LETTERS TO THE EDITORS

Dear Editors

I would like to offer some comments on the recent paper by R. S. McKee and M. J. Brown entitled "An Experiment in Forecasting Precipitation for Smaller Areas . . . Preparing for the NWS Modernization" (*NWD*, **15**, 2, pp. 13–15). These comments relate to the following issues: (1) the characteristics of forecasting performance; (2) the verification of precipitation probability forecasts in terms of areal coverage; (3) the existence of several earlier experiments similar in many respects to the authors' experiment; and (4) the implications of the results of these experiments for precipitation probability forecasting in the modernized and restructured NWS of the mid-1990s.

First, the paper describes the results of an experiment "conducted to determine how *accurately* (emphasis added) forecasters could forecast the probability of precipitation (PoP) for smaller zones" (p. 13). In reality, the comparisons of forecasting performance presented in the paper relate entirely to the reliability (or calibration) of the PoP forecasts, not to their accuracy. Although the reliability of PoP forecasts warrants careful consideration (especially in the early stages of a new forecasting program), other characteristics of forecasting performance are equally important. In this case, it would also be desirable to compare the respective distributions describing the frequency of use of the various forecast probabilities. These distributions characterize the sharpness (or refinement) of the forecasts, and sharper distributions are exemplified by more frequent use of relatively high and low probability values (Murphy and Daan, 1985). Consideration of calibration *and* refinement provides a more comprehensive picture of forecasting performance (Murphy and Winkler, 1987).

If the authors are interested in the accuracy of the zone (and/or subzone) forecasts, then they need to compute a measure of the accuracy of PoP forecasts such as the Brier score (Brier, 1950). Alternatively, in view of the fact that the zone and subzone precipitation climatologies may differ, it would be more appropriate to compute a skill score based on the Brier score (see Murphy and Daan, 1985).

For additional insight into differences in forecasting performance in this context, it might be useful to compare the zone and subzone distributions of forecast probabilities conditional on the occurrence and nonoccurrence of precipitation. These conditional distributions characterize the ability of the PoP forecasts to discriminate between the two events. Discrimination is another characteristic of forecasting performance.

Second, in addition to traditional point verification, the authors use areal coverage as a means of verifying PoP forecasts. Although this approach may appear to be an "attractive alternative" (to use the authors' words), it is important to recall the conditions under which this type of verification is appropriate. If a PoP forecast represents an average point probability for the area of concern, then it can be shown to be mathematically equivalent to the *expected* areal coverage (Winkler and Murphy, 1976). Thus, PoP forecasts possess two possible interpretations—as point forecasts *and* as (expected) areal coverage forecasts.

Since PoP forecasts admit two interpretations, they can be evaluated as point forecasts *or* as areal coverage forecasts. The pros and cons of the two types of evaluation have been discussed in some detail, first by Murphy (1978) and then by Glahn (1981), Murphy (1979, 1981), and Smith (1979), and these points will not be repeated here. Perhaps the most important message forthcoming from this discussion is that the two types of evaluation should be viewed as complementary to each other rather than as alternatives. This message becomes quite apparent when it is recognized that the two evaluations are concerned with different types of forecasts—PoP forecasts interpreted as point forecasts are *probabilistic* forecasts, whereas PoP forecasts interpreted as areal coverage forecasts are *nonprobabilistic* forecasts.

Third, the authors—and the readers of the *NWD*—might be interested to know that several experiments were conducted in the 1970s to investigate the ability of NWS forecasters to make PoP forecasts on smaller spatial scales (as well as to make various types of "area" precipitation forecasts). These experiments were carried out in collaboration with NWS personnel and took place in St. Louis, Missouri (Winkler and Murphy, 1976), Rapid City, South Dakota (Murphy and Winkler, 1977), and Tucson, Arizona (see Murphy, 1978). The results of these experiments demonstrated that even at that time NWS forecasters could make reliable and skillful PoP forecasts on these smaller spatial scales.

Fourth, on a more fundamental level, it is not clear that any of these experiments provide definitive results regarding the ability of NWS forecasters to make reliable and skillful small-scale PoP forecasts in a modernized and restructured NWS. In the NWS of the mid-1990s, it is expected that weather forecasters will receive a variety of new mesoscale information (data, model output, etc.) as guidance. Initially, this information may lead to overconfidence on the part of the forecasters, with the result that their PoP forecasts might be characterized by too frequent use of extreme probability values. Any such lack of reliability (or miscalibration) presumably will decrease over time as forecasters "adjust" to the new information and calibrate its predictive insights. Formal feedback regarding individual forecasting performance can accelerate the recalibration process.

In effect, it should be possible to maintain reliability whatever the degree of specificity of the information sources and the scale of the forecasts. What can be expected to change as function of the information sources and scale of the forecasts is the distribution that characterizes the frequency of use of the various probability values. For a given forecast scale, increased information specificity should lead to more frequent (and reliable) use of extreme probability values.

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Dear Editors:

I found Eli Jacks' "Quiz on the Interpretation and Use of the National Weather Service's Statistical Guidance Products" (Digest, November 1990) particularly interesting and useful, and I hope you will print more articles of this sort.

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BOOK REVIEW

TITLE: **Meteorology in America, 1800–1870**
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701 West 40th Street; Suite 275
Baltimore, Maryland 21211
Copyright: 1990: Price: $45.00 (Hardback)

Fleming's book, **Meteorology in America, 1800–1870,** examines thoroughly the origin, growth, and subsequent development of meteorology during the colonial and Civil War periods to the time the United States established a national or military weather service. This time segment may be divided into four sections: (1) the age of individual, isolated weather observers prior to 1800; (2) a developing and expanding era between 1800 and 1870 (the subject of this book); (3) the rise of government services from 1870 to 1920 and beyond; and (4) the present professional period which began during the 1920's.

Chapter One, Early Issues and Systems of Observations, narrates the origin of meteorology in Europe. Following this, the scene changes to colonial United States. Thomas Jefferson, James Madison, and Benjamin Franklin were three significant contributors to atmospheric science. Prior to 1836, the Army Medical Department was the only organization supporting meteorological research. Later, from 1817 through 1850, the General Land Office, and the Academies in the State of New York became involved.

From 1834 to 1843, early American experts vehemently argued among themselves concerning alleged causes or origins of certain peculiar weather events. Chapter Two lists three prominent scientists who were involved: William C. Redfield, James Pollard Espy, and Robert Hare. Each man presented and strongly upheld his unique theory concerning the origins of wind circulation, water vapor distribution, rain formation, tornadoes, spouts, whirlwinds, and atmospheric electricity. These men consistently attacked each others' theories and their friendships suffered. Other scientists eventually became involved in this well known "storm controversy" throughout this time period. Near the close of this age, the early 1840's, several meteorologists toured Europe in an attempt to gain followers of their theories. This controversy subsided, only to surface many years later when institutions dedicated to meteorological advancements were being formed.

In Chapter Three, the philosophy of observations is discussed. Espy labored long and hard to try to coordinate weather observations throughout the United States. More institutions joined in this effort, linked together by a Joint Committee on Meteorology. The Franklin Institute, Philadelphia, led the way in 1831; shortly thereafter, the American Philosophical Society reluctantly joined the Institute, followed by several colleges, the Navy Shipyards, The Albany Institute, Army Medical Department, and the Smithsonian Institute. The appointment of Joseph Henry to lead the Smithsonian Institute began a new era in the construction of meteorological observations and systems throughout the United States. In 1848, a movement commenced to distribute instruments, blank forms, guidelines, and tables to all "correspondents" in the United States and Canada. Later, Mexico, Latin America, and some Caribbean nations were invited to participate. Chapter Four describes Henry's desperate efforts to solve the nagging "storm controversy" which raged some years before. He strongly felt that expanded observations would be the key to its solution. The Navy Department aided the Smithsonian in its far-flung endeavors in linking amateurs and professionals for the purpose of observations. Efforts were more limited than what the planners had envisioned. By 1870, observers were principally located east of the Mississippi River and north of the Carolinas and Tennessee. Only a scattering of observers lived in the deep South, and about a half a dozen existed the full length of the west coast of the United States.

Troublesome times, however, erupted again, as documented in Chapter Five. Since a more comprehensive picture of synoptic meteorology was evolving now, the scientists reignited the "storm controversy." Each scientist was increasingly anxious to advance his theory which would be "proven" by these expanded observations. Large numbers of scientific experiments with subsequent research papers were presented at AAAS (American Association for the Advancement of Science) Meetings. Heated exchanges were made at these colloquia and friendships again faded. Besides, the "big four," Henry, Espy, Redfield, and Hare, a new name appeared—Matthew Fontaine Maury. Maury wanted to chart air currents over the oceans, involve agriculture in weather observations, and bring meteorology into the realm of commerce as well. Scientists in Europe accepted Maury's ambitions, whereas those in the United States seemed to gradually shoulder him off to the sidelines of science. Eventually the deaths of some of these scientists quieted the struggle. A scandal erupted between the Smithsonian Institution and the Army Medical Department involving the "ownership" of meteorological data. In the conclusion of this chapter, history seemed to imply that this peculiar event temporarily weakened the Smithsonian's contribution to meteorology.