

# **Ants and Astronauts**

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I'm honored to be asked to contribute to this conference but the very distinguished participants make me feel like an ant among astronauts: a tiny creature that doesn't see very well or think ahead. Astronauts exemplify the very best of human intellectual achievements in science and technology. They peer into the cosmos. They predict orbits accurate to microns and milliseconds.

But is an approach that works brilliantly in outer space well suited for ordering the affairs of earthlings? If ants can't see the tree for leaves, astronauts see neither leaf nor tree nor forest; at best in low orbits they may infer forestland from patches of green. For inanimate objects their predictions may be spot on, but how well can their algorithms cope with events shaped by imagination and enterprise? Can we rely on some law of large numbers to smooth out the unpredictable consequences of idiosyncrasy?

Ants let me add, have coped for millions of years with cataclysmic changes in their habitat and with unrelenting radical uncertainty (as Keynes might call it) in finding food and escaping predators. Somehow, they put their tiny little brains together to solve difficult problems. Therefore, after I have commented on the individual papers, I will take the liberty of sketching out an ant-like process to formulating policy and the role that astronauts can play in such a process.

## **Curbing Credit Cycles**

Booms and busts in bank lending triggered by races to the bottom have vexed banks and their regulators from the get go. After the Civil War, the OCC and clearinghouse associations tried to discourage banks from paying interest on demand deposits because they thought that the practice led to reckless lending. In

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This comment discusses three papers delivered at the conference: 1) "Curbing the Credit Cycle" by David Aikman, Andrew Haldane and Benjamin Nelson. 2) "Swings in the Rules-Discretion Balance" by John Taylor. 3) Principled Policy Making in an Uncertain World by Michael Woodford. Thanks to Srikant Datar, John Kay, Richard Nelson, and Richard Robb for comments and suggestions

its early years the Fed did as well. The 1933 Banking Act simply forbade most banks from paying interest on checking accounts (and regulated interest on time deposits).

The Bank of England's Aikman et. al suggest that the problems of booms and bust and contagious imprudence are greater than ever before. They also argue that macro-prudential policies are a more effective antidote than the structural reforms aimed at individual banks (they call these "micro-prudential" policies) that John Kay and others have suggested.

The paper starts with a model in which coordination failures generate credit cycles. Empirical analysis is then presented suggesting credit cycles exist, are distinct from the business cycle, and arise from coordination failures. The consistency of the evidence with some of the implications of the model is then said to validate the model as a whole. Adding more assumptions, the authors deduce the policy implication that credit cycles are better curbed through macro-prudential policies rather than micro-prudential ones.

If you happen to be so predisposed this sequence of theorizing and empirical testing may well constitute a convincing case for macro-prudential regulation. But what if you are John Kay, or Paul Volcker who said recently of macro prudential regulation: "somehow those words grate on my ears?"<sup>1</sup>

Full disclosure: I too start as a skeptic having just published a book advocating tougher boots on the ground, case-by-case bank examination -- micro prudential regulation as Aikman et al would call it. Perhaps inevitably I have several doubts about the reasoning and conclusions, even as I find the data fascinating.

Does evidence that some of the predictions of a complex model are borne out justify belief that the model as a whole is right? Even if the base model is right, what justifies its extension (with no further testing) to compare macro- and micro-prudential regulation? Does the extension even capture important features of the two alternatives?

Other mechanisms could generate credit booms and busts. For instance, and here I'm literally talking my book\*, I argue that periodic collapses before the 1933 and 1935 Banking Acts resulted from the absence of effective deposit insurance and bank examination and the failure of voluntary coordination to curtail competition for deposits. After these problems were solved, we had decades of banking stability. That stability broke down, because the Federal Reserve let inflation get out of hand in the early 1970s, making

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\* A Call for Judgment: Sensible Finance for a Dynamic Economy (Oxford University Press 2010)

rules such as Regulation Q impossible to sustain. The efficacy of micro-regulation was undone, in my story, by a macro failure.

Given competing explanations we cannot wave away the question of the plausibility of assumptions in the Aikman et al model. Here are a few.

“Each period there is a continuum of agents, all of whom work as financial intermediaries for a single period. In other words, bankers have, by assumption, short horizons. They aim to maximise their reputation in the market at the end of this period. At the beginning of each period, each bank originates a risky asset, the return on which depends on (i) the banker’s ability, which can be high or low, and (ii) the macro state, which can be good or bad.”

The assumption that mega banks and their employees have short horizons resonates. But the stipulation that they are only concerned about maximizing reputations does not. And what about all the many other simplifications such as single asset banks and bankers with high and low abilities?

We don’t permit medical devices designed solely on similarly stripped down models of the human body, even if those models capture essential physiological principles because we know that the seemingly peripheral features have fatal consequences. So it is with car brakes and bridges. With short buildings architects may assume away oscillations caused by high winds but not with skyscrapers. Betting on a highly simplified model of banks and their interactions seems equally as dangerous, although as I will argue in the concluding section, such models can make a contribution to a constructive policy debate.

The increased centralization of authority implied by macro prudential regulation strikes me as high winds left out of the analysis. Recognizing the need for tough banking regulation does not imply the embrace of centralized control. Power corrupts and absolute power corrupts absolutely. Centralization also increases the likelihood of catastrophic mistakes. Trofim Lysenko ruined Russian agriculture because as Stalin’s director of biology he was able to impose his theory of environmentally acquired inheritance. Decentralized authorities also make mistakes. Some examiners may be too strict others too lenient. But such errors are less likely to be systemic.

The paper likens the state of monetary macro-policy today to the state of monetary policy after the Second World War when data was incomplete, theory patchy and policy experience negligible. The comparison does not reassure. We have been through a major crisis that some critics regard to be the result of a terrible mistake in policy; others hold monetary policy blameless. We find sharp divisions over quantitative easing among the very distinguished participants at this very conference. Would it be

progress if fifty years from now we advanced to a similar controversy in macro-prudential policy, hoping and praying that experts from the right camp were in charge?

The authors assert it would be a big mistake to not try to develop a macro-prudential capability. My opposing view is, why? In a decentralized economy we ought to avoid, if we can at all help it, top down edicts. If regulators believe that credit is expanding too rapidly and that the standard tools of monetary policy do not offer effective remedy, why not pass on the word to examiners to be a little tougher? Why set up a centralized apparatus that would presumably seek across the board reductions ignoring crucial differences between individual borrowers, throwing away the valuable local knowledge of on-the-spot examiners?

### **Rules versus Discretion**

If the 2008 crisis impels attention to anyone for precise prescriptions, and not just dire prognostications, it must be John Taylor. As Taylor's paper ends with a broad policy recommendation it behooves us to examine it carefully.

In the last forty years, Taylor observes, there was first a swing toward rule-based economic policy making and away from discretion. The swing improved macroeconomic performance. Yet and without any other obvious compelling political reason, policy then swung towards discretion, worsening macroeconomic performance. The clear implication: "we should go back toward more rules-based policies."

Several questions arise: First, what's a rule?

The Taylor rule, first proposed in 1993, surely qualifies. It says (in case you have been living in a cave) that the short term interest rate should be set by the central bank to equal one-and-a-half times the inflation rate plus one-half times the GDP gap plus one.

But what about the prudent man rule that's been around since 1830? It directs trustees "to observe how men of prudence, discretion and intelligence manage their own affairs, not in regard to speculation, but in regard to the permanent disposition of their funds, considering the probable income, as well as the probable safety of the capital to be invested." Recast for central banking, it could say "central bankers should observe how other central bankers of prudence, discretion and intelligence manage monetary policy, in regard to setting short term interest rates, considering the probable inflation rate as well, as well as the probable unemployment rate."

The Taylor rule would suit Max Weber's idealized bureaucracy comprising experts who made decisions "according to calculable rules" and that in its perfectly developed form, eliminated "love, hatred, and all purely personal, irrational and emotional elements which escape calculation." The prudent man rule in contrast is adjudicated by jurors who are swayed by their subjective assessment of the character of the disputants and by what they believe the right thing to do was.

So is the prudent man rule merely discretion in drag?

Second, how much steadfastness can we expect of policy makers if they almost capriciously abandon rules, as Taylor argues? (In a totalitarian regime it usually takes the death of the despot). If abandonment cannot be easily explained it can't be predicted. And if it can't be predicted, don't some of the advantages of rule-based policy diminish?

Third, to what degree would we even want immutable rules? A dynamic capitalist system is technologically progressive, as Marx put it. Technological advances often demand new rules. When petroleum was discovered courts grappled with the question of mineral rights: did you own the oil that was under your land? Today it has been decided that we have no right to prevent airplanes from flying overhead and it is a criminal offence to block the transmission of cellular phone signals in your home.

Regulators and law-makers once swore by the real bills doctrine for banks – don't use short term deposits to make long term loans. The real bills principle was built into the 1913 Federal Reserve Act. Then came the great boom in mass production and mass consumption and the demand for installment credit to buy cars and radios and vacuum cleaners. Eventually by the mid-1930s, with new deposit insurance rules in place, regulators decided that maturity transformation by banks was not such a bad thing. Today we take it for granted.

The Taylor rule is a valuable heuristic, a worthy default principle for central bankers; but, in an ever evolving economy it cannot be treated as an immutable law of nature. The main independent variable, inflation is an arbitrary construct: in a dynamic economy the basket of goods is always changing. The composition of our consumption has also changed, and continues to change, towards more services, often produced by governments rather than private businesses. Discretion in measuring inflation is unavoidable. We should expect that the right Taylor rule, in terms of its structure and parameters, will change. John Taylor tells us that mechanistic estimations of changing parameters are a bad idea. Fair enough. But doesn't this imply using ongoing discretion to decide when and how we should alter the rule?

What to do in emergencies? I happen to agree with many of Taylor's criticisms of the policy response to the 2008 crisis: like Taylor I too wrote op-eds at the time arguing that the measures were too panicky, that sound principles were too quickly abandoned. But our assessments were judgments. In a world without counterfactuals, even looking back and deeming the measures to have been absolutely necessary or a reckless waste is also ultimately a guess.

Finally under what conditions would we prefer not to be bound by a rule? We shouldn't default towards a rule simply because discretion fails as it quite possibly did in the invasion of Iraq. But that doesn't suggest to me the need for a calculable rule for invasions and regime change.

### **The Power of Principles**

Michael Woodford's paper on "principled policymaking" addresses head on the problems that arise in maintaining a predictable policy stance in an uncertain world. Woodford offers a hierarchy of possible policy commitments, ranging from the specific to the abstract. Non routine change makes commitment to a specific, fully explicit formula, such as the Taylor rule, undesirable (and I would add, as a practical matter difficult to sustain.) But commitment to more abstract strategies and policies can be more desirable. Here too I'd add, more practically sustainable: the prudent man rule has survived for 170 years because it can be flexibly interpreted and reinterpreted as times change.

Woodford's hierarchy raises three questions for me, although I am largely in tune with his argument. What is the right level of abstraction in 'higher level' principles? More abstract principles can more easily accommodate change. But they also have less bite and therefore less utility in terms of the predictability of the specific decisions that will be made through their application. Think of the concept of the "pursuit of happiness." Are there any general principles or even ad-hoc heuristics that can help us think about the right degree of abstraction?

Second: how does one formulate principles for questions that have never arisen before? Before there was broadcasting, there was no conception of an FCC, much less of the principles that such an agency would adhere to. Before there was a Fed, there could be no discussion of the principles it would follow in formulating monetary policy. And indeed even when the Fed was created in 1913 its monetary policy was simply adherence to the gold standard (according to a Minneapolis Fed publication.)

And, what happens if the principles adopted at the outset turn out to be dysfunctional? Are we bound to live with the original sin as it were?

Let me therefore suggest an even higher level commitment that Woodford does not explicitly discuss – commitment to a process. We cannot possibly predict what laws Congress will pass or the mandates of the new regulatory bodies it will create. What cases the Supreme Court will choose to hear ten years from now and how it will rule is unknowable. But we can and should expect a predictable legislative and judicial process. Although the U.S. Constitution was farsighted, it wasn't perfect. One of its great strengths has been a process that strikes the right balance between continuity and change, constancy and adaptability. If the US constitution didn't contain provisions for amendments we might still have slavery and women wouldn't have the right to vote. Amendments permit experiments such as Prohibition -- and their termination when society deems them to have failed. At the same time by making amendments arduous, the constitution affords a predictability to existing and not-yet conceived of rules that would not exist if there were no constitution or changes could be made by simple majorities in the legislature.

### **A Pragmatist (not a typo) Process**

The importance of process leads me to an issue that all three papers raise: How do we resolve policy questions – choose between macro- and micro-prudential regulation and discretionary and rule-based regimes, for instance?

Milton Friedman's seminal 1953 essay, "The Methodology of Positive Economics," provides the intellectual foundation for an approach espoused by many economists. The essay divides economics into "positive" and "normative" – how the world works and how it ought to work. In Friedman's view, the positive ought to precede the normative: we can't figure out how we should act unless we can predict the consequences of our actions.

Friedman then proposes a two-step process for developing positive propositions: the formulation of an abstract theory, followed by empirical tests of its predictions. The first step *requires* unrealistic assumptions -- "wildly inaccurate descriptive representations of reality." They must be because: "a hypothesis is important if it 'explains' much by little" by abstracting "crucial elements from the mass of detailed and complex circumstances."

The second step -- empirical tests of a theory's predictions, according to Friedman, redeems simplifying or even blatantly false assumptions. Expert billiard players may never actually make complicated mathematical calculations; but if the hypothesis that expert players make their shots "as if" they did, yields excellent predictions, why not go with this fantastic formulation?

Model building is mathematical and abstract whereas testing requires rules that map the abstractions into observable phenomena. These rules “cannot possibly be abstract and complete” and their formulation is at least as much art (which cannot be learned by rote) as science. Yet the two steps are inseparable. Rules that specify the circumstances under which the abstract hypothesis works and the “general magnitude of the error in its predictions under various circumstances” are “an essential part of the hypothesis.”

In the idealized world postulated by Friedman, abstract models would be put to normative use only after they had been validated by testing their predictions – and then only in situations specified by the rules. No “off-label” use, to use a term from the pharmaceutical industry (for drugs approved for one condition but prescribed for another).

Friedman’s approach does not appeal to this ant. Abstract models have serious limitations in analyzing or making good predictions about complex physical entities or social arrangements. The philosopher Nancy Cartwright offers the striking example of how the amount of gas entering the chamber of a carburetor is controlled. Starting with David Macaulay’s entirely verbal description in *How Things Work* she constructs a system of equations for the functional laws governing the amount of gas in the chamber. Much is lost in such a translation however observes Cartwright: Macaulay’s original text is replete with “thick” verbs (the carburetor *feeds* gasoline) and therefore provides a far more content rich causal theory about carburetors even when the functional forms of the equations are right. \*

The limitations of symbolic representation encourage product developers to build physical replicas and prototypes to test out their ideas. But when the artifacts are complex or must be embedded in complex systems even physical models produce unreliable results. Products and processes that work flawlessly under laboratory conditions fail in factories and day-to-day use. Artificial organs that have been perfected as physical models and work in chimpanzees can kill humans.

Social arrangements are even more difficult to model. Their foundational causal mechanisms cannot be reduced to precise laws (unlike the laws of thermodynamics that govern carburetors).<sup>2</sup> The interactions between the individual elements of the arrangements and foundational causal mechanisms are even more unpredictable and complex. And, the ever-evolving nature of social arrangements makes historical patterns an unreliable foundation for future choices. In contrast, once developers hit on workable medical devices, they can count on that design to perform satisfactorily forever.

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\* *Hunting Causes and Using them* (Cambridge University Press 2007)

Yet that very dynamism of the human condition, the individual and organizational enterprise that marks modern society, creates a constant demand for new rules in the private as well as the regulatory sphere. Who would have thought just a few years ago that privacy on Facebook and Google Earth would become public issues?

Empirical testing of predictions does not prevent the misuse of flawed models in the way that Friedman suggests it should.

In most cases, it is next to impossible to run controlled experiments to test predictions. Here, Friedman argues that predictions about historic data – “phenomena that have occurred but observations on which have not yet been made or are not known to the person making the prediction” can be an adequate substitute.<sup>3</sup>

As Friedman concedes though, the interpretation of historical data “generally requires subtle analysis and involved chains of reasoning, which seldom carry real conviction.” Statistical models must be interposed between the hypothesis and the data. These models come with their own slew of assumptions, some unverifiable (for instance that the model has the right specification), others implausible (e.g. that all observations have been generated by the same process) and yet others often demonstrably false (that errors are normally distributed, for instance).<sup>4</sup>

Then there is the issue of who decides whether predictions are “good enough” since they are never perfect. “The background of the scientists is not irrelevant to the judgments they reach” as Friedman points out. “The economist will be more tolerant than the sociologist” in judging the evidence favoring a hypothesis that assumes the “single-minded pursuit of pecuniary self-interest.” Even among economists, partisans of competing theories can offer radically different interpretations. Witness the rancorous divisions over what the 2009 stimulus package achieved.

More often than not therefore econometric tests have a Rorschach quality, surfacing prior beliefs rather than the veracity of a hypothesis.\* And we end up with implausible models pressed into normative use without convincing empirical evidence, and on occasion, in spite of, contradictory evidence.<sup>5</sup>

My proposal follows the process used by many engineers, medical researchers, judges and businesses, not to mention ants.

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\* The Bayesian (as opposed to the “classical”) approach to statistics, subjective interpretations are explicit.

First off it focuses on concrete problems without assuming away important but inconvenient particulars that might stand in the way of a pet solution. Thus engineers undertake the design of hybrid cars, medical researchers to find a cure for cancer, businesses to streamline their supply chains and Supreme Court justices rule on the constitutionality of search and seizure practices. I'm not suggesting a wall between practical problem solving and abstract theorizing. Abstract principles – laws from the natural sciences for instance – can help solve engineering problems. Conversely the solution to particular problems can suggest general principles. Engineering breakthroughs have led to advances in the sciences and a Massachusetts court decision, *Harvard College v. Armory* established the prudent man rule. But my proposal does entail quite different sequence than the one in Friedman essay where abstract model building comes first.

Again taking my cue from engineering, medicine, business and the law, I would suggest that practical problem solving is well served by inclusion, integration and iteration.

An *inclusive* process is valuable because a plurality of perspectives and knowledge drawn from multiple fields is often necessary to solve complex problems. New products were once developed and businesses built by solo inventors and entrepreneurs; we now rely on multi-functional teams and networks that cross conventional organizational boundaries. The U.S. constitution was drafted and ratified by individuals with a wide range of backgrounds and beliefs and plurality was built into all branches of government. Even the right to declare war lies with Congress says the constitution, not the Commander-in-Chief.

Processes that *integrate* multiple viewpoints help realize the benefits of wide inclusion, making the ant colony much smarter than any one ant. Vigorous debate helps weed out bad ideas, but although a good process is dialectical it is not gladiatorial. The goal is to find the best common ground not the ant with the best argument. An astute Speaker of the House crafts legislation that attracts large bipartisan majorities and a good Chief Justice builds a consensus for unanimous opinions. Such opinions strike a balance between the arguments of prosecutors and defendants, the briefs of police and civil libertarians, the letter of the law and prevailing norms, and the diverse beliefs of the individual justices.

Good processes also contain mechanisms for sensible *iteration* to correct for errors and to adapt to new circumstances, such as the collapse of Bretton Woods, the radical economic reforms in China after 1978, or the astonishing proliferation of the web.

Abstract economic models with highly simplified assumptions do have a role to play in my proposal. It even allows us to embrace theories whose results can't be tested (and would therefore be rejected under the Friedman standard). Testable or not, simplified models can alert us to unintended consequences and

tradeoffs that aren't intuitively obvious (comparative advantage and the Coase theorem come to mind) and raise doubts about choices that many consider unavoidable (between inflation and unemployment for instance). In my scheme, statistical tests can also contribute to the dialectical process, in spite of their inevitable ambiguity and inconclusiveness.

The process I have sketched and the role for models and statistical evidence it implies has much in common with the "pluralist methodology for economics, employing a range of partial models and of sources of evidence, based on a notion of rationality as 'reasonableness'" that, according to Sheila Dow's paper at this conference, follows from Keynes's ideas on knowledge.\*

But playing a role in policy formulation is one thing, dominating the sandbox is another. Ricardo's ideas made an important intellectual contribution but the Corn Laws were repealed after a vigorous and extensive political debate. Unfortunately in recent decades a relatively small number of similar theories and beliefs have dominated the formulation of many important public policies and private financial choices. The Fed for instance has come a long way from the 1913 legislation that was designed to give many individuals and interests voice in its operation and to prevent a few in Washington or New York from calling the shots.

We can only hope the crisis leads to a serious rethink.

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\* Keynes argued, writes Dow "that any formal model is bound to be an incomplete representation of an open-system reality" because "it abstracts from the complex interactions and evolution of interactions, as well as unforeseen developments which characterise that reality.... The better approach therefore, from a Keynesian perspective, is simultaneously to segment off certain aspects of reality for partial formal modelling, keeping 'at the back of our heads' what needs to be relaxed when applying the model's conclusions to combination with the conclusions of other partial models, and then to policy recommendations. This methodology explains why Keynes's general theory did not take the form of a single large model"

## ENDNOTES

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<sup>1</sup> Remarks delivered at the Federal Reserve Bank of Chicago on September 23 2010. Downloaded from <http://blogs.wsj.com/economics/2010/09/23/volcker-spares-no-one-in-broad-critique/>

<sup>2</sup> According to Elster with social phenomena causal mechanisms often act in opposing pairs and are triggered in unpredictable ways.

<sup>3</sup> “A hypothesis may imply” Friedman writes, “that such and such must have happened in 1906, given some other known circumstances. If a search of the records reveals that such and such did happen, the prediction is confirmed; if it reveals that such and such did not happen, the prediction is contradicted.”

<sup>4</sup> It is also tempting to construct models that “predict” historical facts that are in fact known to the modelers or fish for facts that fit the model and ignore the ones that don’t.

<sup>5</sup> There has long been an enormous market in second-hand cars; this didn’t discourage interested parties from invoking the Akerlof model to pass lemon laws.