Mill, recognized the economy's complexity and were very modest in their claims about the usefulness of their models. They carefully presented their models as aids to a broader informed common sense. They built this modesty into their policy advice and told policy-makers that the most we can expect from models is half-truths. To make sure that they did not claim too much for their scientific models, they divided the field of economics into two branches-one a scientific branch, which worked on formal models, and the other political economy, which was the branch of economics that addressed policy. Political economy was seen as an art which did not have the backing of science, but instead relied on the insights from models developed in the scientific branch supplemented by educated common sense to guide policy prescriptions.

In the early 1900s that two-part division broke down, and economists became a bit less modest in their claims for models, and more aggressive in their application of models directly to policy questions. The two branches were merged, and the result was a tragedy for both the science of economics and for the applied policy branch of economics.

It was a tragedy for the science of economics because it led economists away from developing a wide variety of models that would creatively explore the extraordinarily difficult questions that the complexity of the economy raised, questions for which new analytic and computational technology opened up new avenues of investigation.[1] Instead, the economics profession spent much of its time dotting i's and crossing t's on what was called a Walrasian general equilibrium model which was more analytically tractable. As opposed to viewing the supply/demand model and its macroeconomic counterpart, the Walrasian general equilibrium model, as interesting models relevant for a few limited phenomena, but at best a stepping stone for a formal understanding of the economy, it enshrined both models, and acted as if it explained everything. Complexities were just assumed away not because it made sense to assume them away, but for tractability reasons. The result was a set of models that would not even pass a perfunctory common sense smell test being studied ad nauseam.

Initially macroeconomics stayed separate from this broader unitary approach, and relied on a set of rough and ready models that had little scientific foundation. But in the 1980s, macroeconomics and finance fell into this "single model" approach. As that happened it caused economists to lose sight of the larger lesson that complexity conveys—that models in a complex system can be expected to continually break down. This adoption by macroeconomists of a single-model approach is one of the reasons why the economics profession failed to warn society about the financial crisis, and some parts of the profession assured society that such a crisis could not happen. Because they focused on that single model, economists simply did not study and plan for the inevitable breakdown of systems that one would expect in a complex system, because they had become so enamored with their model that they forgot to use it with common sense judgment.

Models and Macroeconomics

Let me be a bit more specific. The dominant model in macroeconomics is the dynamic stochastic general equilibrium (DSGE) model. This is a model that assumes there is a single globally rational representative agent with complete knowledge who is maximizing over the infinite future. In this model, by definition, there can be no strategic coordination problem—the most likely cause of the recent crisis—such problems are simply assumed away. Yet, this model has been the central focus of macro economists' research for the last thirty years.

Had the DSGE model been seen as an aid to common sense, it could have been a useful model. When early versions of this model first developed back in the early 1980s, it served the useful purpose of getting some inter-temporal issues straight that earlier macroeconomic models had screwed up. But then, for a variety of sociological reasons that I don't have time to go into here, a majority of macroeconomists started believing that the DSGE model was useful not just as an aid to our understanding, but as *the* model of the macroeconomy. That doesn't say much for the common sense of rocket economists. As the DSGE model became dominant, important research on broader non-linear dynamic models of the economy that would have been more helpful in understanding how an economy would be likely to crash, and what government might do when faced with a crash, was not done.[2]

Similar developments occurred with efficient market finance models, which make similar assumptions to DSGE models. When efficient market models first developed, they were useful; they led to technological advances in risk management and financial markets. But, as happened with macro, the users of these financial models forgot that models provide at best half truths; they stopped using models with common sense and judgment. The modelers knew that there was uncertainty and risk in these markets that when far beyond the risk assumed in the models. Simplification is the nature of modeling. But simplification means the models cannot be used directly, but must be used judgment and common sense, with a knowledge of the limitations of use that the simplifications require. Unfortunately, the warning labels on the models that should have been there in bold print—**these models are based on assumptions that do not fit the real world, and thus the models should not be relied on too heavily**—were not there. They should have been, which is

[1] Some approaches working outside this Walrasian general equilibrium framework that I see as promising includes approaches using adaptive network analysis, agent based modeling, random graph theory, ultrametrics, combinatorial stochastic processes, co-integrated vector auto-regression, and the general study of non-linear dynamic models.

[2] Among well known economists, Robert Solow stands out in having warned about the use of DSGE models for policy. (See Solow, in Colander, 2007, pg. 235.) He called them "rhetorical swindles." Other economists, such as Post Keynesians, and economic methodologists also warned about the use of these models. For a discussion of alternative approaches, see Colander, ed. (2007). So alternative approaches were being considered, and concern about the model was aired, but those voices were lost in the enthusiasm most of the macroeconomics community showed for these models.

why in the Dahlem Report we suggested that economic researchers who develop these models be subject to a code of ethics that requires them to warn society when economic models are being used for purposes for which they were not designed.

How did something so stupid happen in economics? It did not happen because economists are stupid; they are very bright. It happened because of incentives in the academic profession to advance lead researchers to dot i's and cross t's of existing models, rather than to explore a wide range of alternative models, or to focus their research on interpreting and seeing that models are used in policy with common sense. Common sense does not advance one very far within the economics profession. The over-reliance on a single model used without judgment is a serious problem that is built into the institutional structure of academia that produces economic researchers. That system trains show dogs, when what we need are hunting dogs.

The incorrect training starts in graduate school, where in their core courses students are primarily trained in analytic techniques useful for developing models, but not in how to use models creatively, or in how to use models with judgment to arrive at policy conclusions. For the most part policy issues are not even discussed in the entire core macroeconomics course. As students at a top graduate school said, "Monetary and fiscal policy are not abstract enough to be a question that would be answered in a macro course" and "We never talked about monetary or fiscal policy, although it might have been slipped in as a variable in one particular model." (Colander, 2007, pg. 169).

Suggestions

Let me conclude with a brief discussion of two suggestions, which relate to issues under the jurisdiction of this committee, that might decrease the probability of such events happening in the future.

Include a wider range of peers in peer review

The first is a proposal that might help add a common sense check on models. Such a check is needed because, currently, the nature of internal-to-the-subfield peer review allows for an almost incestuous mutual reinforcement of researcher's views with no common sense filter on those views. The proposal is to include a wider range of peers in the reviewing process of NSF grants in the social sciences. For example, physicists, mathematician, statisticians, and even business and governmental representatives, could serve, along with economists, on reviewing committees for economics proposals. Such a broader peer review process would likely both encourage research on much wider range of models and would also encourage more creative work.

Increase the number of researchers trained to interpret models

The second is a proposal to increase the number of researchers trained in interpreting models rather than developing models by providing research grants to do that. In a sense, what I am suggesting is an applied science division of the National Science Foundation's social science component. This division would fund work on the usefulness of models, and would be responsible for adding the warning labels that should have been attached to the models.

This applied research would not be highly technical and would involve a quite different set of skills than the standard scientific research would require. It would require researchers who had an intricate consumer's knowledge of theory but not a producer's knowledge. In addition it would require a knowledge of institutions, methodology, previous literature, and a sensibility about how the system works. These are all skills that are currently not taught in graduate economics programs, but they are the skills that underlie judgment and common sense. By providing NSF grants for this work, the NSF would encourage the development of a group of economists who specialized in interpreting models and applying models to the real world. The development of such a group would go a long way toward placing the necessary warning labels on models, and make it less likely that fiascoes like a financial crisis would happen again.

Bibliography

Colander, David. 2006. (ed.) *Post Walrasian Macroeconomics: Beyond the Dynamic Stochastic General Equilibrium Model.* Cambridge, UK. Cambridge University Press.

Colander, David. 2007. *The Making of an Economist Redux.* Princeton, New Jersey, Princeton University Press.

Solow, Robert. 2007. "Reflections on the Survey" in Colander (2007).

Appendix

Appendix
The Financial Crisis and the Systemic Failure of Academic Economics*

David Colander,
Department of Economics
Middlebury College
Middlebury, VE, USA

Hans Föllmer
Department of Mathematics
Humboldt University Berlin
Berlin, Germany

Armin Haas
Potsdam Institute for Climate Impact Research
Potsdam, Germany

Michael Goldberg
Whittemore School of Business & Economics
University of New Hampshire
Durham, NH, USA

Katarina Juselius
Department of Economics
University of Copenhagen
Copenhagen, Denmark

Alan Kirman
GREQAM, Université d'Aix-Marseille lll,
EHESS et IUF
Marseille, France

Thomas Lux
Department of Economics
University of Kiel
&
Kiel Institute for the World Economy
Kiel, Germany

Brigitte Sloth
Department of Business and Economics
University of Southern Denmark
Odense, Denmark

Abstract: The economics profession appears to have been unaware of the long build-up to the current worldwide financial crisis and to have significantly underestimated its dimensions once it started to unfold. In our view, this lack of understanding is due to a misallocation of research efforts in economics. We trace the deeper roots of this failure to the profession's focus on models that, by design, disregard key elements driving outcomes in real-world markets. The economics profession has failed in communicating the limitations, weaknesses, and even dangers of its preferred models to the public. This state of affairs makes clear the need for a major reorientation of focus in the research economists undertake, as well as for the establishment of an ethical code that would ask economists to understand and communicate the limitations and potential misuses of their models.

Keywords: financial crisis, academic moral hazard, ethic responsibility of researchers

* This opinion paper is the outcome of one week of intense discussions within the working group on 'Modeling of Financial Markets' at the 98th Dahlem Workshop, 2008. David Colander served as moderator of this group and Thomas Lux served as Rapporteur. We are grateful to Carlo Jaeger and Rupert Klein for organizing this stimulating meeting and to Deirdre McCloskey and Peter Sørensen and other participants for helpful comments.

1. Introduction

The global financial crisis has revealed the need to rethink fundamentally how financial systems are regulated. It has also made clear a *systemic failure of the economics profession.* Over the past three decades, economists have largely developed and come to rely on models that disregard key factors—including heterogeneity of decision rules, revisions of forecasting strategies, and changes in the social context—that drive outcomes in asset and other markets. It is obvious, even to the casual observer that these models fail to account for the actual evolution of the real-world economy. Moreover, the current academic agenda has largely crowded out research on the inherent causes of financial crises. There has also been little exploration of early indicators of system crisis and potential ways to prevent this malady from developing. In fact, if one browses through the academic macroeconomics and finance literature, "systemic crisis" appears like an otherworldly event that is absent from economic models. Most models, by design, offer no immediate handle on how to think about or deal with this recurring phenomenon.[3] In our hour of greatest need, societies around the world are left to grope in the dark without a theory. That, to us, is a *systemic failure of the economics profession.*

The implicit view behind standard equilibrium models is that markets and economies are inherently stable and that they only temporarily get off track. The majority of economists thus failed to warn policy-makers about the threatening system crisis and ignored the work of those who did. Ironically, as the crisis has unfolded, economists have had no choice but to abandon their standard models and to produce hand-waving common sense remedies. Common sense advice, although useful, is a poor substitute for an underlying model that can provide much-needed guidance for developing policy and regulation. It is not enough to put the existing model to one side, observing that one needs, "exceptional measures for exceptional times." What we need are models capable of envisaging such "exceptional times."

The confinement of macroeconomics to models of stable states that are perturbed by limited external shocks and that neglect the intrinsic recurrent boom-and-bust dynamics of our economic system is remarkable. After all, worldwide financial and economic crises are hardly new and they have had a tremendous impact beyond the immediate economic consequences of mass unemployment and hyper inflation. This is even more surprising, given the long academic legacy of earlier economists' study of crisis phenomena, which can be found in the work of Walter Bagehot (1873), Axel Leijonhuvfud (2000), Charles Kindleberger (1989), and Hyman Minsky (1986), to name a few prominent examples. This tradition, however, has been neglected and even suppressed.

The most recent literature provides us with examples of blindness against the upcoming storm that seem odd in retrospect. For example, in their analysis of the risk management implications of CDOs, Krahnen (2005) and Krahnen and Wilde (2006) mention the possibility of an increase of 'systemic risk.' But, they conclude that this aspect should not be the concern of the banks engaged in the CDO market, because it is the governments' responsibility to provide costless insurance against a system-wide crash. We do not share this view. On the more theoretical side, a recent and prominent strand of literature essentially argues that consumers and investors are too risk averse because of their memory of the (improbable) event of the Great Depression (e.g., Cogley and Sargent, 2008). Much of the motivation for economics as an academic discipline stems from the desire to explain phenomena like unemployment, boom and bust cycles, and financial crises, but dominant theoretical models exclude many of the aspects of the economy that will likely lead to a crisis. Confining theoretical models to 'normal' times without consideration of such defects might seem contradictory to the focus that the average taxpayer would expect of the scientists on his payroll.

This failure has deep methodological roots. The often heard definition of economics—that it is concerned with the 'allocation of scarce resources'—is short-sighted and misleading. It reduces economics to the study of optimal decisions in well-specified choice problems. Such research generally loses track of the inherent dynamics of economic systems and the instability that accompanies its complex dynamics. Without an adequate understanding of these processes, one is likely to miss the major factors that influence the economic sphere of our societies. This insufficient definition of economics often leads researchers to disregard questions about the coordination of actors and the possibility of coordination failures. Indeed, analysis of these issues would require a different type of mathematics than that which is generally used now by many prominent economic models.

Many of the financial economists who developed the theoretical models upon which the modern financial structure is built were well aware of the strong and highly unrealistic restrictions imposed on their models to assure stability. Yet, financial economists gave little warning to the public about the fragility of their models,[4] even as they saw individuals and businesses build a financial system based on their work. There are a number of possible explanations for this failure to warn the public. One is a "lack of understanding" explanation—the researchers did not know the models were fragile. We find this explanation highly unlikely; financial engineers are extremely bright, and it is almost inconceivable that such bright individ-

[3] Reinhart and Rogoff (2008) argue that the current financial crisis differs little from a long chain of similar crises in developed and developing countries. We certainly share their view. The problem is that the received body of models in macro finance to which the above authors have prominently contributed provides no room whatsoever for such recurrent boom and bust cycles. The literature has, therefore, been a major source of the illusory 'this time it is different' view that the authors themselves criticize.

[4] Indeed, few researchers explored the consequences of a breakdown of their assumptions, even though this was rather likely.

uals did not understand the limitations of the models. A second, more likely explanation, is that they did not consider it their job to warn the public. If that is the cause of their failure, we believe that it involves a misunderstanding of the role of the economist, and involves an ethical breakdown. In our view, economists, as with all scientists, *have an ethical responsibility to communicate the limitations of their models and the potential misuses of their research.* Currently, there is no ethical code for professional economic scientists. There should be one.

In the following pages, we identify some major areas of concern in theory and applied methodology and point out their connection to crisis phenomena. We also highlight some promising avenues of study that may provide guidance for future researchers.

2. Models (or the Use of Models) as a Source of Risk

The economic textbook models applied for allocation of scarce resources are predominantly of the Robinson Crusoe (representative agent) type. Financial market models are obtained by letting Robinson manage his financial affairs as a sideline to his well-considered utility maximization over his (finite or infinite) expected lifespan taking into account with correct probabilities all potential future happenings. This approach is mingled with insights from Walrasian general equilibrium theory, in particular the finding of the Arrrow-Debreu two-period model that all uncertainty can be eliminated if only there are enough contingent claims (i.e., appropriate derivative instruments). This theoretical result (a theorem in an extremely stylized model) underlies the common belief that the introduction of new classes of derivatives can only be welfare increasing (a view obviously originally shared by former Fed Chairman Greenspan). It is worth emphasizing that this view is not an empirically grounded belief but an opinion derived from a benchmark model that is much too abstract to be confronted with data.

On the practical side, mathematical portfolio and risk management models have been the academic backbone of the tremendous increase of trading volume and diversification of instruments in financial markets. Typically, new derivative products achieve market penetration only if a certain industry standard has been established for pricing and risk management of these products. Mostly, pricing principles are derived from a set of assumptions on an 'appropriate' process for the underlying asset, (i.e., the primary assets on which options or forwards are written) together with an equilibrium criterion such as arbitrage-free prices. With that mostly comes advice for hedging the inherent risk of a derivative position by balancing it with other assets that neutralize the risk exposure. The most prominent example is certainly the development of a theory of option pricing by Black and Scholes that eventually (in the eighties) could even be implemented on pocket calculators. Simultaneously with Black-Scholes option pricing, the same principles led to the widespread introduction of new strategies under the heading of portfolio insurance and dynamic hedging that just tried to implement a theoretically risk-free portfolio composed of both assets and options and keep it risk-free by frequent rebalancing after changes of its input data (e.g., asset prices). For structured products for credit risk, the basic paradigm of derivative pricing—perfect replication—is not applicable so that one has to rely on a kind of rough-and-ready evaluation of these contracts on the base of historical data. Unfortunately, historical data were hardly available in most cases which meant that one had to rely on simulations with relatively arbitrary assumptions on correlations between risks and default probabilities. This makes the theoretical foundations of all these products highly questionable—the equivalent to building a building of cement of which you weren't sure of the components. The dramatic recent rise of the markets for structured products (most prominently collateralized debt obligations and credit default swaps—CDOs and CDSs) was made possible by development of such simulation-based pricing tools and the adoption of an industry-standard for these under the lead of rating agencies. Barry Eichengreen (2008) rightly points out that the "development of mathematical methods designed to quantify and hedge risk encouraged commercial banks, investment banks and hedge funds to use more leverage" as if the very use of the mathematical methods diminished the underlying risk. He also notes that the models were estimated on data from periods of low volatility and thus could not deal with the arrival of major changes. Worse, it is our contention that such major changes are endemic to the economy and cannot be simply ignored.

What are the flaws of the new unregulated financial markets which have emerged? As we have already pointed out in the introduction, the possibility of systemic risk has not been entirely ignored but it has been defined as lying outside the responsibility of market participants. In this way, *moral hazard* concerning systemic risk has been a necessary and built-in attribute of the system. The neglect of the systemic part in the 'normal mode of operation,' of course, implies that external effects are not taken properly into account and that in tendency, market participants will ignore the influence of their own behavior on the stability of the system. The interesting aspect is more that this was a known and accepted element of operations. Note that the blame should not only fall on market participants, but also on the deliberate ignoring of the systemic risk factors or the failure to at least point them out to the public amounts to a sort of *academic 'moral hazard.'*

There are some additional aspects as well: asset-pricing and risk management tools are developed from an individualistic perspective, taking as given (*ceteris paribus*) the behavior of all other market participants. However, popular models might be used by a large number or even the majority of market participants. Similarly, a market participant (e.g., the notorious Long-Term Capital Management) might become so dominant in certain markets that the *ceteris paribus* assumption becomes unrealistic. The simultaneous pursuit of identical micro strategies leads to synchronous behavior and mechanic contagion. This simultaneous application might generate an unexpected macro outcome that actually jeopardizes the success of the underlying micro strategies. A perfect illustration is the U.S. stock market crash of October 1987. Triggered by a small decrease of prices, automated hedging strategies produced an avalanche of sell orders that out of the blue led to a fall in U.S. stock indices of about 20 percent within one day. With the massive sales to rebalance their portfolios (along the lines of Black and Scholes), the relevant actors could not realize their attempted incremental adjustments, but rather suffered major losses from the ensuing large macro effect.

A somewhat different aspect is the danger of a *control illusion:* The mathematical rigor and numerical precision of risk management and asset pricing tools has a tendency to conceal the weaknesses of models and assumptions to those who have not developed them and do not know the potential weakness of the assumptions and it is indeed this that Eichengreen emphasizes. Naturally, models are only approximations to the real world dynamics and partially built upon quite heroic assumptions (most notoriously: Normality of asset price changes which can be rejected at a confidence level of 99.9999 . . .. Anyone who has attended a course in first-year statistics can do this within minutes). Of course, considerable progress has been made by moving to more refined models with, e.g., 'fat-tailed' Levy processes as their driving factors. However, while such models better capture the intrinsic volatility of markets, their improved performance, taken at face value, might again contribute to enhancing the control illusion of the naïve user.

The increased sophistication of extant models does, however, not overcome the robustness problem and should not absolve the modelers from explaining their limitations to the users in the financial industry. As in nuclear physics, the tools provided by financial engineering can be put to very different uses so that what is designed as an instrument to hedge risk can become a weapon of 'financial mass destruction' (in the words of Warren Buffet) if used for increased leverage. In fact, it appears that derivative positions have been built up often in speculative ways to profit from high returns as long as the downside risk does not materialize. Researchers who develop such models can claim they are neutral academics—developing tools that people are free to use or not. We do not find that view credible. Researchers have an ethical responsibility to point out to the public when the tool that they developed is misused. It is the responsibility of the researcher to make clear from the outset the limitations and underlying assumptions of his models and warn of the dangers of their mechanic application.

What follows from our diagnosis? Market participants and regulators have to become more sensitive towards the potential weaknesses of risk management models. Since we do not know the 'true' model, robustness should be a key concern. Model uncertainty should be taken into account by applying more than a single model. For example, one could rely on probabilistic projections that cover a whole range of specific models (cf., Föllmer, 2008). The theory of robust control provides a toolbox of techniques that could be applied for this purpose, and it is an approach that should be considered.

3. Unrealistic Model Assumptions and Unrealistic Outcomes

Many economic models are built upon the twin assumptions of 'rational expectations' and a representative agent. "Rational expectations" instructs an economist to specify individuals' expectations to be fully consistent with the structure of his own model. This concept can be thought of as merely a way to close a model. A behavioral interpretation of rational expectations would imply that individuals and the economist have a complete understanding of the economic mechanisms governing the world. In this sense, rational expectations models do not attempt to formalize individuals' actual expectations: specifications are not based on empirical observation of the expectations formation process of human actors. Thus, even when applied economics research or psychology provide insights about how individuals actually form expectations, they cannot be used within RE models. Leaving no place for imperfect knowledge and adaptive adjustments, rational expectations models are typically found to have dynamics that are not smooth enough to fit economic data well.[5]

Technically, rational expectations models are often framed as dynamic programming problems in macroeconomics. But, dynamic programming models have serious limitations. Specifically, to make them analytically tractable, not more than one dynamically maximizing agent can be considered, and consistent expectations have to be imposed. Therefore, dynamic programming models are hardly imaginable without the assumptions of a representative agent and rational expectations. This has generated a vicious cycle by which the technical tools developed on the base of the chosen assumptions prevent economists from moving beyond these restricted settings and exploring more realistic scenarios. Note that such settings also presume that there is a single model of the economy, which is odd given that even economists are divided in their views about the correct model of the economy. While other currents of research do exist, economic policy advice, particularly in financial economics, has far too often been based (consciously or not) on a set of axioms and hypotheses derived ultimately from a highly limited dynamic control model, using the Robinson approach with 'rational' expectations.

The major problem is that despite its many refinements, this is not at all an approach based on, and confirmed by, empirical research.[6] In fact, it stands in stark contrast to a broad set of regularities in human behavior discovered both in psychology and what is called behavioral and experimental economics. The corner stones of many models in finance and macroeconomics are rather maintained *despite* all the contradictory evidence discovered in empirical research. Much of this literature shows that human subjects act in a way that bears no resemblance to the rational expectations paradigm and also have problems discovering 'rational expectations equilibria' in repeated experimental settings. Rather, agents display various forms of 'bounded rationality' using heuristic decision rules and displaying inertia in their reaction to new information. They have also been shown in financial markets to be strongly influenced by emotional and hormonal reactions (see Lo et al., 2005, and Coates and Herbert, 2008). Economic modeling has to take such findings seriously.

What we are arguing is that as a modeling requirement, internal consistency must be complemented with *external consistency:* Economic modeling has to be compatible with insights from other branches of science on human behavior. It is highly problematic to insist on a specific view of humans in economic settings that is irreconcilable with evidence.

The 'representative agent' aspect of many current models in macroeconomics (including macro finance) means that modelers subscribe to the most extreme form of *conceptual reductionism* (Lux and Westerhoff, 2009): by assumption, all concepts applicable to the macro sphere (i.e., the economy or its financial system) are fully reduced to concepts and knowledge for the lower-level domain of the individual agent. It is worth emphasizing that this is quite different from the standard reductionist concept that has become widely accepted in natural sciences. The more standard notion of reductionism amounts to an approach to understanding the nature of complex phenomena by reducing them to the interactions of their parts, allowing for new, emergent phenomena at the higher hierarchical level (the concept of 'more is different,' cf. Anderson, 1972).

Quite to the contrary, the representative agent approach in economics has simply set the macro sphere equal to the micro sphere in all respects. One could, indeed, say that this concept negates the existence of a macro sphere and the necessity of investigating macroeconomic phenomena in that it views the entire economy as an

---

[5] For a critique of rational expectations models on epistemological grounds, see Frydman and Goldberg (2007, 2008) and references therein.

[6] The historical emergence of the representative agent paradigm is a mystery. Ironically, it appeared over the 70s after a period of intense discussions on the problem of aggregation in economics (that basically yielded negative results such as the impossibility to demonstrated 'nice' properties of aggregate demand or supply functions without imposing extreme assumptions on individual behavior). The representative agent appeared without methodological discussion. In the words of Deirdre McCloskey: "It became a rule in the conversation of some economists because Tom and Bob said so." (personal communication). Today, this convention has become so strong that many young economists wouldn't know of an alternative way to approach macroeconomic issues.

organism governed by a universal will.[7] Any notion of "systemic risk" or "coordination failure" is necessarily absent from, and alien to, such a methodology.

For natural scientists, the distinction between micro-level phenomena and those originating on a macro, system-wide scale from the interaction of microscopic units is well-known. In a dispersed system, the current crisis would be seen as an involuntary emergent phenomenon of the microeconomic activity. The conceptual reductionist paradigm, however, blocks from the outset any understanding of the interplay between the micro and macro levels. The differences between the overall system and its parts remain simply incomprehensible from the viewpoint of this approach.

In order to develop models that allow us to deduce macro events from microeconomic regularities, economists have to rethink the concept of micro foundations of macroeconomic models. Since economic activity is of an essentially interactive nature, economists' micro foundations should allow for the interactions of economic agents. Since interaction depends on differences in information, motives, knowledge and capabilities, this implies heterogeneity of agents. For instance, only a sufficiently rich structure of connections between firms, households and a dispersed banking sector will allow us to get a grasp on "systemic risk," domino effects in the financial sector, and their repercussions on consumption and investment. The dominance of the extreme form of conceptual reductionism of the representative agent has prevented economists from even attempting to model such all important phenomena. It is the flawed methodology that is the ultimate reason for the lack of applicability of the standard macro framework to current events.

Since most of what is relevant and interesting in economic life has to do with the interaction and coordination of ensembles of heterogeneous economic actors, the methodological preference for single actor models has extremely handicapped macroeconomic analysis and prevented it from approaching vital topics. For example, the recent surge of research in network theory has received relatively scarce attention in economics. Given the established curriculum of economic programs, an economist would find it much more tractable to study adultery as a dynamic optimization problem of a representative husband, and derive the optimal time path of marital infidelity (and publish his exercise) rather than investigating financial flows in the banking sector within a network theory framework. This is more than unfortunate in view of the network aspects of interbank linkages that have become apparent during the current crisis.

In our view, a change of focus is necessary that takes seriously the regularities in expectation formation revealed by behavioral research and, in fact, gives back an independent role to expectations in economic models. It would also be fallacious to only replace the current paradigm by a representative 'non-rational' actor (as it is sometimes done in recent literature). Rather, an *appropriate micro foundation* is needed that considers interaction at a certain level of complexity and extracts macro regularities (where they exist) from microeconomic models with dispersed activity.

Once one acknowledges the importance of empirically based behavioral micro foundations and the heterogeneity of actors, a rich spectrum of new models becomes available. The dynamic co-evolution of expectations and economic activity would allow one to study out-of-equilibrium dynamics and adaptive adjustments. Such dynamics could reveal the possibility of multiplicity and evolution of equilibria (e.g., with high or low employment) depending on agents' expectations or even on the propagation of positive or negative 'moods' among the population. This would capture the psychological component of the business cycle which—though prominent in many policy-oriented discussions—is never taken into consideration in contemporary macroeconomic models.

It is worth noting that understanding the formation of such low-level equilibria might be much more valuable in coping with major 'efficiency losses' by mass unemployment than the pursuit of small 'inefficiencies' due to societal decisions on norms such as shop opening times. Models with interacting heterogeneous agents would also open the door to the incorporation of results from other fields: network theory has been mentioned as an obvious example (for models of networks in finance see Allen and Babus, 2008). 'Self-organized criticality' theory is another area that seems to have some appeal for explaining boom-and-bust cycles (cf. Scheinkman and Woodford, 1992). Incorporating heterogeneous agents with imperfect knowledge would also provide a better framework for the analysis of the use and dissemination of information through market operations and more direct links of communication. If one accepts that the dispersed economic activity of many economic agents could be described by statistical laws, one might even take stock of methods from statis-

[7] The conceptual reductionist approach of the representative agent is also remarkably different from the narrative of the 'invisible hand' which has more the flavor of 'more is different'.

tical physics to model dynamic economic systems (cf. Aoki and Yoshikawa, 2007; Lux, 2009, for examples).

4. Robustness and Data-Driven Empirical Research

Currently popular models (in particular: dynamic general equilibrium models) do not only have weak micro foundations, their empirical performance is far from satisfactory (Juselius and Franchi, 2007). Indeed, the relevant strand of empirical economics has more and more avoided testing their models and has instead turned to calibration without explicit consideration of goodness-of-fit.[8] This calibration is done using "deep economic parameters" such as parameters of utility functions derived from microeconomic studies. However, at the risk of being repetitive, it should be emphasized that micro parameters cannot be used directly in the parameterization of a macroeconomic model. The aggregation literature is full of examples that point out the possible "fallacies of composition." The "deep parameters" only seem sensible if one considers the economy as a universal organism without interactions. If interactions are important (as it seems to us they are), the restriction of the parameter space imposed by using micro parameters is inappropriate.

Another concern is nonstationarity and structural shifts in the underlying data. Macro models, unlike many financial models, are often calibrated over long time horizons which include major changes in the regulatory framework of the countries investigated. Cases in question are the movements between different exchange rate regimes and the deregulation of financial markets over the 70s and 80s. In summary, it seems to us that much of contemporary empirical work in macroeconomics and finance is driven by the pre-analytic belief in the validity of a certain model. Rather than (mis)using statistics as a means to illustrate these beliefs, the goal should be to put theoretical models to scientific test (as the naïve believer in positive science would expect).

The current approach of using pre-selected models is problematic and we recommend a more data-driven methodology. Instead of starting out with an ad-hoc specification and questionable *ceteris paribus* assumptions, the key features of the data should be explored via data-analytical tools and specification tests. David Hendry provides a well-established empirical methodology for such exploratory data analysis (Hendry, 1995, 2009) as well as a general theory for model selection (Hendry and Krolzig, 2005); clustering techniques such as projection pursuit (e.g., Friedman, 1987) might provide alternatives for the identification of key relationships and the reduction of complexity on the way from empirical measurement to theoretical models. Co-integrated VAR models could provide an avenue towards identification of robust structures within a set of data (Juselius, 2006), for example, the forces that move equilibria (*pushing forces,* which give rise to stochastic trends) and forces that correct deviations from equilibrium (*pulling forces,* which give rise to long-run relations). Interpreted in this way, the 'general-to-specific' empirical approach has a good chance of nesting a multi-variate, path-dependent data-generating process and relevant dynamic macroeconomic theories. Unlike approaches in which data are silenced by prior restrictions, the Co-integrated VAR model gives the data a rich context in which to speak freely (Hoover et al., 2008).

A chain of specification tests and estimated statistical models for simultaneous systems would provide a benchmark for the subsequent development of tests of models based on economic behavior: significant and robust relations within a simultaneous system would provide empirical regularities that one would attempt to explain, while the quality of fit of the statistical benchmark would offer a confidence band for more ambitious models. Models that do not reproduce (even) approximately the quality of the fit of statistical models would have to be rejected (the majority of currently popular macroeconomic and macro finance models would not pass this test). Again, we see here an aspect of *ethical responsibility* of researchers: Economic policy models should be theoretically and empirically sound. Economists should avoid giving policy recommendations on the base of models with a weak empirical grounding and should, to the extent possible, make clear to the public how strong the support of the data is for their models and the conclusions drawn from them.

5. A Research Agenda to Cope with Financial Fragility

The notion of financial fragility implies that a given system might be more or less susceptible to produce crises. It seems clear that financial innovations have made

[8] It is pretty obvious how the currently popular class of dynamic general equilibrium models would have to 'cope' with the current financial crisis. It will be covered either by a dummy or it will have to be interpreted as a very large negative stochastic shock to the economy, i.e., as an event equivalent to a large asteroid strike.

the system more fragile. Apparently, the existing linkages within the worldwide, highly connected financial markets have generated the spill-overs from the U.S. subprime problem to other layers of the financial system. Many financial innovations had the effect of creating links between formerly unconnected players. All in all, the degree of connectivity of the system has probably increased enormously over the last decades. As is well known from network theory in natural sciences, a more highly connected system might be more efficient in coping with certain tasks (maybe distributing risk components), but will often also be more vulnerable to shocks and—systemic failure! The systematic analysis of network vulnerability has been undertaken in the computer science and operations research literature (see e.g., Criado et al., 2005). Such aspects have, however, been largely absent from discussions in financial economics. The introduction of new derivatives was rather seen through the lens of general equilibrium models: more contingent claims help to achieve higher efficiency. Unfortunately, the claimed efficiency gains through derivatives are merely a theoretical implication of a highly stylized model and, therefore, have to count as a *hypothesis*. Since there is hardly any supporting empirical evidence (or even analysis of this question), the claimed real-world efficiency gains from derivatives are not justified by true science. While the economic argument in favor of ever new derivatives is more one of persuasion rather than evidence, important negative effects have been neglected. The idea that the system was made less risky with the development of more derivatives led to financial actors taking positions with extreme degrees of leverage and the danger of this has not been emphasized enough.

As we have mentioned, one neglected area is the degree of connectivity and its interplay with the stability of the system (see Boesch et al., 2006). We believe that it will be necessary for supervisory authorities to develop a perspective on the network aspects of the financial system, collect appropriate data, define measures of connectivity and perform macro stress testing at the system level. In this way, new measures of financial fragility would be obtained. This would also require a new area of accompanying academic research that looks at agent-based models of the financial system, performs scenario analyses and develops aggregate risk measures. Network theory and the theory of self-organized criticality of highly connected systems would be appropriate starting points.

The danger of systemic risk means that regulation has to be extended from individualistic (regulation of single institutions which of course, is still crucial) to system wide regulation. In the sort of system which is prone to systemic crisis, regulation also has to have a systemic perspective. Academic researchers and supervisory authorities thus have to look into connections within the financial sector and to investigate the repercussions of problems within one institute on other parts of the system (even across national borders). Certainly, before deciding about the bail-out of a large bank, this implies an understanding of the network. One should know whether its bankruptcy would lead to widespread domino effects or whether contagion would be limited. It seems to us that what regulators provide currently is far from a reliable assessment of such after effects.

Such analysis has to be supported by more traditional approaches: Leverage of financial institutions rose to unprecedented levels prior to the crisis, partly by evading Basle II regulations through special investment vehicles (SIVs). The hedge fund market is still entirely unregulated. The interplay between leverage, connectivity and system risk needs to be investigated at the aggregate level. It is highly likely, that extreme leverage levels of interconnected institutions will be found to impose unacceptable social risk on the public. Prudent capital requirements would be necessary and would require a solid scientific investigation of the above aspects rather than a pre-analytic *laissez-faire* attitude.

We also have to re-investigate the informational role of financial prices and financial contracts. While trading in stock markets is usually interpreted as at least in part transmitting information, this information transmission seems to have broken down in the case of structured financial products. It seems that securitization has rather led to a loss of information by anonymous intermediation (often multiple) between borrowers and lenders. In this way, the informational component has been outsourced to rating agencies and typically, the buyer of CDO tranches would not have spent any effort himself on information acquisition concerning his far away counterparts. However, this centralized information processing instead of the dispersed one in traditional credit relationships might lead to a severe loss of information. As it turned out, standard loan default models failed dramatically in recent years (Rajan et al., 2008). It should also be noted that the price system itself can exacerbate the difficulties in the financial market (see Hellwig, 2008). One of the reasons for the sharp fall in the asset valuations of major banks was not only the loss on the assets on which their derivatives were based, but also the general reaction of the markets to these assets. As markets became aware of the risk involved, all such assets were written down and it was in this way that a small sector of the market "contaminated" the rest. Large parts of the asset holdings of major banks abruptly lost much of their value. Thus the price system itself can be destabilizing as expectations change.

On the macroeconomic level, it would be desirable to develop early warning schemes that indicate the formation of bubbles. Combinations of indicators with time series techniques could be helpful in detecting deviations of financial or other prices from their long-run averages. Indication of structural change (particularly towards non-stationary trajectories) would be a signature of changes of the behavior of market participants of a bubble-type nature.

6. Conclusions

The current crisis might be characterized as an example of the final stage of a well-known boom-and-bust pattern that has been repeated so many times in the course of economic history. There are, nevertheless, some aspects that make this crisis different from its predecessors: First, the preceding boom had its origin—at least to a large part—in the development of new financial products that opened up new investment possibilities (while most previous crises were the consequence of over-investment in new physical investment possibilities). Second, the global dimension of the current crisis is due to the increased connectivity of our already highly interconnected financial system. Both aspects have been largely ignored by academic economics. Research on the origin of instabilities, over-investment and subsequent slumps has been considered as an exotic side track from the academic research agenda (and the curriculum of most economics programs).This, of course, was because it was incompatible with the premise of the rational representative agent. This paradigm also made economics blind with respect to the role of interactions and connections between actors (such as the changes in the network structure of the financial industry brought about by deregulation and introduction of new structured products). Indeed, much of the work on contagion and herding behavior (see Banerjee, 1992, and Chamley, 2002) which is closely connected to the network structure of the economy has not been incorporated into macroeconomic analysis.

We believe that economics has been trapped in a sub-optimal equilibrium in which much of its research efforts are not directed towards the most prevalent needs of society. Paradoxically self-reinforcing feedback effects within the profession may have led to the dominance of a paradigm that has no solid methodological basis and whose empirical performance is, to say the least, modest. Defining away the most prevalent economic problems of modern economies and failing to communicate the limitations and assumptions of its popular models, the economics profession bears some responsibility for the current crisis. It has failed in its duty to society to provide as much insight as possible into the workings of the economy and in providing warnings about the tools it created. It has also been reluctant to emphasize the limitations of its analysis. We believe that the failure to even envisage the current problems of the worldwide financial system and the inability of standard macro and finance models to provide any insight into ongoing events make a strong case for a major reorientation in these areas and a reconsideration of their basic premises.

References

Allen, F. and A. Babus, 2008, *Networks in Finance.* Wharton Financial Institutions Center Working Paper No. 08-07. Available at SSRN: *http://ssrn.com/abstract=1094883*

Anderson, P.W., 1972, More is different, *Science* 177, 393–396.

Aoki, M. and H. Yoshikawa, 2007, *Reconstructing Macroeconomics: A Perspective from Statistical Physics and Combinatorial Stochastic Processes.* Cambridge University Press: Cambridge and New York.

Bagehot, W., 1873, *Lombard Street: A Description of the Money Market.* Henry S. King and Co.: London.

Banerjee, A., 1992, A simple model of herd behaviour, *Quarterly Journal of Economics,* 108, 797–817.

Boesch, F.T., F. Harary, and J.A. Kabell, 2006, Graphs as models of communication network vulnerability: Connectivity and persistence, *Networks,* 11, 57–63.

Brigandt, I. and A. Love, *'Reductionism in Biology'* in the *Stanford Encyclopedia of Philosophy.* Available at *http://plato.stanford.edu/entries/reduction-biology/*

Campos, J., N.R. Ericsson and D.F. Hendry, 2005, Editors' Introduction to *General to Specific Modelling,* 1–81, Edward Elgar: London.

Chamley, C.P., 2002, *Rational Herds: Economic Models of Social Learning.* Cambridge University Press: Cambridge.

Coates J.M. and J. Herbert, 2008, Endogenous steroids and financial risk taking on a London trading floor, *Proceedings of the National Academy of Sciences,* 6167–6172.

Cogley, T. and T. Sargent, 2008, The market price of risk and the equity premium: A legacy of the Great Depression?, *Journal of Monetary Economics,* 55, 454–476.

Criado, R., J. Flores, B. Hernández-Bermejo, J. Pello, and M. Romance, 2005, Effective measurement of network vulnerability under random and intentional attacks, *Journal of Mathematical Modelling and Algorithms,* 4, 307–316.

Eichengreen, B., 2008, *Origins and Responses to the Crisis,* unpublished manuscript, University of California, Berkeley.

Föllmer, H., 2008, Financial uncertainty, risk measures and robust preferences, in: Yor, M., ed., *Aspects of Mathematical Finance,* Springer: Berlin.

Friedman, J., 1987, Exploratory projection pursuit, *Journal of the American Statistical Association,* 82, 249–266.

Frydman, R. and M.D. Goldberg (2007), *Imperfect Knowledge Economics: Exchange Rates and Risk,* Princeton, NJ: Princeton University Press.

Frydman, R. and M.D. Goldberg (2008), Macroeconomic Theory for a World of Imperfect Knowledge, *Capitalism and Society,* 3, Article 1.

Hellwig, M.F., 2008, *Systemic Risk in the Financial Sector: An Analysis of the Subprime-Mortgage Financial Crisis,* MPI Collective Goods Preprint, No. 2008/43.

Hendry, D., 2009, The Methodology of Empirical Econometric Modelling: Applied Econometrics Through the Looking-Glass, forthcoming in *The Handbook of Empirical Econometrics,* Palgrave.

Hendry, D.F., 1995. *Dynamic Econometrics.* Oxford University Press: Oxford.

Hendry, D.F. and H.-M. Krolzig, 2005, The Properties of Automatic Gets Modelling, *Economic Journal,* 115, C32–C61.

Hoover, K., S. Johansen, and K. Juselius, 2008, Allowing the data to speak freely: The macroeconometrics of the cointegrated vector autoregression. *American Economic Review* 98, 251–55.

Juselius, K., 2006, *The cointegrated VAR model: Econometric Methodology and Empirical Applications.* Oxford University Press: Oxford.

Juselius, K. and M. Franchi, 2007, Taking a DSGE Model to the Data Meaningfully, *Economics—The Open-Access, Open-Assessment E-Journal,* 4.

Kindleberger, C.P., 1989, *Manias, Panics, and Crashes: A History of Financial Crises.* MacMillan: London.

Krahnen, J.-P. and C. Wilde, 2006, *Risk Transfer with CDOs and Systemic Risk in Banking.* Center for Financial Studies, WP 2006–04. Frankfurt.

Krahnen, J.-P., 2005, Der Handel von Kreditrisiken: Eine neue Dimension des Kapitalmarktes, *Perspektiven der Wirtschaftspolitik* 6, 499–519.

Leijonhufvud, A., 2000, *Macroeconomic Instability and Coordination: Selected Essays,* Edward Elgar: Cheltenham.

Lo, A., D.V. Repin and B.N. Steenbarger, Fear and Greed in Financial Markets: A Clinical Study of Day-Traders, *American Economic Review* 95, 352–359.

Lux, T. and F. Westerhoff, 2009, Economics crisis, *Nature Physics* 5, 2–3.

Lux, T., 2009, Stochastic Behavioral Asset Pricing Models and the Stylized Facts, chapter 3 in T. Hens and K. Schenk-Hoppé, eds., *Handbook of Financial Markets: Dynamics and Evolution.* North-Holland: Amsterdam, 161–215.

Minsky, H.P., 1986, *Stabilizing an Unstable Economy.* Yale University Press: New Haven.

Rajan, U., A. Seru and V. Vig, 2008, *The Failure of Models that Predict Failure: Distance, Incentives and Defaults.* Chicago GSB Research Paper No. 08–19.

Reinhart, C. and K. Rogoff, 2008, *This Time is Different: A Panoramic View of Eight Centuries of Financial Crises.* Manuscript, Harvard University and NBER.

Scheinkman, J. and M. Woodford, 1994, Self-Organized Criticality and Economic Fluctuations, *American Economic Review* 84 (Papers and Proceedings), 417–421.

BIOGRAPHY FOR DAVID COLANDER

David Colander has been the Christian A. Johnson Distinguished Professor of Economics at Middlebury College, Middlebury, Vermont since 1982. He has authored, co-authored, or edited over 40 books (including a principles and intermediate macro text) and 150 articles on a wide range of topics. His books have been, or are being, translated into a number of different languages, including Chinese, Bulgarian, Polish, Italian, and Spanish.

He received his Ph.D. from Columbia University and has taught at Columbia University, Vassar College, the University of Miami as well as Middlebury. He has also been a consultant to Time-Life Films, a consultant to Congress, a Brookings Policy Fellow, and a Visiting Scholar at Nuffield College, Oxford. In 2001–2002 he was the Kelly Professor of Distinguished Teaching at Princeton University.

He is a former President of both the Eastern Economic Association and History of Economic Thought Society and is, or has been, on the editorial boards of the *Journal of the History of Economic Thought, Journal of Economic Methodology, Eastern Economic Journal,* and *The Journal of Socioeconomics,* and *Journal of Economic Perspectives.* He is a member of the AEA Committee on Economic Education.

DISCUSSION

Chairman MILLER. I want to thank all of you.

APPROPRIATE USES OF FINANCIAL MODELS

Let me begin this panel with a question of the earlier panel. Some of those responsible, involved in developing economic modeling now say that the fundamental problem was that the model was wrong, there is more data. I don't think anyone thinks that models should be prohibited or people should be prohibited from acting on their models for their investment decisions or whatever. The extent to which it can be used, it should be used for regulation, safety and soundness regulation. Do any of you—what do each of you think about whether the models may be improved and will become reliable, sufficiently reliable to base capital requirements on—or do you think that it is so inherently unpredictable that economic forecasts will never become like predicting the movements of the planets, that it may be useful for seeing if a financial institution is headed towards trouble, but not to say it is got nothing to worry about? Any of you. Dr. Berman.

Dr. BERMAN. Thank you. I think models definitely have a very significant role, not just in finance but in society in general. The question is, what aspect of a model are you looking to use. Certain models are designed to predict the future. That is always very difficult to do. We can predict where the planets are going to go but it is very difficult to predict where the stock market is going to go today. That is a very small portion of what financial modeling is about, predicting the future. Unfortunately, that is what folks glean onto when they start thinking about capital requirements. A much larger portion of what modeling is about is understanding: if something happens to X, what happens to Y? You don't have to predict the future in order to do that, you just need to know the relationships between two different things.

Let's take an excellent example. The world's largest insurance company entered into massive amounts of credit default swaps that ultimately were responsible for their demise. The bet that they took might have turned out to be the best bet that they ever could have made. We don't know because those CDSs are probably still out there to a certain extent. But they failed to account for the fact that, what would happen if there was a small dip in the value of these, and my counterparty asks for collateral? That's not a matter of predicting the future, that's just understanding this is the way that market works. When the value falls, your counterparty asks for collateral. They missed that aspect of the model. That had nothing to do with predicting the future but just in understanding how that worked, and that ultimately led to the demise. And you see that pervasive through many, many different types of models throughout the system.

Chairman MILLER. It does remind me of Yoga Berra's wisdom that predictions are difficult, especially about the future. Mr. Rickards?

Mr. RICKARDS. Mr. Chairman, I think it is interesting that a number of Members and the witnesses today have referred to planetary motion as an example of models that work, but I will remind everyone that from 200 B.C. to 1500 A.D., the model of the universe was a geocentric model in which the sun revolved around the Earth. And it was obvious because you woke up in the morning and the sun came up over here and went down over there, and that was not just a religious belief, that was actually a scientific belief, and many brilliant mathematicians worked for centuries to write the equations. They weren't automated, of course, but they wrote those models, and when people began observing data from improved telescopes that didn't conform to that model, they said well, we just need to tweak the model a little bit. Instead of these cycles, they created epicycles. They were little twirls within the big twirls, and they kept going down that path. The model was completely wrong. Actually, the model was right, the paradigm was wrong. The understanding of how the world worked was wrong. The sun did not revolve around the Earth; the Earth revolved around the sun.

That is my view of today. You can tweak it, you can improve it, you can separate the so-called fat tail and zero in on that tail, and there is a complex method called GARCH, Generalized Autoregressive Condition Heteroskedasticity and variations on that. They are all wrong because the paradigm is wrong, because the risk is not normally distributed in the first place. So I think these are fatally flawed.

If a hedge fund that is non-systemically important wants to use this model, that is fine. They can use voodoo, as far as I am concerned, but if you are talking about a bank or regulated financial institution, they should be prohibited because they don't work.

Chairman MILLER. Mr. Whalen.

Mr. WHALEN. I agree with them, and also I think Dr. Berman made this point. When you are talking about safety and soundness, you don't want to look at a tactical short-term loss possibility, you want to look at the worst case, and we see that now with the banking industry. By next year, I think we are going to be looking at a double- or triple-digit deficit in the insurance fund, and banks are going to have to pay that back. No one anticipated that magnitude of loss. So what you have is, on the one hand, a marketplace which is very short term. They are working on today's earnings, next quarter's earnings, what have you, and yet over time, since Glass-Steagall, we have slowly eroded the limits on risk taking. So the models—whether they worked or not is kind of irrelevant. We slowly allowed banks to take more and more risk. So I think what we have to do first is say, what risk do we want the utility side of this industry, the depository, the lending, the cash distribution part of banks, to take, and what part do we want to force, for example, into the hedge fund community, which is a perfect place for risk taking.

You know, we can't come up with the answer to your question, Mr. Chairman, as to safety and soundness and capital adequacy, unless we quantify the risks that the institutions take. I will give you an example. Citigroup in 1991 peaked at about three and a half percent charge-offs versus total loans. I think they are going to get up to about six this time. Now, can you imagine the public and market reaction when the large money centers get up to something like two, maybe two and a half times their 1990 loss rate? But that is how severe of a skew we are seeing. In the Depression, we got up to five percent losses on total loans, so we are closing in on the 1930s. I don't think it will be quite that bad, but we will see how long we stay there. That is the other question, how long will we see those losses? Will it be two quarters or four? This is the kind of question you need to answer very precisely but the only way you can answer your question about capital and safety and soundness is if you first quantify the risk taking, because otherwise I don't think you can get an answer.

And by the way, we wrote about this last week. I don't think you can ask the markets to give more capital to banks. I think the G-20 and Secretary Geithner are wrong. You have to reduce the risk taking, because I don't think the markets would let J.P. Morgan have 20 percent capital assets because the returns will be too low. It would be low single digits at best and on a risk-adjusted basis I think they would be negative. This, by the way, is the context you ought to bear in mind. Most of the large banks on a risk-adjusted basis really aren't that profitable. It is only the super-normal returns that they get from OTC derivatives, investment banking, proprietary trading that helped the whole enterprise look profitable. If you look at the retail side, the cash securities trading, it is barely profitable, really, and that is why you have seen the changes in the industry that you have.

Chairman MILLER. Dr. Colander.

Dr. COLANDER. In answer to your question, in social science you will never get the amount of exactness that you will get in natural sciences, mainly because the agent in social science is not an atom which sort of follows a set of rules, you know, it is a human being, it is an agent who will try to do everything he can to screw you every time you are trying to control him. So the thought that you are going to be able to design any model is pretty much impossible. That being said, I think models can be used and have to be used. We all use models. How can you not sort of picture what is going on? The question is, what type of models, and how many different models do you have in your mind, and how quickly can you jump from one model to another and recognize we have really moved there, and that is the issue that I think people are talking about.

PROPOSALS FOR AVOIDING RECURRENCES OF FINANCIAL PROBLEMS

Chairman MILLER. Interesting set of answers. Mr. Rickards did mention at least three proposals for avoiding a catastrophe like what we have had. Do the rest of you have specific proposals as well of how we avoid this again? I think the financial industry is already treating what happened last September, October—we are still in it—as a hiccup, something that was a fluke, will not happen again, we don't have to change conduct very much. I assume all of you don't agree with that, but what is it that we should do?

Dr. BERMAN. I think there are two courses of action. I think most of the discussion on regulatory capital is trying to solve a symptom as opposed to the cure itself. A good portion of the funds come from investors who are feeding the big bonuses, let's say, at large banks, so while there is lots of talk about the restriction on bonuses and whether we should hold people legally liable for clawbacks, et cetera, the fact is that the fuel is there. That fuel causes crisis. The fuel is done generally by greed, but mostly uninformed greed. Probably the number one thing that regulators can enforce is better transparency and better disclosure on finance itself. If more people understood what they were actually buying, less people would buy these things. Wall Street is a marketing arm as are all commercial companies. Their practices came from the desires of people to invest in those products, invest in those services, and invest in the companies themselves. If we don't like those practices, then we should make it clear what those practices are and let investors choose whether or not they want to engage in those. That would dampen further—well, certainly it would help reverse this crisis a bit, and it would certainly dampen the ability for the market to even create these very, very large bubbles in the first place.

Mr. WHALEN. One simple thing that I would add to Dr. Berman's comment, and speaking as an investment banker, make the lawyers your friend. What you want to do is, instead of allowing banks to bring these structured assets and derivatives in an unregistered forum, you force them to register with the SEC, and what that does is two things. First off, the lawyers of the deals will not allow more than a certain degree of complexity, because once that deal is registered, it can be purchased by all investors, and so they will force simplicity onto their banks. Because otherwise they will get sued, and the trial lawyers will enforce this, believe me. Remember, most of the toxic waste, the complex structured assets, were all done at private placements. You can't even get a copy of the prospectus.

The second thing I would tell you is that, you know, in terms of overall market structure, we've got to decide whether or not, going back to my earlier comment, we are going to allow people to contrive of any security for any investor that doesn't have some rational basis, some objective basis in terms of valuation, because that is really the key problem that we have all faced over the last couple years, is valuation. When the investors realized that they couldn't get a bid from the dealer that sold them the CDO and they couldn't value it by going to anybody in the cottage community, they just withdrew from the market and we had a liquidity problem. If you force these deals to be registered, guess what? Every month when the servicer data for the underlying becomes available, they will have to drop an 8K and then that data will be available to the community for free. We won't have to spend hundreds of thousands of dollars a year to buy servicer data so that we can manually construct models to try and understand how a very complicated mortgage security, for example, is going to perform. You will open up the transparency so that the cottage industry that currently supports valuation for simple structures, which are very easy to value—credit card deals, auto deals—there is really no problem with these and they are coming back, by the way. You are starting to see volume come back to that market. It is about disclosure. I think Dr. Berman says it very well.

ABUSE OF THE VAR

Chairman MILLER. Dr. Colander? You don't have to speak on every topic if you don't want to.

Dr. Berman, everyone agrees that the VaR can be abused, has been abused, was certainly used foolishly in lowering capital requirements for investment banks. Without revealing proprietary information, can you give us some of the ways that you have seen firms abuse the VaR, or try to abuse the VaR apart from regulatory matters?

Dr. BERMAN. Sure. I don't think that VaR in itself was purposefully or willfully abused. VaR is a model that requires a significant number of assumptions. For example, if I buy a product, such as an option, then I should assume that if the value of the stock goes down, then the value of the option will go down. If I write that option, so I sell it, then if the stock goes up, I can lose a lot of money. If I don't have the desire or the technology or the capability or the incentive to bother being careful about that, then I will assume that, if the stock goes up, I will make or lose a limited amount of money. That is a very, very poor assumption, which I think we have heard a lot today. If you take many of those poor assumptions and you add them up, you wind up getting VaR numbers, and not just VaR numbers but numbers of all sorts of different models that wind up being all but meaningless because of so many small poor assumptions that have added up into something that is just wildly incorrect. But folks like to believe their own numbers, especially when those numbers allow them to do things that they weren't able to do before. So it wasn't a willful misconduct as much as a carelessness, given the incentive structures that are out there today.

Chairman MILLER. Anyone else? Mr. Rickards.

PAST CONGRESSIONAL ATTEMPTS TO REGULATE THE FINANCIAL INDUSTRY

Mr. RICKARDS. Yes, Mr. Chairman, I just want to say that my recommendations, if we are going back to something like Glass-Steagall, there was more to that than just a walk down Memory Lane. I am not saying, gee, the system today has obviously failed, let us go back to what we had before. I actually derived these from my own research into the power load relationship that I talked about earlier, which is that scale—as scale goes up, as you triple or quadruple or increase by five or ten times the system, you are increasing risk by a factor of 100, 1,000, 10,000. That is the nonlinear relationship that Dr. Taleb talked about earlier, and I very quickly came to the conclusion—well, if that is the problem, then descaling is the answer, and Glass-Steagall is an example of that. There is a little bit, I think, of—you know, easy with hindsight, but perhaps some arrogance in the 1998–2001 period where I think Members looked back at the Congress in the 1930s and said, you know, they were Neanderthals, they didn't understand modern finance, they created this system. The Members of Congress in the 1930s had actually lived through something very similar to what we are living through now and this was their solution. They actually had firsthand experience.

Now, did a Member of Congress in 1934 understand fractal mathematics? No, it was invented in the 1960s. But they had an intuitive feel for the risks and I think their solution—we had a system that worked from 1934 to 1999, for 65 years. When the savings & loan (S&L) crisis happened in the early 1990s, it didn't take hedge funds with it. When we had the banking crisis in the mid-1980s, it didn't affect the S&L industry or it didn't affect investment banking. We were compartmented, and that is what saved the system. We have torn down all the walls. Commercial banks look like hedge funds. Investment banks look like hedge funds. Hedge funds originate commercial loans. It is a big business for them. So when everyone else is in everyone else's business, should it come as any surprise that if one part fails, it all fails.

Chairman MILLER. Thank you.

SHOULD A GOVERNMENT AGENCY TEST FINANCIAL PRODUCTS FOR USEFULNESS?

Dr. Taleb earlier suggested that there be something like the FDA that approves—actually it was not clear to me in the earlier panel to what extent they were calling for government conduct or setting rules by government that would prohibit things, or just people not doing them because they were stupid, but assuming we are talking about rules that may be set by government, Dr. Taleb suggested that the FDA reviews drugs to see if they do any good, they don't allow—the FDA doesn't allow patent medicines mixed up in a bathtub to be sold to cure cancer anymore. You could do all that you wanted in the 1930s. You can't do it now. And a great many of the financial instruments that led to all this have no readily apparent social utility and create enormous risk that is dimly understood by even the people who are selling them, certainly the CEOs and the boards of directors of their institutions. Should we be reviewing financial instruments for whether they have any useful purpose, and can you give examples of instruments that have no apparent purpose and have done great damage? Mr. Whalen.

Mr. WHALEN. Well, I think the short answer is no. I am not a big fan of regulation. I don't think the government has the competency to analyze complex securities in the first place. You would have to hire the people that do it. I think it is better to let the market discipline this behavior. Large buy-side investors, who I would remind you are probably the survivors of this period, they are the ones with the money, they tell the sell side what they want and they are going to tell the rating agencies what they want to see as well, and if you increase the liability to the issuers by forcing disclosure, by forcing SEC registration, you are going to see simplicity. Because otherwise my friends at the trial bar are going to come over the hill like the barbarians, and they are going to feast, and I think that is the way you do it. You don't want to get the government into a role where they have to make judgments about securities, because, frankly, who would you ask? The folks at the Fed? I mean, the Fed is populated by monetary economists who couldn't even work on Wall Street. I mean, I love them dearly, I go fishing with a lot of these people but I would not ever let them have any operational responsibility because they just don't have the competency.

So I think we have to try and take a minimalist approach that is effective, and the way you do that is by making the issuer retain a portion of the deal that they bring so that they have to own some of the risk. You make them make a market in these securities too. They can't just abandon their clients when they bring some complex deal and not even make a bid for it. That is a big part of the problem. If you make the dealers retain some risk and retain responsibility, then I think you will see change.

Chairman MILLER. Dr. Colander.

Dr. COLANDER. I wanted to expand a little bit on regulation from a different perspective, again, agreeing very much with what Mr. Whalen said, that there is a problem with government regulation, and we can go back and think about Glass-Steagall. You know, people responded to Glass-Steagall and said here is the problem, you know, that we deregulated. The problem was, during that time there was enormous technological change. We had to change the regulations, and now you have to—regulation isn't a one-time thing. It has got to be continually changed, and here is the problem. My students, when we asked how many were going on, you know, sort of—Paul Volker came up and spoke and he said, you know, big audience, "How many of you are planning to go on and work for government?" and I think two people raised their hand. Then he said, "How many people are planning to go on to Wall Street?" You know, you had all this large number, and this was a number of years ago. When my students coming out of Middlebury College as seniors can earn $150,000 to $200,000 in the first or second year and somebody coming into government can get, what, as a GS–8 or 9, you know, sort of $34,000 or something. You know, where are you going to go, how are you going to get the expertise to do it? And so what happens is, you know, you have an unfair system there, where no matter how much regulation you get, given the pay structure, given what's there, the people who are having it designed will be able to snow anybody who is trying to regulate it, and that is why very much I think you have to design it, not so we have to regulate it, but it is self-regulatory, and that, I think, is what you are hearing from people, that you have responsibility. If it's too big to fail, we have to regulate it so therefore let us see that is not too big to fail by making it smaller, that we structure it by the people who know the institutional structure, so that here you figure why you won't make that deal. But not for government to be coming in mainly because government will get beat.

Chairman MILLER. Mr. Rickards.

Mr. RICKARDS. Mr. Chairman, I think the idea that there would be a government panel of some kind that would vet and approve financial products in the manner that the FDA approves drugs is probably not workable, probably beyond the ability of government. But for example, credit default swaps: There is actually a use for them. They are socially useful when they are used to hedge a position in the underlying bond, but they become a casino ultimately underwritten by the taxpayers when they are used with no insurable interest. So it is hard enough understanding what a credit default swap is, but to get that distinction just right, when it may or may not be useful, would be extremely difficult. But I do believe there should be a quarantine in the sense that—let's have these products in hedge funds, in long-run investors or maybe with mild leverage. Let us keep them out of FDIC-insured banks and other institutions that perform this utility function and are in effect gambling with taxpayers' money.

I also endorse Dr. Colander's suggestion that, in the National Science Foundation, in the peer review process, there is a rule for looking at these things, perhaps not in the regulatory sense of approving them but in the academic sense of understanding them. And I believe what Dr. Colander is referring to is what I call 'cognitive diversity.' Let's just not have a bunch of economists or, for that matter, a bunch of physicists but let us have physicists, economists, behavioral scientists, psychologists and others work together. I think it is interesting that Dr. Kahneman at Princeton won the Nobel Prize in economics a few years ago. He is the world's leading behavioral psychologist. He wouldn't describe himself as an economist, but he made very valuable contributions to economics.

If you get 16 Ph.D.'s in a room and they all went to one of four schools, let us say Chicago, MIT, Harvard and Stanford, and they are all fine schools, you will actually improve the decision-making if you ask two or three of them to leave and invite in the first couple people who walk down the street. You will lower the average IQ, but you will improve the overall outcome because those people will know something that the Ph.D.'s don't. So at the National Science Foundation level, to encourage that kind of collaboration I think is very valuable.

I have actually—I am involved in a field called econophysics which basically is understanding economics using some physics tools, and I don't claim it is the answer to all these things but it does make some valuable contributions. But when I speak to—I have spoken at Los Alamos and the Applied Physical Laboratory, and I get a very warm reception. The physicists are very intrigued and they see the applications. When you talk to economists, they have no interest. They are like, what do physicists have to tell us. And I think more collaboration would be helpful.

IDENTIFYING FIRMS THAT ARE 'TOO BIG TO FAIL'

Chairman MILLER. Mr. Rickards, you know from personal experience that it is not just depository institutions that are systemically significant. How do we identify—I think you and Dr. Colander both have spoken about the problem of scale. How do we reduce the size of institutions? How do we identify the ones that are systemically important, either because of their size or their interconnectedness, as inappropriate for the kind of risk taking . . . that if we assume that there are some institutions, most hedge funds, that can be born and die without any great consequence to the rest of the planet, and that if they want to use voodoo, they can. How do we identify those that we have different standards for? Mr. Whalen.

Mr. WHALEN. Well, I think there is two simple answers. First off, we have to revisit market share limits. You have already seen this in process with the FDIC because they have started to levy premiums against total assets, less capital, instead of domestic deposits. I think that is a very healthy change. But perhaps more important, we have to let institutions fail, because if you convince investors that you are going to put a Lehman Brothers or a Washington Mutual into bankruptcy, they are going to change their behavior, and I think both of those events were inevitable, by the way. I think it is ridiculous to argue that Lehman could be saved. They were for sale for almost a year. Nobody wanted to buy it. So, you know, at the end of the day, if we don't allow failure, and we don't inoculate our population against risk by letting them feel some pain from time to time, then we will repeat the mistake.

Last point, we have got to get the Fed out of bank supervision. Monetary economists like big banks. They love them. I worked in the applications area of the Fed in New York. I can't recall a merger, a large bank merger that they have ever said no to. I worked on the "Manny Hanny" (Manufacturers Hanover Trust) transaction, I worked on the Chemical Bank merger, following that with Chase. In each case, you could make a very strong case that those were bad mergers. They destroyed value. And then look at Bank of America. They had to buy Countrywide because they were the chief lender to Countrywide's conduit. They had no choice. It was kind of like J.P. Morgan buying Bear Stearns. There really was no choice. But then we have the Fed slam Merrill Lynch into Bank of America to save a primary dealer. These are the monetary economists saying oh, dear, we want to have a few big primary dealers we can manage and deal with, it is easier for us. Well, I think that is a really skewed perspective. I would like to see another agency responsible for approving mergers of financial institutions that actually looks at it on an objective basis and says, is this a good idea, because we have got a couple mergers, Wells, Wachovia and Bank of America with Merrill Lynch that I am not sure are going to work. I think both of those institutions may have to be restructured and downsized significantly in the next couple of years.

Chairman MILLER. Mr. Rickards.

MONITORING AND ANALYZING HEDGE FUND ACTIVITY AND RISK

Mr. RICKARDS. Mr. Chairman, on the issue of what is a systemically important hedge fund, at the end of the day there will be some element of subjectivity in it—whether a $10 billion hedge fund is systemically important, but if you have $9.8 billion, you are not. It will be a little bit arbitrary and it can't be based solely on size. It has to be based on the complexity. But the first step is transparency. You will never be able to make any informed decisions like that without good information, and every hedge fund manager—I have worked in hedge funds banks and investment banks—they will resist that for various reasons but I don't understand why the United States Government couldn't create a facility that would keep that information on a secure basis. We keep military secrets, we keep intelligence secrets, we keep other information confidential. You could have a firm like, you know, IBM Global Services that would come in, build a facility. It could be secure, get clear people running it, and then just say to all hedge funds, look, you have to give us all of your information, all of your positions, all of your factors in a standardized format, in an automated format once a week, we will keep it in a totally secure environment, it will not leak out, but we are going to take that and load it into, you know, covariance metrics. We are also going to do that for your firm, and we are going to have an idea at that point when you are taking systemic risks, and at that point there ought to be an ability to intervene. And I agree with Mr. Whalen, it should not be the Federal Reserve. They do a lousy job with their primary task of preventing inflation, and I don't know why they have been given all these other jobs. But there certainly would be expertise in the government to do that much, and then to intervene when necessary.

Dr. BERMAN. Adding that, taking all that data, bringing it together——

Chairman MILLER. Dr. Berman.

Dr. BERMAN.—and putting it into a large covariance matrix sort of sounds like VaR. I mean, that is—so you come back to those same questions all the time when you say how do we make predictions? This may sound like I am answering the question with the same exact question, but the best way to protect against this is to ask the question to the bank: what would happen if you failed? And then determine what the outcome to society or to the economy would be. It is not based on the size of the bank, it is based on, look at what would happen, not the probability but if the bank fails, if a hedge fund fails, what actually will wind up happening, what are the knock-on effects. That requires an enormous amount of transparency but you don't need to necessarily make the predictions about that, you just need to follow that thread through.

Chairman MILLER. Dr. Colander.

Dr. COLANDER. One of the principles, you know, sort of within economics, is taxes have to have reasons and everything else. And I think one of the things that thinking of the economy as a complex system brings up is that bigness is, per se, bad, you know, sort of an interconnection is, so we have lost the sense that there can be a tax on 'bigness' so that people can decide but it is built within that. And to start thinking that, here, if you have a complex system, you have got to keep a whole number of different elements, and the only way you are going to be able to do that—because there is enormous pressure to grow—is to somehow design within the system a counterweight to that, and so thinking along those lines, I think is something that follows thinking of the economy as a complex system.

Chairman MILLER. Mr. Whalen.

Mr. WHALEN. I will come back to something Mr. Broun said about the community banks because I think it is very important, and you all are going to be hearing about this a lot next year. If you are going to tax institutions based on risk, and I think that is sound, you start with the FDIC. The big banks should pay more than the little banks, and when we see the size of the hole that we have to fill in over the next, I don't know, 25 years from this crisis, I think that is going to become a very compelling argument. The community bankers are going to be living up here next year when they start seeing the estimates for what they have to give up in revenue and income to fill in this hole. Remember, we are still paying for the S&L crisis. There is still debt out there that we are paying interest on. We are going to be paying for this crisis for 100 years. That is how big the numbers are. So think of that as a load on the economy. That is kind of the cost of modeling run amuck, and, you know, I am serious about this. We are going to be paying for this, the banking industry, consumers, investors in banks are going to be paying for this for many, many decades.

Chairman MILLER. We are—Mr. Rickards.

Mr. RICKARDS. Just briefly. The inverse of complexity is scale. You can have complexity at the small scale, a medium scale or a large scale. Failure at the first two will not destroy you. Failure at the third may, and so I am not against complexity. There is going to be complexity. But, again, Dr. Taleb's example, an elephant is a very complex organism, but if it dies, the entire ecosystem doesn't crash. And so let us keep these things in boxes and reduce the scale as the antidote to complexity.

Chairman MILLER. We are at the end of our time, but I want to thank all of you for being here. Under the rules of the Committee, the record will remain open for two weeks for additional statements from Members and for answers to any follow-up questions the Committee may have for the witnesses. Again, I appreciate your willingness to come and testify, and it will be useful to have all of you as resources for the future as well. Thank you very much. The witnesses are excused and the hearing is now adjourned.

[Whereupon, at 1:30 p.m., the Subcommittee was adjourned.]

○