possible. We demonstrate that an upper bound for the ratio $C_{max}(\text{LPT*})/C_{max}(S^*)$ is at least 10/9 and that an upper bound for the ratio $C_{max}(\text{LPT**})/C_{max}(S^*)$ is at least 6/5. For LPT*, one lower bound on a worst-case performance ratio is attained with the following example. Choose $\bar{p} = \{0, 0, 2, 2, 3, 4, 4, 5, 6\}$ so that $C_{max}(\text{LPT*}) = 10$ and $C_{max}(S^*) = 9$. Thus, if $C_{max}(\text{LPT*})/C_{max}(S^*) \leq r$ for all instances when $r \geq 10/9$. The following example shows that there are instances for which the ratio $C_{max}(\text{LPT**})/C_{max}(S^*) \geq 6/5$. Consider $\bar{p} = \{0, 0, 2, 2, 4 - \epsilon, 4 + 2\epsilon, 4 + 2\epsilon, 6 + 3\epsilon, 8 + 2\epsilon\}$ where $\epsilon > 0$. Then $C_{max}(\text{LPT**}) = 12 + \epsilon$, whereas $C_{max}(S^*) = 10 + 4\epsilon$. Letting $\epsilon \to 0$ gives us the desired example.

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COMMENT ON "A RANDOMIZATION RULE FOR SELECTING FORECASTS"

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Foster and Vohra's (1993, this issue) technique for randomly selecting a forecast is intriguing because of its generality. No distributional assumptions, no restriction on the nature of the error function other than boundedness. However, the generality, coupled with their definition of "better than," leads to a counterintuitive and not very useful decision rule.

I begin from the premise that the decision maker who would implement Foster and Vohra's approach is thoughtful and desires to use available information (forecasts A and B) in the best possible way. In fact, the decision maker evidently has defined the error function in a way that is consistent with the decision problem at hand; in essence, a utility function has been assessed. Given this, it seems inconsistent to choose the forecast whose average error converges—in probability over the long run—to a level at least as small as that of any other forecast. Such a thoughtful decision maker should be choosing a forecast on the basis of which one is expected to be better over an appropriate planning horizon such as tomorrow or next year. If a thoughtful decision maker can carefully define the error function for use in the Mixing Method, surely he or she can also determine an appropriate planning horizon and should be skeptical of a definition of better that relies on convergence in probability as n goes to infinity.

If our thoughtful decision maker has gotten this far, how should he or she proceed? The convention is to invoke the standard of maximizing expected utility. In this case, it means choosing a forecast with the minimum expected error. Foster and Vohra eschew this approach on the grounds that it requires assumptions about the probability distribution of the errors,

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including stationarity, which rarely seem to hold. In so stating, though, they misrepresent the expected-utility approach. The issue is not to figure out what the "real" underlying process is. The decision maker's problem is to model his or her beliefs regarding the uncertainty that surrounds the pertinent future forecast errors. This is best viewed as a problem of assessing a subjective probability distribution to represent the decision maker's beliefs. Indeed, the decision maker may need to do some "modeling" or invoke some assumptions, especially if past data are to be used in the assessment. Nevertheless, even if we leave the decision maker with considerable flexibility, he or she may find it quite appropriate to impose typical regularity conditions on the error distribution, thereby ignoring many or all of the "perverse and pathological" distributions which Foster and Vohra must consider.

An important difference between Foster and Vohra's approach and subjective expected utility is that expected utility requires the decision maker to make and use appropriate subjective judgments. In fact, decision analysts often argue that such judgments are critical for good decision making. For forecast selection, the result is intuitive and readily defensible: Choose the forecast that is expected to be better, in the sense of the defined error function, over the appropriate time horizon. Given that there are no strategic issues involved, no randomization scheme would be strictly preferred to the preferred individual forecast.

On the other hand, Foster and Vohra, having required the decision maker to define the error function, now want either to save the decision maker the trouble of making the probability judgments and calculations, or to provide an all-purpose tool that can be used by anyone in any situation. Both are laudable objectives. However, their generality leaves them with a result that, while mathematically intriguing, does not stand up to pragmatic scrutiny and intuition. Imagine explaining their forecast selection process to the boss: "We have two forecasts here, and on the basis of past history, we think A is a lot better than B. So we are going to pick one. Most likely we will pick A, but there is a slight chance that we will end up with B." This is what the Mixing Method does, and no thoughtful boss should accept this decision rule.

Foster and Vohra want to be as general as they can, but in so doing they end up with an unreasonable decision rule. Expected utility does indeed require the decision maker to do more work, but that work is appropriate and leads to a more reasonable, defensible, and intuitive decision rule.

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REPLY TO PROFESSOR CLEMEN

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It is better for a paper to be read and damned then never to be read at all. For this reason we are grateful to Professor Clemen for taking the time to outline his reservations about the Mixing Method (MM). His central criticism is that the very generality of the result is its undoing. (One is reminded of Karl Pearson's criticism of linear regression as developed by Yule: "Its chief advantage is that it makes little or no assumption as to the distribution of frequency; its chief defect lies even in this advantage of generality

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