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REJOINDER: THE "AMBIGUITY AVERSION LITERATURE: A CRITICAL ASSESSMENT"

NABIL I. AL-NAJJAR AND JONATHAN WEINSTEIN
Northwestern University

The pioneering contributions of Bewley, Gilboa and Schmeidler highlighted important weaknesses in the foundations of economics and game theory. The Bayesian methodology on which these fields are based does not answer such basic questions as what makes beliefs reasonable, or how agents should form beliefs and expectations. Providing the initial impetus for debating these issues is a contribution that will have the lasting value it deserves.

But while honouring the originality of intellectual pioneers is quite proper, a literature should also periodically reexamine its central premises and not entirely succumb to the temptation to self-perpetuate. Our critique of the ambiguity aversion literature should therefore be viewed in this light: while recognizing the importance of the issues this literature attempts to model, we find its methodology wanting and far from representing a step forward. At best, this literature replaces the unsolved problem of how

We would like to express our gratitude to the Editor Giacomo Bonanno for organizing this special issue and giving us the opportunity to be part of the debate about so central a topic in modern economic theory. We also thank the authors of the replies for their thoughtful observations and criticisms, and Rakesh Vohra for organizing the workshop *Decision Theory and its Discontents* at Northwestern University, 30 April–1 May, 2009, where our paper and some of the replies were presented. In addition to the authors of the replies, we also benefited from critiques of our work in the presentations by Faruk Gul, David Schmeidler, Luca Rigotti, and Peter Klibanoff.
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agents should form beliefs with the more difficult problem of how to form a set of beliefs, a capacity, or a distribution over beliefs.

In commenting on our paper, David Schmeidler noted that theories do not get refuted, but embarrassed. To paraphrase, it is rare that a theory is instantly abandoned due to a single unassailable logical refutation. Instead, an accumulation of discomfiting evidence gradually causes it to fall out of favour. History suggests that any theory can be made, for a time, to survive such mounting evidence by modifying ancillary assumptions, appealing to convoluted explanations, or reinterpreting the evidence.

Our critique of the ambiguity aversion literature cannot ''refute'' this theory; all we can do is highlight some of its troubling consequences. We also argued that the increasingly sophisticated axiomatic models in this literature are unnecessary to explain available evidence and, in fact, confound rather than clarify the interpretation of such evidence.

In his reply, Marciano Siniscalchi notes that embarrassment is subjective and, since no logical errors are committed in the ambiguity aversion literature, the opinions we expressed need not compel anyone to "feel similarly ashamed". This may be so. We were the belief that the ambiguity aversion literature presents economists motivated by with an unusual challenge. On the one hand, terms such as ambiguity, robustness, model uncertainty and Knightian uncertainty sound attractive. On the other hand, this literature has evolved into a highly specialized craft that requires mastery of increasingly esoteric and daunting technical details. As we make clear in our paper, some of our examples and arguments are already known to insiders. Our aim was to present a unified case that the anomalies glossed over as isolated curiosities in this literature pose a fundamental challenge to its premises. We hope to thereby enable non-specialists to make a more informed decision of whether to share our views.

Thomas Kuhn famously compared the adoption of a paradigm to a "conversion experience". It is common for the faithful to develop an insular environment that makes light of mounting anomalies and outsiders' scepticism. Disciplines evolve when communities of scholars reach the conclusion that certain evidence is problematic enough to make a particular theory untenable. At various points in history, some of the brightest minds worked in areas of knowledge, such as astrology and alchemy, which are now dismissed as flawed.[1] Dismissal of these "theories" is usually based on recognizing their flawed premises and conclusions, rather than logical inconsistencies. We view the fact that some

[1] Newton, for instance, devoted greater effort to alchemy and theology compared to calculus, optics, mechanics and gravity. See, for instance, Gleick (2003).

of our points are known to insiders as further illustration of this literature's increasingly insular nature.

In the remainder of this rejoinder we proceed by answering the comments both in the articles in this volume and in the workshop and other presentations we have given of this work. As much as possible, we avoid repeating arguments already discussed in our paper.

BUT AMBIGUITY AVERSION MODELS FIT THE DATA!

We do not deny static Ellsberg choices as a robust behavioural pattern. What we question is the ambiguity aversion literature's ability to articulate a clear case as to why its "explanation" of these choices is superior to alternatives.

Since ambiguity aversion models were designed to fit the static Ellsberg choices, it is not surprising that they succeed in doing so. But there are potentially infinitely many models that can explain the same evidence, including Fox and Tversky (1995), Heath and Tversky (1991) and, more recently, Halevy and Feltkamp (2005) and Gottlieb (2009), among many others. In our paper we proposed an explanation based on decision makers following heuristics. The role of heuristics in explaining behavioural "anomalies" has been recognized by many writers, including Tversky and Kahneman (1974), Aumann (1997), Samuelson (2001), to name a few. The point here is that any collection of empirical findings is consistent with many possible theories.[2]

Our opening question still stands: *why is ambiguity aversion superior to the potentially infinite number of alternative explanations of Ellsberg choices?* A possible answer is that ambiguity aversion models provide a theoretically well-founded, axiomatic approach to modelling these choices. But this is of value only if the resulting models are robust enough to withstand simple "stress tests" like the ones we subject them to. As our examples demonstrate, these models break down under the stress of simple variations of the static Ellsberg problem where decision makers face slightly modified information structures in which ambiguity is resolved gradually.

RECURSIVE MODELS AS A WAY TO OBSCURE THE ISSUES

In his reply, Sujoy Mukerji writes:

> Ambiguity sensitive/non-probabilistically sophisticated preferences can be extended to dynamic frameworks, preserving both dynamic consistency and consequentialism (indeed, monotonicity). [...] The sunk cost fallacy is not

[2] This is known as the Duhem–Quine thesis, stating that theories are under-determined by empirical evidence.

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> possible in any of these preference models. [...] It is certainly not a critique that may be applied to ambiguity sensitive static preferences, nor to dynamic extensions which respect consequentialism, as several prominent extensions of ambiguity sensitive preferences do.

The flawed nature of non-fact-based updating (i.e. failure of consequentialism) is discussed at length in our paper and in Siniscalchi's reply (this issue). The inadequacy of limiting attention to "ambiguity sensitive static preferences" is discussed below. Here we focus on Mukerji's remaining claim, which concerns recursive models.

In our paper, we discuss at length the best known example of recursive models, namely Epstein and Schneider's (2003) paper. We argued that their approach is flawed because it fails a basic test of a coherent theory of dynamic choice, namely to inform us about how choice behaviour changes in response to slight variations in the information structure. In our paper, we cite two other recursive models, those of Maccheroni, Marinacci and Rustichini (2006) and Klibanoff, Marinacci and Mukerji (2009). We pointed out that they suffer from the same problems, but we did so only briefly to avoid unnecessary repetition.

Our detailed discussion of Epstein and Schneider's work may have left the false impression that our critique is valid only for their specific model when, in fact, it applies to recursive models in general. Consider for example the Klibanoff–Marinacci–Mukerji recursive, infinite-horizon model. The conclusion of the model is a recursive formula for the value function taking the form:

$$V_{s^t}(f) = u(f(s^t)) + \beta\phi^{-1}\left[\int_{\Theta} \phi\left(\int_{\chi_{t+1}} V_{(s^t, x_{t+1})}(f)\, d\pi_\theta(x_{t+1}; s^t)\right) d\mu(\theta \mid s^t)\right]$$

Here, f is an infinite horizon intertemporal plan; β is a discount factor; u is the one-period utility function; s^t is a partial history; x_{t+1} is the outcome at time $t+1$; and π_θ and μ are first-order and second-order beliefs, respectively. The reader should consult their paper for more detailed explanations and interpretation.

Complicating the model by considering an infinite-horizon problem obscures rather than clarifies the issues. Passage to infinite horizon seems hardly the right approach to solve problems that are already intractable in simple two-stage problems.

To clarify the issues, all one needs to consider is a simple example where each stage consists of a three-colour Ellsberg urn. Restrict attention to the four memory-less plans "always choose f_i", where

	$\underline{b}$	$\underline{r}$	$\underline{y}$
f_1:	10	0	0
f_2:	0	10	0
f_3:	10	0	10
f_4:	0	10	10

as in our paper. In this simple example, although the setting is dynamic, it consists of little more than stringing together a sequence of identical static problems.

Suppose now that we perturb the information structure by introducing a subperiod in which it is revealed whether the ball is yellow or not, and enrich the set of plans to allow the decision maker to take advantage of the new information. This brings us back full circle to the sort of examples discussed in our paper, and the problems plaguing updating in this literature reappear. The recursive model above rules out information structures where ambiguity is partially resolved. This is done through a subtle combination of assumptions. Assumption 7 states that the decision maker is ambiguity-neutral when it comes to uncertainty about θ, the only parameter linking the different time periods. This means that all ambiguity is within a single period. Assumptions 5 and 6 imply that decisions made at any given time t only interact with the uncertainty revealed at time t and not with any other time t' (this is also clear from examining the representation above). Taken together, these assumptions imply that this recursive model is not a dynamic model of ambiguity at all, but instead a model of a series of static ambiguity problems, linked dynamically only by an ambiguity-neutral overall preference. By assuming the problem away, the recursive model is silent on what a decision maker should do in natural settings, like those in our examples, where ambiguity is partially resolved. Settings like these are simply ruled out by fiat.

To sum up, the problem of updating in the ambiguity aversion models is fundamental in nature; an infinite horizon extension obscures rather than clarify the issues. The recursive models, like their non-recursive counterparts, fail to answer to the basic question: *what should the decision maker do if he faces partial resolution of ambiguity?* Instead, they adopt the approach of ruling out natural and important settings, focusing instead only on environments where their desired conclusions hold.

DYNAMIC CHOICE IS NOT OPTIONAL

The idea that it is somehow good enough to deal only with static choice is ill-considered. Without the ability to handle dynamic choice, it is difficult to imagine what use the ambiguity aversion models will have in game theory and information economics. The problem we point out in our paper may explain why, 20 years after the first axiomatizations of ambiguity aversion, this literature's contribution to game theory and information economics consists of little over a handful of papers. This is despite the fact that ambiguity, in principle, ought to be of even greater significance in settings with strategic interactions and incomplete information. The problem is that game theory and information economics fundamentally rely on dynamic choice, thus posing insurmountable challenges to the ambiguity aversion methodology.

AVERSION TO INFORMATION VS. DESIRE FOR COMMITMENT

Siniscalchi (in this volume) argues at length that we have committed a misinterpretation by characterizing a sophisticated ambiguity-averse individual as "averse to information", when his behaviour rather just shows a "desire for commitment". In fact, these two aspects of the decision in question are clearly complementary: the decision-maker avoids information precisely because he cannot make a commitment as to how to use it. We fully appreciated the commitment aspect, but had several important reasons to emphasize aversion to information:

1. The inability to commit is not directly observable from behaviour; it involves the *ex ante* preferences which, as Siniscalchi points out, do not lead directly to behaviour. On the other hand, aversion to information is directly observable; we see the decision maker make a choice which leads to an otherwise identical scenario where she receives less information.
2. We find the inability to commit in this context psychologically implausible. The decision maker in question is sophisticated enough to perceive her dynamic inconsistency. She knows that it is caused by the updating rule. So why continue to use this updating rule? In the typical Strotz model of temptation, the decision maker is endowed with an *ex post* preference which is dynamically inconsistent with her *ex ante* preference. All of the standard examples involve cases where uncontrollable temptation is a compelling psychological phenomenon, such as addiction, an irresistible desire for dessert, and so on. It is easy to imagine a decision maker who is indeed endowed with these preferences as an immovable constraint on the ability to commit, and cannot simply wish them away. The case of updating rules for ambiguity preferences is different. While there is strong evidence that addiction and temptation are hardwired into the brain, it would be fanciful to claim the same for the updating rules in the ambiguity

aversion literature. A reflective decision maker would be able to change the updating rule if desired. In any case, a validation of sophistication on descriptive grounds would require compelling empirical evidence about off-the-path preferences, something which seems unlikely to obtain.

3. In addition to its weakness as descriptive model, sophistication is not normatively compelling either. Siniscalchi proposes that we should think of true preferences as those someone would hold on reflection. By this criterion, the normative case for violating dynamic consistency is very weak indeed. Imagine a consultant proposing to a client, or a class of MBAs,

   > In the best of all possible worlds, you would go up at the first node and then right at the second. But once you go up, my algorithm would force you to go left, so you had better go down after all.

   How would such advice be received? We suggest a reflective decision maker will take this convoluted advice by the consultant as a reason to reject ambiguity aversion and reassess his model of the problem he is facing.

To sum up, Siniscalchi argues that we are simply expressing a preference for dynamic consistency. True. We think it is self-evident that one should maintain dynamic consistency, barring a compelling reason to relax it. Our paper argues thoroughly that the ambiguity aversion literature fails to provide such a reason.

AMBIGUITY VS. AMBIGUITY AVERSION

We are in agreement with Nehring's point (this volume) about the need for a careful separation between ambiguity and ambiguity aversion. At an intuitive level, "ambiguity" refers to the inability of a decision maker, for whatever reason, to form a probability judgment about events. With this interpretation of ambiguity as "probabilistic ignorance",[3] the challenge we face as modellers is two-fold. First, to address the question: what are the behavioural implications of ambiguity? Second, to develop formal models that characterize ambiguity-driven behaviour and explain the sources of such probabilistic ignorance. There are a number of models that attempt to do so, most notably Bewley's (1986, 2002) incomplete preference approach.[4]

Our critique is specific to the ambiguity aversion models. On the one hand, the Ellsberg choices do not constitute an interesting behavioural

[3] A term we owe to Nehring, private conversation.
[4] Al-Najjar (2009) provides a model of "statistical ambiguity" based on learning.

manifestation of ambiguity. At best, they confound the issue of ambiguity with heuristics that are better understood in terms of game theory and bounded rationality. At worst, they express paranoia that can be more fruitfully (and simply) studied as part of behavioural economics. On the other hand, the formal models in the ambiguity aversion literature rely on taste to fit observed behaviour, offering no substantive insights into *why* decision makers cannot form probability judgments. All in all, these models offer a needlessly complex way to fit simple behavioural phenomena, with little to show for this complexity.

THE GAME-AGAINST-NATURE INTERPRETATION

A common argument is that the game-against-Nature interpretation of ambiguity aversion models is just a representation of preferences. This defence misses the point that an important role of representations is to help us clarify the reasoning and motives underlying behaviour. The fact that a decision maker behaves as if he is playing against a malevolent Nature that is out there to get him is an important fact both for interpreting this behaviour and in providing normative advice.

The game-against-Nature interpretation is neither new nor controversial: (1) it naturally follows from the ambiguity aversion axiom; (2) it has been pointed out by numerous authors including, more recently Maccheroni, Marinacci and Rustichini (2006), and (3) it appears in many of the functional forms in this literature. In a recent paper, Cerreia *et al.* (2008) showed that *any* ambiguity averse preference can be thought of in terms of playing a zero-sum game against a malevolent Nature. To be clear, their paper is not necessary to justify interpreting ambiguity aversion as the result of the decision maker believing Nature is out to get him. Rather, their contribution to this line of argument consists of showing precisely which game the decision maker believes he is playing.

ARE AMBIGUITY AVERSION MODELS GOOD PARSIMONIOUS REPRESENTATIONS?

A common defence of the ambiguity aversion models is that they provide a parsimonious reduced-form to model complex underlying games, information problems or learning processes. Early insightful contributions by Gilboa and Schmeidler (1994) and Mukerji (1997) make this interpretation precise. We recognize the value of reduced-form models, provided their limitations are clearly understood. Most questions in economics and game theory are about comparative statics. That is, to understand how behaviour changes as constraints like prices, wealth, technology, timing of moves, beliefs and information change. A parsimonious reduced-form of an economic problem is useful to the extent that it preserves enough of the underlying fundamentals to answer the comparative static questions of interest.

By this criterion, the failure of the ambiguity aversion models to account for dynamic choice is both predictable and irremediable. While they may be adequate reduced-form representations of the static choices, dynamic choice is an entirely different matter. In a game, a player's response to information depends on the structure of the game, including his beliefs, available actions, the game tree, and so on. The ambiguity aversion models are hopelessly at a loss in accounting for how a decision maker responds to new information because the structure of the underlying game is abstracted away. Accounting for dynamic behaviour in these models then runs in circles as each attempt to introduce elaborate axiomatizations to "reconstruct" how the decision maker should incorporate new information generates a fresh set of problems. Compromises, anomalies, and absurd conclusions inevitably follow as one tries to achieve the impossible, namely to model behaviour in a game, but without taking into account the game itself.

In an interesting paper, Ozdenoren and Peck (2008) illustrate this point formally. Ozdenoren and Peck present alternative games against nature that vary according to the timing of moves by the player and the malevolent nature. They show that various updating rules emerge as backwards-induction outcomes depending on the game being played. For example, a sophisticated response to preference reversals arises if the subject considers it possible that the urn will be manipulated by nature at more than one turn. This approach seems to us more straightforward than the roundabout axiomatics of this literature.

AMBIGUITY AVERSION IS JUSTIFIED BECAUSE WE DO NOT KNOW THE TRUE DISTRIBUTION

The title of Gilboa, Postlewaite and Schmeidler's response (this issue, hence forth GPS) is:

> Is it always rational to satisfy Savage's axioms?

We find that in part of their discussion they confound this question with the very different question:

> Are all those who satisfy Savage's axioms always rational?

To the latter question we do not hesitate to agree with them that the answer is no. The axioms are intended as a minimal standard of rationality – as necessary, but not sufficient. For example, GPS mention that although any belief satisfies Savage's axioms, they would find it irrational to believe, in the face of the evidence, that smoking does not impair one's health. So would we. This example suggests that in order to fully capture rationality (in particular, to link beliefs with evidence), additional axioms are needed which restrict the family of preferences characterized by Savage. Their argument loses its way when they use this example to espouse models which take away axioms and thereby expand the set of allowable preferences. In particular, none of the ambiguity aversion models would forbid a set of beliefs all of which assign zero probability to a link between smoking and cancer.

GPS are actually more concerned with examples on the opposite side of the spectrum, where evidence is incomplete, or completely absent; these examples question the *necessity* of Savage's axioms for rationality, in line with GPS's title. Their most extreme example, which we also find a useful thought experiment, is of someone forced to consider whether "all cydophines are abordites" without knowing the meaning of either of these words. We agree with them that it would be silly to think that an arbitrary number, such as 50%, represents "the" probability of this event. One who applies Savage's theory makes no such claim, however; he simply makes decisions involving this event using certain weights which we call his subjective probability. Furthermore, there is certainly nothing to prevent him from having in mind that these weights are provisional and will change when he learns the meaning of the words, although this will not enter into his immediate decision.

True, Savage's theory offers no guidance on how to pick the weights, leaving our decision maker a bit stranded; but it is crucial to realize that more general decision models leave him equally stranded, or more so, since he must pick a set of probabilities, or a capacity, out of thin air.

In real life, we would base our beliefs in response to an odd question about cydophines and abordites mostly on the psychology and incentives of the individual who posed it. Formal decision models – of any kind – are unlikely to help much with this rather rough process. Importantly, ambiguity models are, if anything, less helpful than Savage's. Similar comments apply to GPS's less extreme examples of uncertainty, such as the event that the US President six years from now is a Democrat. While we do not claim that any number is the probability of this event, we do consider it rational, for purposes of immediate decisions, to represent our uncertain knowledge of the event with a single number–provisional, and subject to updating. This single probability is no more arbitrary than any set or capacity we might pick, and it does have the advantage of following Savage's consistency principles.

In sum, we feel that GPS have attacked a straw-man version of Savage's theory, in which the decision maker holds a single probability measure, not as a mere aid to consistent decision-making, but in the conviction that this measure holds some platonic status of universal truth. We want to make it very clear that we do not back this version, and neither does Savage.[5] We readily agree that in most cases, Savage's axioms are not sufficient for rationality, as they offer no guidance as to how to pick a distribution; we part company drastically with the idea that the situation is improved by relaxing the axioms.

Conspicuously absent from GPS's examples of real-world uncertainty is any argument that the belief-type objects in ambiguity models can be picked by less arbitrary means than the single distribution in Bayesian models. We find urn-based examples very deceptive when it comes to this issue; they provide a rare and unrealistic case in which there is a natural set of distributions to use. Perhaps GPS would argue that errors in picking a set of distributions will be less catastrophic than errors in picking a single distribution. This claim is near-impossible to evaluate, both because the objects in question are of different dimension and because as of now we have no specification of how either is picked. We certainly don't think it is obvious that picking a higher-dimensional object will lead to fewer mistakes.

"IF IT IS POPULAR, THEN IT MUST BE TRUE"

Standing in the way of a popular trend always presents a tough challenge. The ambiguity aversion literature has been a runaway success by all objective measures: number of published papers, citations, and outstanding scholars it attracted. Surely, 20 years of intense academic efforts cannot be all wrong!

At the end, all we can offer is arguments and examples. Economists, decision theorists and game theorists will ultimately decide the fate of this literature, whether it will be a lasting part of economic theory, or a bubble like others.

Economic theorists are familiar with bubbles both in economic environments and within their own field. While no one can definitively answer Nehring's question (this volume) of whether this literature is a bubble, we can gain some insight from familiar models of informational cascades.[6]

These models offer clues about how cascades can arise as a rational response to lack of information. Think of a new theoretical framework, like ambiguity aversion, as a new technology which economists may

[5] This point is discussed at length in several sections of Savage (1954). For instance, see page 7: "We must be prepared to find reasoning inadequate to bring about complete agreement [in subjective probabilities]."

[6] For instance, Bikhchandani, Hirshleifer and Welch (1992).

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consider adopting. There is considerable uncertainty about the quality of the new technology: are the models tractable and methodologically sound, do they lead to paradoxes, and so on. A typical economist can make only a noisy assessment about the quality of the new theory given its complexity (understanding what the axioms mean, and how to integrate the functional forms within economic models). Finally, past adoptions (e.g. publications and citations) are available for everyone to witness at little cost.

As is well-known, these forces may well lead decision makers to ignore their own signals (e.g. their doubts about the validity of the model) and join the trend instead. A remarkable aspect of informational cascades is that massive adoption movements may contain very little information. The entire movement may be driven by a few key early adoption decisions, rather than the intrinsic value of what is being adopted. Another remarkable fact is how suddenly cascades can be reversed. This happens precisely because they are based on so little information.

REFERENCES

Al-Najjar, N. I. 2009. Decision makers as statisticians: diversity, ambiguity and learning. *Econometrica.* Forthcoming.

Aumann, R. J. 1997. Rationality and bounded rationality. *Games and Economic Behavior*, 21(1–2): 2–14.

Bewley, T. 1986. Knightian decision theory: Part I. Cowles Foundation Discussion Paper no. 807.

Bewley, T. 2002. Knightian decision theory. Part I. *Decisions in Economics and Finance* 25(2): 79–110.

Bikhchandani, S., D. Hirshleifer and I. Welch 1992. A theory of fads, fashion, custom, and cultural change as informational cascades. *Journal of Political Economy* 100(5): 992.

Cerreia, S., F. Maccheroni, M. Marinacci and L. Montrucchio 2008. *Uncertainty Averse Preferences*. Working paper; Collegio Carlo Alberto.

Epstein, L. and M. Schneider 2003. Recursive multiple-priors. *Journal of Economic Theory* 113: 1–31.

Fox, C. and A. Tversky 1995. Ambiguity aversion and comparative ignorance. *Quarterly Journal of Economics* 110: 585.

Gilboa, I. and D. Schmeidler 1994. Additive representations of nonadditive measures and the choquet integral. *Annals of Operations Research (Historical Archive)* 52(1): 43–65.

Gleick, J. 2003. *Isaac Newton*. Vintage Books.

Gottlieb, D. 2009. *Imperfect memory and choice under risk.* Cambridge, MA: MIT Press.

Halevy, Y. and V. Feltkamp 2005. A Bayesian approach to uncertainty aversion. *Review of Economic Studies* 72(2): 449–66.

Heath, C. and A. Tversky 1991. Preference and belief: Ambiguity and competence in choice under uncertainty. *Journal of Risk and Uncertainty* 4(1): 5–28.

Klibanoff, P., M. Marinacci and S. Mukerji 2009. Recursive smooth ambiguity preferences. *Journal of Economic Theory* 144: 930–79.

Maccheroni, F., M. Marinacci and A. Rustichini 2006. Ambiguity aversion, malevolent nature, and the variational representation of preferences. *Econometrica* 74: 1447–98.

Mukerji, S. 1997. Understanding the nonadditive probability decision model. *Economic Theory* 9(1): 23–46.

Ozdenoren, E. and J. Peck 2008. Ambiguity aversion, games against nature, and dynamic consistency. *Games and Economic Behavior* 62(1): 106–15.

Samuelson, L. 2001. Analogies, adaptation, and anomalies. *Journal of Economic Theory* 97(2): 320–66.

Savage, L. J. 1954. *The foundations of statistics*. New York: John Wiley & Sons Inc.

Tversky, A. and D. Kahneman 1974. Judgment under uncertainty: heuristics and biases. *Science* 185 4157): 1124–31.