

Comment

Charles Wolf, Jr.

Among those asked to review the Mosteller–Youtz path-breaking paper, I am “very likely” ($p \geq 0.75$) to be more dissimilar to the other reviewers than they are to one another. What I mean by this is that the work that I do in the field of international economics, and the people and writings that I encounter frequently, at least once per day ($p \geq 0.5$), employ words that connote probabilities, but leave the reader or listener rather mystified as to the precise meaning that is intended. For example, some of the work involves relations between macroeconomics on the one hand and defense policy and foreign policy on the other; and some of the work involves connections between the international economy and the domestic U.S. economy, e.g., the “twin” deficits—the budget and trade deficits—and the relations between them. In these fields, and such related ones as the studies done by Sovietologists and Japanologists, results are often cast in terms of scenarios and forecasts and expressed as “probable,” “likely,” “possible,” etc. The referents of these terms are remarkably unclear to their intended audiences, whether or not their authors intend to communicate or simply to obfuscate.

In these fields, there is a vital and neglected need for two types of bridges or crosswalks to whose construction the Mosteller–Youtz paper makes a notable contribution. The first type requires a bridge between qualitative expressions of degrees, disposition, and attitude on the one hand and the implicit probabilities associated with these expressions. The second type involves bridges or crosswalks between quantitative expressions of forecasts and estimates on the one hand and their translations into clear, colloquial and non-technical prose.

The first type is a need one finds among social scientists at the “softer” end of that disciplinary spectrum, as well as among politicians, bureaucrats, and the more artful sciences (such as medicine) to facilitate and clarify their communication with people in the more exact sciences. The second type is a need that one finds among statisticians, physical scientists, and economists to facilitate and to de-jargonize their communication with audiences other than their own specialized peer group.

Examples of the first type include policy studies that use simulations and formulate scenarios charac-

terized as “likely,” or as “worst case.” The latter often ($p \geq 0.75$) carry the erroneous implication that the less “bad” excluded contingencies either are of negligible probability, or are somehow included within the “worst case” one. And both of these assumptions are often unwarranted. National intelligence estimates provide another example of verbal formulations couched in qualitative terms that would benefit by making explicit the implicit probabilities, thereby better informing the user, and helping in tracking, scoring, and evaluating the estimators.

An example of the second type of bridge or crosswalk is the practice of economic forecasting based on large macroeconomic models. These generally give a central or base case forecast rather than a range that would reflect the real uncertainties involved in the models. Even when these uncertainties are suggested by verbal descriptions of such forms as “employment is expected to hold steady,” or “the Treasury 90-day bill rate will probably rise by up to $\frac{1}{2}$ percentage points,” the implicit probabilities are seldom made explicit. And even when they are (rarely, $p \leq 0.1$), the associated forecast seldom ($p \leq 0.01$) indicates the track record of the forecaster’s prior forecast. (See Wolf, 1987.)

The Mosteller–Youtz paper makes two especially valuable contributions to this entire area. The first one is the authors’ derivation of the implicit probability estimates for those terms that have a small variance or relatively small interquartile range (e.g., “always,” “almost always,” “very likely,” etc.), so that in the future those who use these terms will be able to convey more clearly to their readers and their listeners what their intended meanings are. Their second notable contribution is to identify those terms that have high variances and high IQRs (e.g., “likely,” “possible,” “might,” etc.), so that readers who encounter these terms will not go away from the exercise with other than a clear awareness of the diffuseness and ambiguity—whether intended or inadvertent—implicit in the account.

In sum, the Mosteller–Youtz piece makes an invaluable contribution to efforts to improve the quality of communication, to raise the level of understanding, and to reduce inadvertent misunderstanding, let alone obscurantism, that arises from the use of vague, but implicitly probabilistic, verbiage. That even the most deliberate and meticulous among us are not entirely immune to this practice is suggested by the fact that even Mosteller and Youtz themselves occasionally use terms like “might” (see the Abstract, page 2), “the

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hope is" (page 18), leaving the reader quite puzzled concerning the probabilities the authors intend to imply.

Rejoinder

Frederick Mosteller and Cleo Youtz

Because our work on codification is still developing, we are most grateful to Carl Morris for arranging discussion by experts from various fields so that we can benefit from their suggestions and criticisms before we firm up our plans. (In what follows, "we" refers to Mosteller and Youtz.) We much appreciate the efforts of the discussants to help us avoid pitfalls and guide us to further material relevant to this work. In this paper we do not propose a codification but organize some material that may be useful in producing one. To treat all the issues raised by the discussants would require much more research than has been done in this field so far. We will, however, respond to the central issues.

We have no problem with the idea of including variability as part of the codification. For example, the data suggest that *even chance* has very little variability associated with it, whereas *possible* has huge variability. Some measures of properties of the acceptability function or of variability built up from variation within individuals, between individuals, and contextual sources offer options for presenting variability along with location (average value). How that information can be profitably communicated poses a question we have not yet settled. We illustrate one option when we discuss Kadane's comments.

Most of the discussants wish to emphasize the effects of context on meaning, and they do this in various ways. We will take up these matters as we go through the comments individually.

Wallsten and Budescu make four main points about the difficulties in trying to produce a codification, and they make some suggestions about such a program. "First, individual differences in the use and understanding of linguistic probability expressions are large, reliable, and probably very resistant to change." For us this offers a reason for including information about variability in a codification, and it explains why it may be important to know what sort of variability people trying to communicate need to face. Part of the effort in codification then should be to inform people about variation. "Second, probability phrases have vague meanings to individuals. Any attempt to render them precise will of necessity overlook the important semantic role of this vagueness." This additional var-

ADDITIONAL REFERENCE

WOLF, C. (1987). Scoring the economic forecasters. *The Public Interest* No. 88 Summer.

iability again is something that has to be included in a plan to create a codification. "Third, context effects on the meanings of probability phrases are substantial and probably cannot be eliminated." As these authors suggest later in their discussion, "it is possible that a subset of phrases can be selected whose meanings are more or less agreed upon." Possibly some phrases are relatively resistant to context or perhaps we can get people to learn to standardize them. The procedure would still have to face the variability associated with individuals, and so it may well be that we have to learn how little can be communicated with probabilistic phrases because they often have broad ranges (considerable variability). We have not decided on a way to communicate these distributions, though we have used the interquartile range here as one method. "Finally, there is often a need to communicate not only a best probability estimate, but also information about the amount and nature of supporting evidence." Although this is true, it may represent a need that goes beyond the notion of a codification, just as a whole theory of probability and risk assessment may be required. Wallsten and Budescu suggest that a program that uncovers the various communication roles for probability phrases together with numerical techniques could make it possible for people to express information about the state of evidence and precision of their opinion. Such a substantial program goes beyond what we have in mind.

Winkler also mentions such an idea in his discussion. We like such a program, indeed, statisticians and psychologists and others have this program in mind in their teaching and research. After 100 years or so of work on it, we all have a long way to go. Our idea is much less ambitious.

Cliff, like Wallsten and Budescu, encourages us to pay more attention to the variability of the meanings of expressions and less to measures of location. As we mentioned above, we do not find this inconsistent with codification.

Cliff suggests that, first, isolation of communicators and, second, specificity of referent will be needed if we are to be successful. We discuss the first point because the second falls under the general heading of context. Because nearly everyone is a statistician some