# Comment

**John W. Pratt**

Distressingly much of this paper strikes me as a regressive exercise in overliteral misreading and straw battle. Its main positive message was being advanced by Howard Raiffa and Robert Schlaifer when I joined forces with them 25 years ago, and our joint paper of 1964 said:

> . . . we consider the problem faced by a person who on most occasions makes decisions intuitively and more or less inconsistently, like all other mortals, but who on some one, particular occasion wishes to make some one, particular decision in a reasoned, deliberate manner. . . . [We have] avoided any reference to the behavior of idealized decision makers all of whose acts are perfectly self-consistent; instead, we have taken a strictly "constructive" approach to the problem of analyzing a single problem of decision under uncertainty, hoping thereby to dispel such apparently common misconceptions as that a utility function and a system of judgmental probabilities necessarily exist without conscious effort, or that they can be discovered only by learning how the decision maker would make a very large number of decisions.

We viewed others' work and its relation with ours quite differently from Shafer, however, titling our paper "An Elementary Exposition" and saying,

> . . . the sophisticated reader will find nothing here that he does not already know. We hope, however, that the paper will help some readers to a better understanding of the foundations of the so-called "Bayesian" position.

In contrast, I consider Shafer's tone and connotations highly misleading, especially his claims to refutation and radical revision, but your effort would be ill spent in reading a detailed disquisition thereon. Better to reread Savage with your own eyes and mind open, not through Shafer's filter or a second filter of mine. To suggest my disagreement, a set of statements contrary to what Shafer implies and a few general conclusions should suffice.

1. No sensible person ever really thought that probability and utility assessments preexist in anyone's mind, or that probabilities of all events could or should be assessed directly and then checked for consistency, or that they would be naturally consistent. Anyway, that horse is long dead.

*John W. Pratt is Professor, Graduate School of Business Administration, Soldiers Field, Harvard University, Boston, Massachusetts 02163.*

2. Hypothetical acts facilitate deciding sometimes and theorizing always. But if you can reach a Bayesian decision without considering hypothetical acts, or ranking all real acts, it is obviously a good idea, and legitimate by anyone's rules, to do so.

3. If your procedures or decisions or feelings are intransitive or otherwise discordant with subjective expected utility, they are incoherent, "irrational," or whatever you want to call it, and trying to justify them as coherent or find other rationalities is a waste of time.

4. The point of defining probability and utility in terms of hypothetical bets is to give them an unmistakable, concrete or operational meaning, but you may assess them however you like.

5. When your concern is scientific inference, as Savage's mainly was, the processes, psychological difficulties, and precise results of subjective assessment are of relatively little interest. If the posterior distribution is sensitive to the choice of prior, you need more data, not alternative modes of inference.

6. Defining consequences as everything you care about—disentangling values from beliefs—is essential not only to the meaning and acceptability of the axioms, but also to any kind of clear thinking or communication about decision making under uncertainty. Reasonable people may prefer different decisions in the "same" situation because they value even deterministic consequences differently, because they hold different beliefs about the uncertain world, or both. Entangling these sources of difference only confuses matters. Criteria that attempt to do without beliefs (such as .05, minimax, or their relatives) have failed as normative rules, whatever their ad hoc or other virtues. Any model with state-dependent consequences can be simply transformed into an equivalent one having state-independent consequences with no increase in complexity and, if the Bayesian axioms are in doubt, great increase in clarity.

7. In Allais' problem, it is indeed possible that your regret at receiving 0 instead of $500,000 is greater if you could have guaranteed yourself $500,000 than if not. Then 0 is an inadequate definition of the consequence: regret also needs to be incorporated (Bell, 1982), even though some hypothetical acts will then be hard to imagine. Similar comments apply if the objectivity of your chance at a prize affects your pleasure in winning or pain at losing, as in Ellsberg's example.

8. Regarding the sure thing principle (or independence postulate, substitution principle, or mixing

argument), methinks so much protest—here and elsewhere—signifies the futility of the search for a weak link in the Bayesian argument. A theory which does not expect a coherent decision maker to stick to a strategy chosen in advance will certainly be unattractive for everyday normative use, if not chaotic. Defining consequences inadequately clouds the argument but does not refute it.

9. Savage (*Foundations*, Section 5.5) explicitly recognized that a small-world consequence depends on grand-world decisions, probabilities, and more fundamental consequences.

10. On one strictly peripheral point I disagree with both Savage and Shafer: people are regularly taken in by pseudomicrocosms that focus on one risk when others, even negatively correlated ones, are present but unmentioned. For example, to someone negotiating for the right to use a patented production process, a fixed payment may seem less risky than royalties, but the picture reverses when profits are looked at, because higher sales accompany higher royalties.

## CONCLUSIONS

Talking about the behavior of a mythical ideally consistent person may still be the best way to convince people—and many still need convincing—that subjected expected utility is uniquely normative. Resisting this idea plays only a regressive role, and obstructs a sound understanding and appraisal of alternative tools. The Bayesian view helps one to distinguish what's important, trivial, ad hoc, fundamental, nonsensical, misleading, irrelevant, or misguided in areas of statistics from sequential stopping to ridge regression to hypothesis testing to unbiased or parameterization-invariant estimation. In problems of decision and inference under uncertainty, other arguments may sometimes be simpler and good enough, but they are never more cogent.

No new rationality has found widespread acceptance since Savage, nor should have. It is no revision of rationality to adopt short cuts, approximations, or even deliberate irrationality according to taste and circumstances, or to recognize that the main concerns often lie elsewhere. Other routes to Bayesian rationality may have advantages, but once it is accepted, even with amendments, the jig is up and the rest is tactics (or strictly for philosophical specialists).

Read literally, Shafer does not contradict most of my numbered remarks. But if he accepts them, and accepts that they are far from novel, what does all his sound and fury signify? If he does not, we live in different worlds.

I am sorry to sound so nasty. For some reason, statisticians who work in the foundations of the field often seem nicer in person than in writing. Shafer does, and I hope I do too.

## ADDITIONAL REFERENCES

BELL, D. E. (1982). Regret in decision making under uncertainty. *Oper. Res.* **30** 961–981.

PRATT, J. W., RAIFFA, H. and SCHLAIFER, R. (1964). The foundations of decision under uncertainty: An elementary exposition. *J. Amer. Statist. Assoc.* **59** 353–375.

# Rejoinder

**Glenn Shafer**

The main thesis of my article was that Savage did not establish the unique normativeness of subjective expected utility. It appears that three of the commentators, Robin Dawes, Phil Dawid, and Peter Fishburn, agree, while two, Dennis Lindley and John Pratt, disagree. In my rejoinder, I will concentrate on this central issue of normativeness. I will also respond, briefly, to the question about alternatives to subjective expected utility.

Fishburn gently notes that aspects of my constructive viewpoint are not altogether new. He adds that the idea of using subjective expected utility constructively was not altogether absent from Savage's own thinking. The points could be put more strongly. My viewpoint has, I hope, all the triteness of common sense. Common sense and historical perspective also tell us that Savage, like everyone else, expected to use subjective expected utility in the constructive direction, from probabilities and utilities to preferences between acts.

One aspect of my constructive viewpoint is the idea that one deliberately compares a problem to a scale of canonical examples involving chance. This aspect is scarcely new. It can be found in Bertrand (1907, page 26) and in Ramsey (1931, page 256). Pratt, Raiffa, and Schlaifer (1964) very effectively incorporated it into their alternative axiomatization of subjective expected utility.

I did not venture, in my article, to survey the many alternative axiomatizations of subjective expected utility that have followed Savage's. Had I done so, I would have had an opportunity to agree with the widespread opinion that Pratt, Raiffa, and Schlaifer's is the most attractive of these. Making explicit the