# Uncertainty in Economic Analysis and the Economic Analysis of Uncertainty*

Lars Peter Hansen†

January 31, 2017

*Real knowledge is to know the extent of one's ignorance.*

- Confucius

## 1 Introduction

When I think about knowledge, I find it virtually impossible to avoid thinking about uncertainty. Uncertainty adds a new dimension to discussions of knowledge that are especially important in economic analyses when we seek a better understanding of markets and the resulting outcomes for society and quantitative answers to important policy questions. It has been important in my research and also more generally in economic scholarship to take inventory, not only of what we know, but also of the gaps to this knowledge. Thus, part of economic research assesses what we know about what we do not know. Uncertainty matters not only for how economic researchers interpret and use evidence, but also for how entities such as consumers and enterprises that we seek to model confront the future.

My own research interests explore connections between dynamic economic models and statistical methods for analyzing time series data. The relevant data are spaced over time by taking snapshots or averages of measures of macroeconomic outcomes and financial market returns. My substantive focus is on the connections between financial markets and the

*Amy Boonstra, William Brock, Stephen Stigler and Grace Tsiang provided valuable feedback on earlier drafts of this essay.

†University of Chicago, E-mail: lhansen@uchicago.edu.

macroeconomy through the construction and assessment of formal models with quantitative ambitions. Quantitatively-oriented economists build what are called structural models. This ambition takes us beyond the pure forecasting problems that occupy considerable attention in the private sector. In his Nobel address, Milton Friedman wrote:[1]

> Positive scientific knowledge that enables us to predict the consequences of a possible course of action is clearly a prerequisite for the normative judgement whether that course of action is desirable.

This ambition requires not only those data but formal economic models to use in interpreting that data, models that are often referred to within economics as structural models. Structural models aim to make counterfactual predictions. Counterfactuals inform us as to what happens when we explore changes in policy that push us outside the realm of historical experience. Such questions emerge when we entertain alternative monetary or fiscal policies and when we consider the impact of governmental oversight of financial markets and other forms of regulation. These are policy questions with consequences to the entire economic system. The counterfactual predictions are an explicit form of policy analysis for an interdependent system typical of dynamic economic models. They are meant to answer policy relevant questions, but to do so rigorously, they require clear statements of what is maintained as constant or invariant when we alter other parts of the system.

The formal definition of a structural model was elegantly articulated by Hurwicz (1966). The credible development and application of structural models in economics relevant for policy analysis remains an important research challenge.

Counterfactual predictions are most appropriately framed by using probabilities. While we might wish that a counterfactual prediction be a simple number, this is typically not a credible ambition. One source of uncertainty confronted in economics is external random impulses or exogenously specified shocks. Decades ago, Frisch (1933) featured dynamic economic models characterized by the transmission of random impulses over time to economic variables of interest. Following on the insights of previous scholars such as Slutsky (1927), random or unanticipated changes to the economic environment have influences that persist over time. Random changes in the weather can have a lasting impact on agricultural production. Random changes in technology, including say information technology, take time to fully absorb and exploit, and they can have durable impacts on the economic system. Formalizing these surprise changes as random impulses when incorporated into an

[1] See Friedman (1977).

economic model makes predictions probabilistic. Given that the inherent random impulses have lasting impact the resulting modeling outcome is a stochastic process with temporal dependence in the economic variables. By assumption, we cannot know in advance the outcome of these random shocks. An additional source of uncertainty emerges because we only know model inputs imperfectly. This source is often and conveniently captured by so-called subjective probabilities. Observations that we accumulate over time and from a variety of sources help in learning or resolving this specification uncertainty, but this learning may occur slowly.

In spite of a strong conceptual basis for structural models, some critics go so far as to dismiss the quantitative aspiration of structural models as hopeless.[2] At the very least there is a justifiable concern that our quantitative models might miss something important, as they are at best rough approximations to a more complex economic system. There are multiple components to *uncertainty in economic analyses*, and it is a challenge for researchers to characterize the full nature and magnitude of these components.

Economic models contain people making decisions often in the presence of uncertainty. For instance, any investment in human or financial capital requires a forward-looking perspective to determine the nature and magnitude of the investment. In even modern day farmers' markets, suppliers confront uncertainty by deciding how much to bring to the market place, and they make guesses as to the likely demands for their goods. For tractability, economists are led to embrace simplified models of decision-making for how individuals cope with this uncertainty, recognizing that they are at best approximations. Such models are well understood not to do justice to the full set of insights from psychology of individual decision-making.

One relevant concept in decision-making is the risk aversion paradigm commonly used in economic analyses which endows decision makers with known probabilities over possible events that can be realized in the future. But market environments can be complex, and this complexity makes it challenging to assign probabilities when using a risk aversion model to capture individual behavior. While some model builders may prefer to use so-called rules of thumb to be used in structural economic models, these so-called rules of thumb still must specify how these rules adapt to the environmental complexity and changes as we explore alterations in the underlying economic environment. The *economic analysis of*

[2] For prominent historical examples, see Keynes (1939)' skepticism of Timbergen's initial efforts at econometric model building and Hayek (1974)'s discussion of quantitative models and their implied measurements in his Nobel address: *The Pretense of Knowledge*.

*uncertainty* becomes a central ingredient in the construction of dynamic economic models. It has ramifications for prices that clear markets and for how resources are allocated through the use of these markets. Instead of positing rules of thumb, I will explore more disciplined ways to extend the elegant and valuable risk aversion model used pervasively in economics.

In less formal terms, imagine entertaining multiple views (in my case models) of the economic system with uncertainty about which might be the best one. These views are relevant because they provide inputs into the forward-looking decisions we make. Instead of committing to just one view, all might be considered but with different weights attached to their validity. The choice of weights may not be obvious and in fact may even be influenced by the implications of the alternative views. Taking this one step further, add in an acknowledgment that each of the possible views is a simplified guess and not a fully complete or accurate picture of the economic system. To connect to the formalization that I use in this essay, think of views as models with implied probabilities of outcomes. How to weight the predictions of these models and to capture their potential limitations adds to uncertainty about opportunities decision makers might face in the future. I take such considerations to be pervasive and applicable to individuals, businesses, and to the design and conduct of economic policy.

## 2 Components of uncertainty

Initial formal contributions of probability as an application of mathematics were to games of chance, such as flipping coins, throwing dice, drawing colored balls randomly from an urn with a known number of each contained in the urn, and complex extensions of such games. The formalization of probability in conjunction with games of chance has a long history. The study of potentially complicated games of chance drew in eminent mathemeticians, including Blaise Pascal and Pierre de Fermat in their famed exchange about the so-called "problem of points" or "division of stakes." The analysis proceeded with given or pre-specified probabilities. This component of uncertainty where we know probabilities but not outcomes is what I will call *risk within a model*, building on a distinction made by Knight (1921) and others. I include *within a model* to remind us that we are taken as given the probabilities. The case of known probabilities is a key part of how economists and others conceive of risk aversion. In dynamic contexts, the random impulses that I mentioned previously when modeled formally with probability specifications provide sources of macroeconomic risk confronted by individuals, markets, and governments.

An original contributor to the use of probability theory for the analysis of social science data is Jacob Bernoulli, one in a family of mathematicians, over three hundred years ago. His discovery is the Law of Large Numbers along with some refinements. His fundamental result characterizes how unknown probabilities are revealed by repeated sampling say from an urn with an unknown fraction of white and red balls. Bernoulli was not motivated by games of chance but instead by the application of probability theory to represent and understand social scientific data. These are data in which probabilities are unknown *ex ante* and only fully revealed imperfectly by actual data. These probabilities or their implications are presumed targets of the empirical investigation. See Stigler (2014) for a thoughtful discussion of Bernoulli (1713)'s accomplishments and influence.

Bernoulli confronted a common situation in which we do not know probabilities but seek to learn about them. Sometimes this learning occurs so quickly as to reveal the answer we seek but often not. This is why we have a field of statistics to study more complicated versions of the question that intrigued Bernouolli. For the purposes of this essay, the conceptual contributions of de Finetti (1937) and Savage (1954) stand out. They provided a framework for subjective probability. If you take n draws from an urn with an unknown fraction of balls, subjectists argue that the draw n+1 should not be viewed as independent of the previous because this draw will be informative about the unknown probability. Statistical independence commonly used in building is a conditional statement, one that conditions on the actual probability. Bernuoulli's calculations were made conditioned on the probability, say the fraction of white balls in the urn, treating the draw $n + 1$ as independent of draw $n$. To complete the probability specification from the de Finetti (1937) and Savage (1954) perspectives require a "subjective probability" (prior) over the possible fractions which induces a form of dependence, but it also allows for the formal probabilistic statement of what we know about the fractions of white balls after observing $n$ draws from an urn. While I use urns in this illustration, what really interested Bernoulli is what we can learn from data about the probabilities of outcomes.

More generally, when external analysts such as econometricians are unsure which among a family of possible models is correct, subjective probability suggests that we assign weights to the alternative models. Given an initial weighting, we open the door to the elegant Bayesian approach to learning. I use the term *ambiguity about a model* for the component of uncertainty that pertains to how we assign weights across alternative models. While de Finetti (1937) and Savage (1954) were both proponents of subjective probability, both also acknowledge the challenge of doing this in practice. This challenge is the impetus for robust Bayesian methods that explore the sensitivity analysis to subjective probability inputs. For instance, see the discussion in Berger (1984).

Models in economics and elsewhere derive their value in part from their simplifications or abstractions. They are necessarily wrong or equivalently misspecified along some dimensions. However, this observation by no means destroys their value. In economic applications this misspecification is often transparent, and we hope that it does not distort too much the answer to the questions we address. But the potential for *model misspecification* gives a third component to uncertainty, one that is perhaps the most difficult to address or quantify. Some of the more interesting attempts to address this challenge come out of the extensive literature on robust control theory. An example that I found to be particularly revealing and valuable in my own research is Petersen et al. (2000) where there is uncertainty about how to specify the probabilities for the outcomes of the random shocks. As I noted previously, following Slutsky (1927) and Frisch (1933), these random shocks are pervasive in modeling economic time series. Uncertainty about the probabilities of these random shocks includes an incomplete understanding of intertemporal dependencies in the constructed dynamic economic models. Recognizing the limitations of the existing models alters their prudent usage.

# 3 Who confronts uncertainty?

As I and others have argued elsewhere, I think of uncertainty from two vantage points, both of which are important in building, assessing, and using dynamic economic models.[3] One perspective is that of researchers who estimate some unknown parameters, just as Bernoulli envisioned, and they assess or test the model implications. I call this perspective of an external analyst as coming from *outside* of the models looking to evaluate them based on evidence or prior judgement. This is the typical vantage point of the discipline of statistics, and a rich array of methods have been developed with this in mind.

Economists' models include economic agents making decisions. For instance, investment decisions are in part based on people's views of the future possible benefits. Decisions on how much to produce when production takes time depends in part on perceived prices or economic rewards for selling the goods in the future. Once economic decision makers are included in formal dynamic economic models, their expectations come into play and become an important ingredient to the model as well as the uncertainties they confront.

[3] See Hansen (2014) for further discussion.

This challenge was well-appreciated by economists such as Pigou, Keynes, and Hicks. Thus, economic agents inside the models that economists build face challenges that bear similarity to those of statisticians. What are sensible ways to forecast the future, and how much confidence should we have in those forecasts?

When building models, while some researchers make simplistic connections to psychology, we make no pretense to capture all of the psychological complexities faced by individuals in different situations. We make bold simplifications to keep the analysis of the interdependent system tractable. An elegant pervasively used simplification is the imposition of rational expectations. This is an equilibrium construct that imposes model consistent beliefs on the individuals inside models. This approach was initiated within macroeconomics by Muth (1961) and Lucas (1972). Following Lucas' paper, in particular, rational expectations became an integral part of an equilibrium for a stochastic economic model. This approach makes the analysis of risk aversion tractable and provides an operational way to analyze counterfactuals using dynamic economic models. There is a direct extension of the rational expectations paradigm that includes unknown parameters or states confronted by economic agents using subjective probabilities and Bayesian learning. The rational expectations hypothesis and its extension to Bayesian learning abstract from ambiguity about subjective inputs and concerns about potential model misspecification.

A substantial literature has evolved on econometric implications of dynamic models with rational expectations with a variety of different implementations. One important line quantifies the impact of alternative shocks featured originally by Slutsky (1927) and Frisch (1933) to the macroeconomy by inferring these shocks from data and measuring how they are transmitted to the macroeconomy. An initial important contributor of this extensively used approach is Sims (1980). An empirical counterpart to rational expectations is implicit in much of this work as the shocks that are identified through econometric methods are also the ones pertinent to the economic system being analyzed. A complementary approach imposes more *a priori* structure on the underlying transition mechanisms while imposing rational expectations in deriving and assessing testable restrictions on the data generation. For instance, see Sargent (1973).[4] As featured in Hansen (2014), I along with several co-authors explored and applied a third approach aimed at studying part of a dynamic economic system while seeking to be agnostic about the rest.[5] Even though the implicit

[4]As I described in Hansen (2014), early in my career I also contributed to this line of research in collaboration with Sargent.

[5]These include John Cochrane, Robert Hodrick, Ravi Jagannathan, Scott Richard, and in particular Ken Singleton.

model of the economy was that of an interrelated dynamic system, it proved advantageous to have econometric methods that allow the researcher to "do something without doing everything." My own interest focused on the implied linkages between the macroeconomy and financial markets. The featured relations captured the forward-looking investment decisions of individuals and enterprises. This approach also imposed an empirical counterpart to rational expectations by, in this case, presuming the beliefs of the economic agents are consistent with historical time series data. Although not their original aim, empirical investigations, including my own, produced characterizations of empirical puzzles rather than confirmation of models. It pushed me and others to think harder about the potential for model misspecification and its consequences. If I, as a researcher, have to struggle in selecting good models of the economy, perhaps the people inside the models that I study face similar challenges. Thinking about uncertainty in broader terms became an attractive extension of the rational expectations perspective.

The perceived complexity of the economic environment alters how individuals make forward-looking decisions. This is self-evident from statistical decision theory and looks equally pertinent to external analysis as well as to the economic decision makers in the models we build. I find the tools of decision theory and statistics to be valuable in thinking about both challenges. It is easier to imagine behavioral anomalies persisting in complex environments in which model selection is known to be truly challenging even for sophisticated statisticians. There is a rather extensive literature on decision theory under uncertainty that draws on insights from economics, statistics, and control theory that are valuable guides for thinking through such issues. See Gilboa and Marinacci (2013) and Hansen and Marinacci (2016) for recent surveys. My own research and applications have found value from both the axiomatic approaches common in economics, the more practically-oriented control theory methods, and the insights from applied probability theory that feature characterizations of statistical complexity and resulting difficulties in learning from evidence. Decision theory provides two attributes relevant for building and using dynamic economic models. It gives a formal language to discuss decision-making in an uncertain environment, and it provides justifications for tractable ways to represent preferences to be used in formal statements of decision problems.

It is challenging to understand financial markets using the risk aversion model under rational expectations. Asset pricing theory informs us that it is the exposure to macroeconomic risks that requires market compensation. These are the risks that cannot be diversified by averaging over large cross-sections of exposures. The risk compensations are sometimes observed to be large and puzzling. Moreover, in some of the existing economic models, exposure to long-term macroeconomic risks can have even short-term consequences for financial markets.[6] Thus, the models implicitly impose a burden on investors inside the model to assign credible probabilities to events that will only be realized far into the future. Motivated in part by the empirical shortcomings that I mentioned previously, a literature is emerging that uses advances in decision theory to study the impact of uncertainty, broadly conceived, on market prices and the resulting outcomes. Adding in components of uncertainty other than risk provides a different perspective on this evidence. For instance, economists currently debate the possibility of a permanent secular stagnation in the macroeconomy whereby future growth rates will be on average smaller than past ones. The alternative views may be conceptualized as alternative models of the economy with uncertainty as to which of these views gives the best approximation. Uncertainty of this nature spills over to private sector investor decisions and financial market returns. A broad perspective on uncertainty adds a richness to how we capture investor behavior inside economic models. Investor struggles in the presence of ambiguity aversion or concerns with model misspecification aid our understanding of why financial markets reflect more caution in bad macroeconomic times than in good times.[7]

This more general perspective on uncertainty also provides a way to capture investor confidence. A fully confident investor may commit completely to a single model where a less confident investor may entertain multiple models with uncertainty as to how to weight them or suspect each of them to be at best, a coarse approximation (and therefore misspecified). Such a formulation could also be a way to introduce investor heterogeneity in economic models, heterogeneity that captures differences in how confident investors are in their views of the future. I next explore how a more sober perspective on uncertainty could enrich the analysis of prudent policy design.

# 4 Uncertainty and Policy

The connection between uncertainty and incomplete knowledge and the design of economic policy has long been discussed in informal ways. If structural econometric models are to provide quantitative inputs into decision-making, how will uncertainty alter how these models should be used as formal guides for policy-making? The impact of uncertainty has

[6]See, for instance, Bansal and Yaron (2004).

[7]For a recent illustration of this mechanism see Hansen and Sargent (2016).

been recognized by scholars but less so when economists play advisory roles. Indeed years ago when Hayek (1974) wrote on the pretense of knowledge, he warned of the dangers of trying to satisfy what the public seeks[8]

> Even if true scientists should recognize the limits of studying human behaviour, as long as the public has expectations, there will be people who pretend or believe that they can do more to meet popular demand than what is really in their power.

From my standpoint, there are two elaborations of these statements that intrigue me. First, I am inclined to think in terms of *uncertainty* in our understanding of human behavior and its economic consequences. Second, I am concerned about unproductive policies premised on a projected overconfidence in a particular model or perspective of the economic system.

Going further, I see at least two interrelated questions:

- Does incomplete knowledge or understanding of complicated policy problems enhance the appeal of simple solutions?
- How socially detrimental is complexity in policy implementation in light of the resulting uncertainties faced by the private sector?

Regarding the first question, with a complete and confident understanding of an interdependent complex economic system, we might well be led to embrace a complex policy to improve social well-being. How does this perspective change when our understanding is incomplete and does uncertainty or incomplete knowledge make simple solutions to complex problems more appealing?

Decades ago, Friedman (1961) made reference to "long and variable lags" in the mechanism by which money influences prices and the macroeconomy. He used this observation to argue for simple policy rules instead of more ambitious attempts at more subtle management of the macroeconomy. The reference to long and variable lags was a statement of skepticism about the knowledge needed to credibly implement a more complicated policy rule. Monetary policy is different now than when Friedman was writing, and some of Friedman's own perspectives on monetary transition mechanisms have since been challenged in important ways.[9] But Friedman's concern that there will be unproductive outcomes induced by overstating our understanding of a basic mechanism continues to be relevant

[8] See page 6.

[9] See, for instance, Sims (2012).

to current day macroeconomic policy-making. Learning more about the economic system potentially opens the door to more reliable policy levers; but there remains an important task to assess when the uncertainty is sufficiently resolved to justify a more finely tuned approach to the conduct of policy.

Regarding the second question pertaining to policy and complexity, part of the practical ramifications of complexity in the design of policy is to provide additional flexibility to policy-makers in their implementation. For instance, it might well be desirable that policy authorities have some discretionary powers in times of crisis or extreme events that were not appropriately planned for. But this same complexity burdens private sector as it is left guessing about implementation in the future. Counterproductive aspects of regulatory discretion are known from the important work of Stigler (1971) and others. A different twist on discretion occurs in the dynamic macroeconomic policy-setting. Kydland and Prescott (1977) characterize the repeated temptation for discretion along with the resulting adverse consequences relative to rule-based commitments. Both contributions are fundamental, but my interest in this essay is to add to this discussion by suggesting an interplay between complexity and uncertainty.

Going beyond these two questions, I find it both attractive and challenging to provide a more systematic analysis of uncertainty and its consequences for the design and conduct of policy. In what follows, I will talk briefly about two policy challenges for which I find a broad perspective on uncertainty to be revealing. No doubt each one deserves its own essay or more likely treatise; but let me at least place them on the radar screen of readers to provide some more specific context to my discussion.

## 4.1 Financial market oversight

The term "systemic risk" has shown up prominently in the academic literature and in discussions related to financial market oversight since the advent of the financial crisis. Prior to the crisis, the term was rarely used. Mitigating systemic risk is a common defense underlying the need for macro-prudential policy initiatives. How to design and implement such policies remains an open question. When it comes to systemic risk, perhaps we should defer and trust our governmental officials engaged in regulation and oversight to "know it when they see it," but this opens the door to counterproductive regulatory discretion and policy uncertainty.[10]

[10] Recall Justice Potter Stewart's treatment of pornography.

I have written previously on the challenges in identifying and measuring systemic risk.[11] There, I argue for thinking more broadly in terms of systemic uncertainty instead of the more narrow construct of risk. I exposit some of the many challenges that are pertinent to building quantitative models to support the conduct of macro-prudential policy. While I am an enthusiastic supporter of model development in this area, currently we face counterparts to Friedman's concerns about long and variable lags because of our limited understanding of the underlying phenomenon. People on the front lines of policy-making have also noted important limitations both in our understanding of systemic risk and in making it a guiding principle for financial oversight.[12] How best to provide governmental oversight of financial markets is arguably a hard and complex problem. Given limitations in our knowledge base, it is not at all apparent that a complex solution is the best course of action. Friedman's appeal for simple and transparent rules for monetary policy may be equally applicable to the design and conduct of macroprudential policy.

## 4.2 Climate economics

Federal agencies use estimates of the "social cost of carbon" to assess the climate impacts of various programs and regulations. Economists applaud cost-benefit analysis, and the aim to be numerate looks attractive. The current computations come from simulations from alternative models of the interplay between the climate and the economic system. There is a weighting across models, a reported sensitivity to the choice of a discount factor used in computing present values measures, an attempt to make probabilistic statements, and an acknowledgment of some omissions in the measure of climate damages. This all has the appearance of good quantitative social science in action. Unfortunately, the current calculations also abstract from some critical sources of uncertainty about the timing and magnitude of how human inputs influence the climate, and they run the danger of conveying a deeper understanding than truly exists.

Let me start with the basic construct. How useful is it to think of the social cost of carbon divorced from the benefits? How far can we push microeconomic reasoning without

[11] See Hansen (2012).

[12] See, for instance Haldane and Madouros (2012). In a speech on March 31, 2011 entitled *Regulating System Risk* Daniel Tarullo, United States Federal Reserve Board of Governors, argued: "There is also need for more study of the dynamics by which stress at large, interconnected institutions can have negative effects on national and global financial systems. In fact, what may be needed is a new subdiscipline that combines the perspectives of industrial organization economics with finance. Without work of this sort, it may be difficult to fashion the optimally strong, sensible, post-crisis regulatory regime."

thinking through the macroeconomic system-wide consequences? Thus, it is not clear to me conceptually what should be meant by the social cost of carbon net of benefits and system-wide implications. We can determine what is actually measured by opening the hoods, so to speak, of the models used to generate the computations. By so doing, there are at least partial answers to these questions.

But let's take a step back. First, while basic physical considerations play important roles in the construction of climate models, there are important gaps in the ability to translate these insights into reliable quantitative predictions. Second, once merged with economic components the carbon-temperature linkage is dramatically simplified for reasons of tractability with only limited understanding of the consequences of this simplification. Third, it is well known from the theory of asset pricing that there should be an important link between uncertainty and discounting when computing intertemporal valuations that balance off costs over time. Thus, the uncertain social impact of carbon in the future should alter the stochastic discounting of inputs used to measure the net social cost of carbon. While so-called local or small changes are amenable to stochastic counterparts to the discount formulations used in deterministic cost-benefit analyses, more global changes in policy require more comprehensive calculations.[13] These three points just scratch the surface of some truly important modeling challenges that climate scientists and economists continue to wrestle with.[14] Perhaps the most productive outcome of regulatory discussions of the social cost of carbon is the nurturing of future research in this important area rather than the actual reported numbers.

Acknowledging uncertainty and our limits to understanding does not imply a call for inaction. Depending on what aspect of the uncertainty we find to be most consequential to society helps us to better frame a discussion of policy-making in the future. The possibility of major adverse impacts can suffice for justifying policy responses such as carbon taxation or cap and trade. Even though we are uncertain as to the magnitude, timing and climate impacts from carbon emissions, this alone does not rationalize a wait-and-see attitude. Indeed it may well be less costly socially to act now than to defer policy responses to the future. Such tradeoffs are of critical importance to explore and are best done so cognizant of the limits in our understanding and uncertainty in our analyses.

[13]See Hansen et al. (1999) and Alvarez and Jermann (2004) for some illustrations of local welfare analysis from an asset pricing perspective.

[14]See Brock and Hansen (2017) for a more extensive discussion of uncertainty as conceived in this essay and the modeling challenges in climate economics.

## 5 How might decision theory contribute?

I have already discussed how decision theory targeted to broad notions of uncertainty helps us to understand better the behavior of financial markets. In concluding this essay, let me discuss how decision theory can also help shape discussions of prudent policy-making. Some of the insights from decision theory will appear to be self-evident and of little surprise, but the formalism is still of considerable value in both building and using models. Some examples of unsurprising insights include the following. When we are unsure about some modeling inputs, it makes good sense to perform a sensitivity analysis by computing the consequences of changing the inputs. The target of this analysis should be the potential consequences that the decision maker truly cares about.

A sensible decision or a good course of action is one that performs relatively well across a range of model specifications. When there are multiple models to consider and we are unsure on how to weight them, decision theory pushes us to ask what the consequences are of a course of action under each of the possible weightings of models that are entertained. Policies that work well under the alternative models become attractive even if they cease to be the best course of action under any of the specific models. Unless we have a compelling *a priori* way to weight models, caution or aversion to ambiguity translates into looking at the adverse consequences of alternative possible weighting schemes in evaluating alternative policies. Potential misspecification can be conceptualized similarly but places an extra and perhaps unwieldy burden in guessing the myriad of ways the models might be wrong. I am not claiming that this task is easy, but suggesting that it not be forgotten. As I have already mentioned, robust control theory has already wrestled with and produced some tractable and revealing ways to confront model misspecification in dynamic settings. The Hansen and Marinacci (2016) survey paper describes research that builds on some of the insights from control theory and incorporates them formally into decision theory and economic analysis.

Applying decision theory sharpens the questions and frames the analysis, but it is not a panacea that makes prudent decision-making necessarily easy. For instance, even with decision theory I am unaware of any general proposition linking incomplete knowledge of a social or economic problem and to desirability of a simple course of action. However appealing this link may seem, my guess is that justifying it formally may turn out to be context specific and may depend on the details of the actual policy problem.

The decision theoretic approach raises interesting challenges about how to communicate uncertainty in a policy realm. A robust statistician might just report ranges of potential probabilities for important outcomes that are computed by looking across alternative ways to weight model implications. This, as you might imagine, can quickly overwhelm the attempt to communicate uncertainty. For sufficiently nice decision problems, there are so-called "worst-case" weighting schemes that depend on the details of the decision problem, including the delineation of potential ways to weight the models that are of interest. By construction, the so-called robust course of action is actually the best course under this worst-case weighted family of models. This worst-case model reflects caution induced by adopting a broad notion of uncertainty. A fully-committed Bayesian would only entertain one such weighting scheme making the worst case calculation determined by data and priors over alternative models. The chosen robust course of action, however, would agree with that of a Bayesian fully committed to the worst-case weighting.

The worst-case prior over a family of models is not just subjectively determined. Its computation relies on the details of the decision problem and the resulting weighting is slanted towards models with adverse consequences for the decision maker. It is the result of the aversion to ambiguity or a concern about model misspecification. Reporting (constrained) worst-case computations, opens the door to claims of a biased treatment of the data. Indeed this claim is accurate, but purposefully so. The worst-case prior deliberately slants how models are weighted and is part of the output when solving a decision problem. It also understates that underlying uncertainty.

A policy advisor may be tempted to slant model choices along the lines of this worst-case weighting in order to defend a course of action. Conveying formally the worst-case weighting as a weighting scheme of particular interest may be too subtle for communication pertinent in the policy arena. Instead projecting views with great confidence is perhaps the easiest way to persuade policy makers and the public even when this confidence is not real. If only we had the requisite knowledge that allowed us to avoid such tricky issues and to embrace simple models with full confidence. Unfortunately, we are seldom that lucky. But naively ignoring uncertainty opens the door to ill-conceived policies that fail to deliver on their intended ambition.

# References


Alvarez, Fernando and Urban J. Jermann. 2004. Using Asset Prices to Measure the Cost of Business Cycles. *Journal of Political Economy* 112 (6):1223–1256.

Bansal, R. and A. Yaron. 2004. Risks for the Long Run: A Potential Resolution of Asset Pricing Puzzles. *Journal of Finance* 59 (4):1481–1509.

Berger, J. 1984. The Robust Bayesian Viewpoint. *J. Kadane., Amsterdam: North-Holland* 63–124.

Bernoulli, J. 1713. 1713, Ars conjectandi, opus posthumum. *Accedit Tractatus de seriebus infinitis, et epistola gallicé scripta de ludo pilae reticularis (Thurneysen Brothers, Basel, Switzerland)* .

Brock, W. and L. P. Hansen. 2017. Uncertainty and Climate Economic Models: Wrestling with Decision Theory. Tech. rep., University of Chicago.

de Finetti, B. 1937. La Prevision: Ses Lois Logiques, Ses Sources Subjectives. *Annales de l'Institute Henri Poincare'* 7:1 – 68. English translation in Kyburg and Smokler (eds.), *Studies in Subjective Probability*, Wiley, New York, 1964.

Friedman, M. 1961. The Lag in Effect of Monetary Policy. *Journal of Political Economy* 69 (5):447–466.

———. 1977. Nobel lecture: Inflation and Unemployment. *Journal of Political Economy* 85 (3):451–472.

Frisch, R. 1933. Propagation Problems and Impulse Problems in Dynamic Economics. In *Economic Essays in Honour of Gustav Cassel*, 171–205. Allen and Unwin.

Gilboa, I. and M. Marinacci. 2013. Ambiguity and the Bayesian paradigm. In *Advances in economics and econometrics: Theory and applications*, edited by D. Acemoglu, M. Arellano, and E. Dekel. Cambridge University Press, Cambridge.

Haldane, A. and V. Madouros. 2012. The Dog and the Frisbee. In *The Changing Policy Landscape*. Economic Policy Symposium.

Hansen, L. P. 2012. Challenges in Identifying and Measuring Systemic Risk. In *Risk Topography: Systemic Risk and Macro Modeling*. University of Chicago Press.

———. 2014. Nobel Lecture: Uncertainty Outside and Inside Economic Models. *Journal of Political Economy* 122 (5):945 – 987.

Hansen, L. P. and M. Marinacci. 2016. Ambiguity Aversion and Model Misspecification: An Economic Perspective. *Statistical Science* 31 (4):511–515.

Hansen, L. P. and T. J. Sargent. 2016. Prices of Macroeconomic Uncertainties witih Tenuous Beliefs. Tech. rep., University of Chicago and New York University.

Hansen, L. P., T. J. Sargent, and T. D. Tallarini. 1999. Robust Permanent Income and Pricing. *Review of Economic Studies* 66 (4):873–907.

Hayek, F. A. Von. 1974. The Pretence of Knowledge. Nobel Prize in Economics Documents 1974-2, Nobel Prize Committee.

Hurwicz, L. 1966. On the Structural Form of Interdependent Systems. In *Logic, Methodology and Philosophy of ScienceProceeding of the 1960 International Congress*, vol. 44 of *Studies in Logic and the Foundations of Mathematics*, edited by Patrick Suppes Ernest Nagel and Alfred Tarski, 232 – 239. Elsevier.

Keynes, J. M. 1939. Professor Tinbergen's Method. *Economic Journal* 49 (558-68).

Knight, F. H. 1921. *Risk, Uncertainty and Profit*. Houghton Mifflin Company.

Kydland, F. E. and E. C. Prescott. 1977. Rules Rather Than Discretion: The Inconsistency of Optimal Plans. *Journal of Political Economy* 85 (3):473–491.

Lucas, R. E. 1972. Expectations and the Neutrality of Money. *Journal of Economic Theory* 4 (2):103–124.

Muth, J. H. 1961. Rational Expectations and the Theory of Price Movements. *Econometrica* 29 (3):315–335.

Petersen, I. R., M. R. James, and P. Dupuis. 2000. Minimax Optimal Control of Stochastic Uncertain Systems with Relative Entropy Constraints. *IEEE Transactions on Automatic Control* 45:398–412.

Sargent, T. J. 1973. Rational Expectations, the Real Rate of Interest, and the Natural Rate of Unemployment. *Brookings Papers in Economic Activity* 4 (2):429–480.

Savage, L. J. 1954. *The Foundations of Statistics*. New York: John Wiley and Sons.

Sims, C. A. 1980. Macroeconomics and Reality. *Econometrica* 48:1–48.

———. 2012. Statistical Modeling of Monetary Policy and Its Effects. *American Economic Review* 102 (4):1187–1205.

Slutsky, E. 1927. The Summation of Random Causes as the Source of Cyclic Processes. In *Problems of Economic Conditions*, vol. 3. Moscow: The Conjuncture Institute.

Stigler, G. 1971. The Theory of Economic Regulation. *Bell Journal of Economics* 2 (1):3–21.

Stigler, S. 2014. Soft Questions, Hard Answers: Jacob Bernoulli's Probability in Historical Context. *International Statistical Review* 82 (1):1–16.