commentary and analysis

Comments on "Detection and Attribution of Recent Climate Change: A Status Report"
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Barnett et al. (1999) provide an excellent review of progress in model-based detection and attribution during the past few years. Unfortunately, the section on "Paleoclimate proxies" is flawed. We cannot agree that "our best estimates of natural variability will come from CGCMs" or that "paleoproxy data have serious shortcomings that preclude their use for this purpose." On the contrary, we believe that paleoclimate data provide the *only* prospect for realistically estimating the envelope of natural climate variability and validating model simulations. Here we point out several issues that lead to erroneous conclusions in their paper.

1) Barnett et al. state: "Straightforward comparisons via cross-spectral analysis . . . show that most of the paleodata are not simple proxies of temperature." This may be true for the sample of proxy data that they selected. Unfortunately, no information is given on what temperature variable or season was used in the calculation of correlations and coherency squared in their Table 1, and so the reader is not able to judge the appropriateness of the comparisons they report. However, their inability to identify a strong local temperature signal in the proxies they select is almost irrelevant to the detection and attribution problem at hand. The authors imply that this is a fundamental limitation to the use of paleoclimatic data in large-scale temperature reconstructions. *This is not so.* Several studies (e.g., Bradley 1996; Mann et al. 1998; Evans et al. 1998) have shown that the available network of proxy climate records is more than adequate to reconstruct the principal patterns of temperature variability. These studies have pointed out that the main issue is whether or not a particular distribution of data is adequate to sample the key *patterns* of temperature variability on interannual and longer timescales (associated, for example, with ENSO, the North Atlantic oscillation, and various decadal and multidecadal patterns of intrinsic climate variability; cf. Kelly et al. 1999). Proxies that may be poorly correlated with *local temperature records* (i.e., from nearby weather stations) may nonetheless capture some of the variance of the *large-scale* temperature patterns. This is because the proxy may be responding to a set of conditions that are correlated with large-scale climatic conditions. Consider, for example, the drought-sensitive tree-ring latewood width chronologies from Mexico and the southwestern United States (Stahle et al. 1998), which show very strong (and statistically significant) correlations with the Southern Oscillation index. In combination with coral records in the tropical Pacific (reflecting, e.g., salinity changes related to ENSO) such indicators can constrain quite effectively the behavior of the ENSO phenomenon. In so doing, this combination of indicators is thus able to constrain a prominent global surface temperature pattern responsible for substantial variability over much of the tropical and extratropical Pacific Ocean, where the available proxy indicators themselves are quite sparse. Similarly, trees that are sensitive to drought in Morocco (Stockton and Glueck 1999) appear to be effective indicators of the North Atlantic oscillation, a primary mode of extratropical cold-season temperature variability. Again, such proxy indicators can be calibrated to resolve a prominent share of large-scale surface temperature variability, even though they may not be a direct indicator of their local temperature. Hence, a simple correlation of such records against local instrumental temperature data is not a useful assessment of their potential value in large-scale temperature pattern reconstruction. Barnett et al. (1999) confuse the issue of the usefulness of a proxy record in calibrating a local

temperature signal, and the potential usefulness of the same record in calibrating a large-scale pattern of climate (and temperature) variability.

2) The statement that "only a few of the tree-ring records from mid- to high-latitude sites can be interpreted directly as temperature changes" is factually incorrect; the authors overlook several decades of dendroclimatological research which demonstrate otherwise (e.g., Briffa et al. 1988, 1990; Graybill and Shiyatov 1992; Hughes et al. 1999; Jacoby and Cook 1981; Vaganov et al. 1996). There is a body of careful work in which the difficulties of using natural archives are addressed explicitly (e.g., Briffa et al. 1998; Vaganov et al. 1999).

3) The statement "The disparity between these reconstructions at some times over the last 400 years is *as large* as the observed changes in global temperature over the last 100 years" [in reference to the comparison shown in their Fig. 2 between the hemispheric temperature estimates of Mann et al. (1998), Briffa et al. (1998), and Jones et al. (1998)] is incorrect. The maximum discrepancy during the 400-yr interval shown is 0.45°C, between the Briffa et al. (1998) and Jones et al. (1998) estimates around 1840. In contrast, the mean twentieth century warming indicated by the smoothed instrumental record is approximately 0.8°C. In fact, by appropriate measures, the proxy reconstructions of hemispheric mean temperature are more similar to some of the model simulations in terms of the amplitude of estimated internal variability than the models are to each other. Crowley and Kim (1999), using an energy balance model, estimate that as much as 18%–34% of low frequency variability in proxy-based hemispheric temperature reconstructions over the preanthropogenic interval could be forced by volcanism and solar variability. Most importantly, they show that the spectrum of the residuals (i.e., the remaining component after this forced variability is accounted for) agrees with that of unforced variability from control runs of coupled models, at the 90% significance level.

The claim that the disparity between the three reconstructions shown in their Fig. 2 is largely due to intrinsic uncertainties in our ability to estimate hemispheric mean temperature ignores evidence to the contrary. A large part of the difference between the Mann et al. (1998) and Jones et al. (1998) reconstructions, for example, is to be expected, because the reconstructions were designed to cover different regions and seasons (Jones et al. 1998; Mann et al. 2000). The Jones et al. estimate is simply scaled to be an indicator of extratropical summer temperatures, while the Mann et al. (1998, 1999) series are calibrated as a Northern Hemisphere and annual-mean estimate. Other series, such as the Overpeck et al. (1997) estimate, are based on a yet different, high-latitude summer sampling emphasis. These seasonal and latitudinal sampling issues appear to be the primary reason for differences between the estimates. If the Mann et al. pattern reconstructions are sampled in a similar extratropical latitude band (30°–70°N), the two reconstructions lie well within the self-consistently estimated uncertainties of Mann et al. (1998). Showing these series without mentioning the explicit differences between them is misleading. Furthermore a more recent version of the Briffa et al. (1998) series, using more conservative standardization techniques (Briffa and Osborn 1999), is far more similar to the Mann et al. (1998) series than their earlier study reported.

4) Barnett et al.'s statement that it is "debatable whether there is enough temperature proxy data to be representative of hemispheric, let alone global climate changes given . . . lack of large spatial coherence in the data" is incorrect on at least two counts.
   (i) The ability to skillfully reconstruct Northern Hemisphere mean temperature with such data has been rigorously demonstrated with careful cross-validation exercises by Mann et al. (1998, 1999) using independent large-scale temperature measurements from the nineteenth century, and the sparse available measurements several centuries back in time in Europe and North America, to test the pattern reconstructions.
   (ii) Highly significant coherence in these data has been clearly demonstrated; Mann et al. (1995) showed that interdecadal (15–35 yr) and multidecadal (50–100 yr) frequency bands exhibit clearly enhanced amplitude climate variability in global proxy data during the past five centuries, even relative to the null hypothesis of spatially correlated colored climatic noise. Such findings have been confirmed by other recent work (Delworth and Mann 2000), which not only demonstrate the robustness of the multidecadal signal in the proxy data, but show striking similarities (in both timescale and spatial pattern of the climate signal) to an

intrinsic multidecadal mode of variability evident in long integrations of the Geophysical Fluid Dynamics Laboratory coupled ocean–atmosphere model. The latter establishes, indeed, a causal dynamical mechanism for the signal.

5) Finally, we believe that relying on computer models without the validated ability to model century and longer timescale variability to estimate the actual low-frequency variability of the climate is thoroughly unwise. Despite the comments of Barnett et al. (1999), it is self-evident that global climate models can never be relied upon to give a definitive description of the natural variability of the true climate system. *Models cannot estimate variability associated with processes or feedbacks that are not present in the models themselves.* The potential nonlinearities of small-scale processes that are not resolved, dynamical feedbacks that are not accurately represented (cf. Cane et al. 1997), and numerous potential surprises regarding the true global climate system (e.g., the role of gas hydrates) will forever limit the ability of models to accurately represent the true system. The arguments of Barnett et al. fly in the face of the most basic tenet of natural science, whereby the real-world truth must always be the target for our theoretical models. In the control integrations that Barnett et al. suggest we rely upon to estimate natural variability, the very factors that may have led to that variability are missing. For example, there is absolutely no representation of long-term changes in solar irradiance forcing, volcanic forcing, astronomical variations, land use changes, and other factors, which may influence climate on decadal to millennial timescales. Without a consideration of such effects, model-based estimates of "natural variability" must necessarily be very imperfect approximations to reality.

In contrast to the conclusion of Barnett et al., we argue that, in fact, paleo data offer the best and *only* hope of validating the low-frequency behavior of models. They add a critical element to the detection and attribution problem that will never be possible from a comparison of twentieth century model simulations and instrumental observations alone. The paleo data allow us to look at potential forcing mechanisms prior to the era of anthropogenic forcing, potentially validating or testing model-based estimates of climate sensitivity.

In summary, Barnett et al. (1999) completely undervalue the role of paleoclimatic data in assessing and documenting temperature variability. Indeed, the utility of these data is not only limited to the study of past fluctuations in seasonal or annual temperature, but extends to precipitation, circulation patterns, the past behavior of the oceans and of the cryosphere, as well several important forcing factors such as atmospheric composition, solar irradiance changes, and volcanic eruptions (Alverson et al. 2000; Bradley 1999). Models are useful tools to understand the complexities of past and future climate variations, but they are no substitute for reality.

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