Reply

ROLAND A. MADDEN

National Center for Atmospheric Research[1], *Boulder, CO 80307*

22 June 1982 and 16 November 1982

1. Introduction

Dr. Shukla's comments on my 1976 paper are well taken and in my reply I have divided them into points with which I agree, those with which I disagree, and those of which I am uncertain.

2. Points of agreement

At least in principle, my analysis deals with "long-range" forecasting as defined by Leith (1973); that is, forecasting for times longer than deterministic predictability times. It in no way indicates what deterministic or dynamic predictability times are. It only provides a measure of the variability of time averages of the details of daily weather which are therefore, by definition, unpredictable at "long range." I treated the observed interannual variation of time-averaged pressure as comprised of two components. The first, which I called natural variability, was assumed to reflect this statistical sampling variation resulting from daily "weather." I imagined "weather" to be a realization sampled from a process with constant statistical properties or an unchanging climate. The second component is any additional variance which I viewed as resulting from shifts in the process mean in response to changing "external conditions" (all influences whose integral time scales are long relative to daily weather fluctuations) or, possibly almost intransitivity (mode of behavior by which the atmosphere might occasionally move from one set of statistical properties to another set through internal dynamics). Any attempt like this to consider observed meteorological time series as consisting of simply separable components involves approximation and uncertainty. Ideally one would have liked to estimate the effects of weather (assumed unpredictable at lead times longer than dynamic predictability limits) and those of external forcings (assumed at least potentially predictable at lead times longer than dynamic predictability limits) based on a thorough understanding of the physical processes involved. In practice I presumed that the essential character of variations due to weather could be isolated by observing the spectra of relatively short-time scale variations and modeling their longer-time scale effects by extending these spectra to zero frequency with a constant value (low-frequency white noise, LFWN). The actual longer-time scale variations were then assumed to be composed of unpredictable sampling variations approximated by these LFWN weather effects and possibly additional variance which is at least potentially predictable at long lead times. The LFWN assumption is uncertain but I think reasonable since we would expect weather to be uncorrelated with itself at long lags. The choice of where to separate relatively short- from relatively long-time scales is a discretionary one. I chose to separate time scales shorter and longer than a season. Shukla is absolutely correct in stating that changing external conditions, as we defined them, could affect variations on time scales shorter than a season. This is a point we made in later papers on the subject (Madden and Shea, 1978; Madden, 1981), but, unfortunately, failed to articulate in the first one (Madden, 1976, hereafter M).

While I do not want to downplay the role of natural variability or climate noise in determining the potential for long-range predictability, it should be stressed that my results do not show that monthly means are completely unpredictable as Shukla states has been inferred in the recent literature. The analysis of variance that I performed with the F-ratio provides evidence that some potential predictability exists for sea-level pressure at nearly all locations north of 20°N, excepting the midlatitude band (~40–50°N) (M). In addition, I pointed out that the F-ratio is not particularly powerful[2] and therefore I further compared actual signal-to-noise ratios for two assumed "signals" (linear trends and the Quasibiennial Oscillation). These comparisons are consistent with the analysis of variance in that minimum signal-to-noise ra-

---

[1] The National Center for Atmospheric Research is sponsored by the National Science Foundation.

[2] In this case, the actual interannual variability must be considerably greater than the natural variability before the null hypothesis of no predictability can be safely rejected.

tios occur at midlatitudes, but many areas were shown to reflect shifts in mean values that might be of practical value. It is true, however, that I have discussed the likely frequency of occurrence of large signal-to-noise ratios in a more recent paper (Madden, 1981), and I argue there that it is, at best, small now and will always be limited by the natural variability.

It is also important to restate that large natural variability need not be synonymous with low potential predictability. It is only so when the natural variability (large or small) is as large as the actual interannual variability. Furthermore, as mentioned at the outset, my results say nothing about the limits of dynamical predictability of the details of weather in general, nor about the long waves or blocking events in particular.

3. Points of disagreement

Shukla argues that the notion of day-to-day weather fluctuations introducing an unpredictable variability to monthly means is contradicted by the fact that most of the variance of monthly means comes from variations on time scales of 96 days or more. This apparent contradiction can be resolved by realizing that even though their passage and intensification may occur on a relatively short time scale, the occasional very deep low or intense high produces some relatively low-frequency variance. Because monthly or seasonal averages are low-pass filters, it is only this low-frequency variance of the weather that contributes to the natural variability.

Shukla concludes that my values of natural variability are overestimates. His reasoning concerning the likely consequences of slowly changing boundary condition is persuasive in this regard. Nevertheless, because we do not fully understand the physical processes of weather nor how boundary conditions may affect them, it should be stated that our LFWN assumption at frequencies less than $1/96$ day^{-1} need not necessarily be an overestimate and, in fact, it is not inconceivable that it results in an underestimation of natural variability.

Because he judges my values of natural variability to be overestimates, Shukla concludes that my results should be considered as the lower bound for potential predictability. On the other hand, we have stated: "Although there are uncertainties in estimating the natural variability of time-averaged temperature, we propose that the σ_T's of Fig. 2 (i.e., estimates of natural variability of temperature made in a manner similar to that for pressure) provide reasonable lower limits for the standard error of estimates of any long-range predictions." (Madden and Shea, 1978, p. 1701). This contradiction is more one of emphasis than of fact. Shukla views my numbers as overestimates of natural variability and therefore they can give only a lower bound for potential predictability. We assume that at least some of the variance that exceeds natural variability (uncertainties in estimating the natural variability aside) is unpredictable and therefore natural variability itself provides a lower bound for the unpredictable part of time averages.

4. Uncertain points

Because, as Shukla points out, weather and climate are in reality inseparable, the impact of weather alone on the standard error of time averages cannot be determined with precision. Furthermore, since any estimates we do make may contain some measure of unknown effects of changing external conditions as well as weather, we cannot be certain at what long range or lead time they represent a wholly unpredictable component. Since my estimates were based on what could be determined about the spectra on time scales of a season or less, I think that I was safe in defining long range as a year or more. If effects of changing external conditions are small within a season, and the LFWN assumption at periods longer than 96 days closely approximates the true character of the spectrum of idealized weather, then my results would apply to predictions of shorter range as well. I expect that regions determined to have relatively high or low potential predictability are valid for lead times as short as a month or two, even though the absolute values of natural variability may not be; however, I emphasize again that my results do not apply at a range so short that there exists dynamic predictability.

Fortunately, we may be able to get some insights into the spectrum of idealized weather from numerical models. We can look at the spectrum of model data generated by long runs with constant external conditions. Lau (1981) has published such spectra determined from 15 90-day winter simulations of a GFDL spectral general circulation model.[3] The spectra are for the amplitudes of the first eigenvector of 1000 and 500 mb geopotential height. They increase monotonically from high frequency to the lowest resolvable frequency, $1/90$ days^{-1} (Lau's Fig. 21). These first eigenvectors reflect large-scale variations and their spectra may differ from that of grid point data. Bates and Blackmon have been computing spectra of grid point model data from the Community Climate Model at NCAR (for basic documentation of the NCAR CCM, see Pitcher *et al.*, 1983). Their spectra are estimated from the equivalent of eight 90-day segments and, in general, increase from high fre-

---

[3] The model contains no low frequency forcing (other than seasonal) from fluctuations in sea surface temperature, ozone, cloud amounts or insolation, but may have some resulting from processes relating to changing snow cover and soil moisture (Manabe and Hahn, 1981).

quency to $\frac{1}{60}$ day^{-1}, their lowest resolvable frequency (personal communication, 1982). Straus and Halem (1981) studied GLAS GCM sea-level pressure data generated for the equivalent of 11 separate "30-day Januaries." Straus has provided me with the spectra at two grid points that were computed for, but not published with, the Straus and Halem paper (Straus, personal communication, 1982). They do not increase monotonically from high to low frequency since each has a relative maximum at $\frac{1}{6}$ day^{-1}. Nevertheless, they have absolute maxima at $\frac{1}{30}$ day^{-1}, the lowest resolvable frequency.

It would be good to have spectra from several more models to set bounds on the likely behavior of the spectrum of idealized weather. Although admittedly speculative, I can use the above limited results to set bounds and then determine the sensitivity of estimates of the natural variability to these bounds. For simplicity, I will consider a first order autoregressive or Markov process for the upper bound. Its spectrum increases monotonically to zero frequency (e.g., Gilman *et al.,* 1963). For a lower bound, I will truncate the spectrum of the first order autoregressive process at $\frac{1}{14}$ day^{-1} and consider it to be white noise at lower frequency, since spectra from all three model experiments mentioned above increase with decreasing frequency to frequencies less than $\frac{1}{14}$ day^{-1}.

Straus and Halem (1981) have shown that higher order autoregressive processes than the first are generally better models for sea-level pressure over the United States. It has been our experience that the spectra of first order processes tend to overestimate observed spectra of sea-level pressure and temperature at periods less than approximately three days, underestimate them in the range of ~3–10 days, and fit reasonably well at periods longer than ~10 days. Since estimates of the natural variability are primarily based on the spectra at periods longer than ~30 days, the first order process should serve adequately to demonstrate the sensitivity of my analysis procedure to changing truncation points.

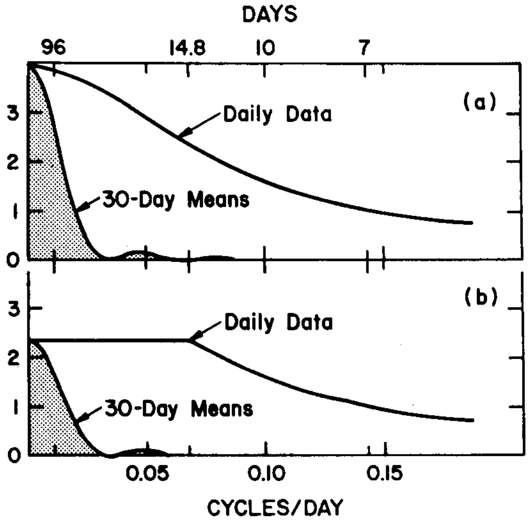

FIG. 1. (a) Spectra of daily and 30-day averaged first-order autoregressive process with a lag-1-day autocorrelation of 0.6. (b) Similar to (a) but values set equal to a constant from 0 to $\frac{1}{14}$ day^{-1}. Ordinate is variance expressed as ratio to white noise value. Areas are proportional to variance and the shaded areas are proportional to those of 30-day averages.

The average lag one day autocorrelation for sea-level pressure north of 20°N in winter is ~0.6 (Schuman and van Rooy, 1952; also reproduced in Madden, 1979). Fig. 1a shows the low-frequency spectrum for a first-order autoregressive process with a lag-1-day autocorrelation of 0.6. The spectrum of 30-day averages of such a process is indicated as well. Fig. 1b shows the same two spectra but they have been truncated to LFWN at $\frac{1}{14}$ day^{-1}. Areas under the spectral curves are proportional to variance and the reduction in variance for 30-day averages from Fig. 1a to 1b is 60%. This provides a measure of the sensitivity of my results for this case. The ratio of spectral values at zero frequency from Fig. 1b to that from Fig. 1a is only slightly less than 0.60 as well. Since it is the near zero spectral values that contribute to the variance of very long time averages, we conclude that the impact of changing truncation points is approximately the same for all time averages of 30 days or longer.

Fig. 2 summarizes the change in variance of 30-day averages for other truncation points and other lag-1-day autocorrelations. A truncation point of $\frac{1}{96}$ day^{-1} was used in M and that is taken to be unity. This comparison shows, as I pointed out in M, that my results are close to those given by a first order autoregressive process. On the other hand, they would give values nearly three times greater than a first order autoregressive process with a lag correlation equal to 0.8 that is truncated at $\frac{1}{14}$ day^{-1}. This, I think, is an extreme case and for more moderate values of correlation equal to 0.7 or less and truncation near $\frac{1}{30}$ day^{-1} differences are less than 20%.

In summary, the results are sensitive to my choice of $\frac{1}{96}$ day^{-1} as a truncation point. Nevertheless, there is no evidence available, as yet, that this is a seriously biased choice. Spectra of more model data may provide this evidence. Even if my results can eventually be shown to be overestimates of the standard errors of time averages of idealized weather, I believe the regions and seasons, determined in M as having more or less long-range potential predictability, will not be considerably altered.

I think that the effect of removal of the annual cycle is a less serious problem than that of our lack of exact knowledge of the low-frequency spectrum of the idealized weather. Of course, it should be removed to avoid contributions to the variance and autocorrelation due to the annual cycle. On the other hand, with no *a priori* reason to expect a trend within a

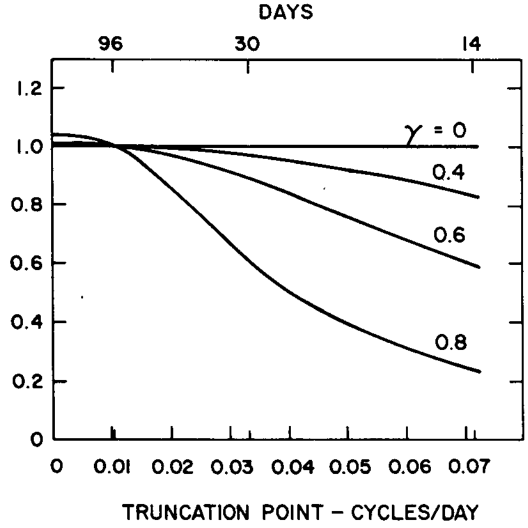

FIG. 2. Ratio of the variance of 30-day averages determined from spectra of a first-order autoregressive processes with selected lag-1-day autocorrelations γ which have been truncated to white noise beyond differing periods to that truncated at 96-day periods.

season (over and above that associated with the annual cycle) it would be an arbitrary decision to remove one. I expect that the method of removing the annual cycle and the impact of linear trends within seasons are second order effects.

5. Summary

Dr. Shukla has underscored difficulties in evaluating the natural variability. My treatment of meteorological time series as composed of two separable components is an idealized one. Inspite of the uncertainties, I expect that regions and seasons of more or less potential predictability are reliable, and, although I cannot defend the precision of my estimates of natural variability, I am reluctant to conclude that they are over estimates without a better understanding of the nature of the idealized weather.

My results are quantitatively sensitive to my choice of the shape of the spectrum of idealized weather. I propose that more model data determined from experiments with constant external conditions be diagnosed to determine their spectra. Models have their own peculiar problems that may rival those associated with estimating natural variability from real data, but they do offer an opportunity for a controlled experiment. Hopefully, results from several different models will provide consistent insights into the low frequency behavior of idealized weather so that we can establish the quantitative reliability of my estimates of natural variability or develop new methods to improve on them.

Finally, my results say nothing about the limits of deterministic predictability and it is important to continue work on establishing what those limits are.

Acknowledgments. I wish to thank R. Livezey for his helpful comments on an earlier version of this paper, G. Bates, M. Blackmon and D. Straus for allowing me to discuss their, as yet, unpublished spectra.

REFERENCES

Gilman, D. L., F. J. Fuglister and J. M. Mitchell, Jr., 1963: On the power spectrum of "red noise." *J. Atmos. Sci.*, **20**, 182–184.

Lau, N.-C., 1981: A diagnostic study of recurrent meteorological anomalies appearing in a 15-year simulation with a GFDL general circulation model. *Mon. Wea. Rev.*, **109**, 2287–2311.

Leith, C. E., 1973: The standard error of time averaged estimates of climatic means. *J. Appl. Meteor.*, **12**, 1066–1069.

Madden, R. A., 1976: Estimates of the natural variability of time-averaged sea-level pressure. *Mon. Wea. Rev.*, **104**, 942–952.

——, 1979: A simple approximation for the variance of meteorological time averages. *J. Appl. Meteor.*, **18**, 703–706.

——, 1981: A quantitative approach to long-range prediction. *J. Geophys. Res.*, **86**, 9817–9825.

——, and D. J. Shea, 1978: Estimates of the natural variability of time-averaged temperatures over the United States. *Mon. Wea. Rev.*, **106**, 1695–1703.

Manabe, S., and D. G. Hahn, 1981: Simulation of atmospheric variability. *Mon. Wea. Rev.*, **109**, 2260–2286.

Pitcher, E. J., R. C. Malone, V. Ramanathan, M. L. Blackmon, K. Puri and W. Bourke, 1983: January and July simulations with a spectral general circulation model. *J. Atmos. Sci.*, **40**, 580–604.

Schumann, T. E. W., and M. P. van Rooy, 1952: The autocorrelation of daily sea-level pressure over the Northern Hemisphere. No. 17, Pretoria Weather Bureau, South Africa, 7 pp.

Straus, D. M., and M. Halem, 1981: A stochastic-dynamical approach to the study of the natural variability of climate. *Mon. Wea. Rev.*, **109**, 407–421.