Reply

KERRY H. COOK
Department of Earth and Atmospheric Sciences, Cornell University, Ithaca, New York

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In my modeling study, I found that a dipole precipitation perturbation occurred in a GCM over southern Africa due to an idealized ENSO sea surface temperature anomaly (SSTA), involving warming only in the eastern Pacific. The dipole perturbation is realistic, being similar to that observed for an average of ENSO warm events (e.g., Kiladis and Diaz 1989). The dipole anomaly is generated by a northeastward shift of the South Indian convergence zone (SICZ). The paper (Cook 2000) discusses this response as an example of how thinking about the basic dynamics of land-based convergence zones can be used to understand better the mechanics of precipitation variability in certain parts of the world. A subsequent paper (Cook 2001) includes a more in-depth explanation of the dynamics of the SICZ shift in the model and relates it to eastern Pacific warming.

The response over southern Africa in the model is forced by warming in the eastern Pacific, through an atmospheric “bridge” and occurs in the absence of warming in the Indian Ocean. Thus, the modeling study suggests that warming in the eastern Pacific can modify the southern Africa precipitation field without an intermediary warming in the Indian Ocean.

Nicholson (2003) disagrees with this possibility. One reason for her concern is that my results disagree with some other studies. One approach that has been used is empirical analysis, through the generation of statistical correlations between African precipitation and SSTs in various parts of the world. These studies reveal statistical links between precipitation over southern Africa and the Indian Ocean and, it is not surprising, indicate that these links are stronger than those between African precipitation and eastern Pacific SSTs. Statistical analysis provides a powerful guide for where to look for physical connections but cannot reveal mechanisms. The small number of well-observed warm events, combined with strong inter-ENSO variability and the high level of variability of the climate system, suggests that we need to combine the statistical approach with mechanistic (process) studies to understand fully and better predict such responses.

The work reported in Cook (2000, 2001) does not directly address the relative influence of the Pacific and Indian Ocean basins on African precipitation variability. This work is focused on identifying and validating physical mechanisms that connect warming in the eastern Pacific with precipitation variability and that cause the realistic precipitation perturbation in the model. In a subsequent investigation, which is currently being prepared for publication, we conduct additional ensemble simulations that include Indian Ocean SSTAs. We find that the southern Africa dipole precipitation response also occurs in response to Indian Ocean warming alone, but it occurs via a somewhat different mechanism. When Indian Ocean warming accompanies eastern Pacific warming, the dipole shift occurs one month later (January) than in the case of eastern Pacific warming alone. We are developing the idea that a shifting of the SICZ is a preferred mode of variability for this region that can be triggered in a number of ways, similar to the way people think about the Pacific–North America (PNA) pattern.

Nicholson (2003) also mentions that my results disagree with another modeling study. In particular, she cites the work of Goddard and Graham (1999), who performed simulations to understand better how SST variability in the global oceans, the Pacific Ocean alone, and Indian Ocean alone (GOGA, POGA, and IOGA simulations) propagates into eastern and southern Africa. Goddard and Graham conclude that “the central-eastern/southern African dipole pattern in the GOGA simulation arises from changes in convective heating over the Indian Ocean. . . .” Forcing with SST variability only in the Pacific Ocean in their model does not produce the dipole response.

I agree that the results from Goddard and Graham’s
work are different from the results in my modeling study, and it would be interesting to reconcile these results through an exploration of physical processes. One possibility is that the atmospheric bridge from the eastern Pacific warm SSTAs to the southern Africa precipitation field in the idealized simulations is easily interrupted, so that including the full variability of the Pacific Ocean basin obscures the signal. Another possibility is that averaging over many different years, as one does in these GOGA-type integrations, makes it difficult to isolate the physical mechanisms that operate in some years but not in others because of inter-ENSO variability and the significant differences in ENSO response that originate in the background state of the atmosphere.

A more recent modeling study of the global sensitivity to SSTAs in the equatorial Pacific by Barsugli and Sardeshmukh (2002) shows strong sensitivity over southern Africa to the imposition of warm SSTAs in the central and eastern Pacific. This set of simulations is designed to suppress the effects of inter-ENSO variability and different background states, like the Cook (2000) simulations and unlike Goddard and Graham (1999).

Perhaps most convincing, however, is the evidence from observations. For example, consider the strong ENSO events of 1982/83 and 1997/98. While the Indian Ocean was anomalously warm in both cases, the dipole precipitation signal occurred only during austral winter 1983, and the upper-level circulation anomaly during January 1983 featured anomalous convergence over the equatorial Indian Ocean (similar to my modeling results). During the 1997/98 event, the dipole pattern did not emerge and the upper-level circulation anomaly over the Indian Ocean was very different, despite Indian Ocean warming in both cases.

To improve our understanding of these processes and our prediction ability for this complicated and highly variable system, we need to use a variety of approaches. These include statistical analysis to suggest possible physical connections within the observed fields and the application of complex GCMs to test the extent of our ability to simulate. They also include process studies that seek to isolate and understand the dominant physical mechanisms. My modeling approach, which uses a fully nonlinear dynamical model with boundary conditions of various levels of complexity and idealization, is designed to allow us to isolate and study physical mechanisms. For example, the ensemble simulation design described in Cook (2000, 2001) suppresses all sources of variability except those associated with a particular, idealized SSTAs, and eliminates inter-ENSO variability. It is quite possible that with this design we could be simulating a response that would rarely happen in the real world, since it may be a weak response that is often overwhelmed by other sources of variability. That is why we progress to more realistic simulations and include extensive comparisons with observations. In the case of the southern Africa dipole, the response simulated by the model is realistic, suggesting that forcing from the eastern Pacific through the atmosphere alone may be relevant.

Nicholson (2003) also takes issue with my interpretation of the precipitation field near southern Africa as being a land-based convergence zone, similar to the South Atlantic convergence zone (SACZ). The South Indian convergence zone (SICZ) is certainly not as constant as the SACZ, and sometimes the precipitation in this region looks more like a southward excursion of the ITCZ. But at other times, for example, during January 1994 (Fig. 1b in Cook 2000) and during many other austral winter months, the SICZ very clearly exists and is even more distinct than the SACZ. Thus, thinking about the basic dynamics of the convergence zone provides useful insights into the mechanics of precipitation variability in this region.

Nicholson (2003) states that the GCM used in my simulations is “inadequate for studying the geographical region in question.” She bases this statement on a comparison between the simulated precipitation climatology of the model and the rain gauge climatology of Nicholson et al. (1997), and suggests that I used the National Centers for Environmental Prediction–National Center for Atmospheric Research (NCEP–NCAR; Kalnay et al. 1996) reanalysis as my standard of comparison for the modeled precipitation. In Cook (2000), the NCEP–NCAR reanalysis precipitation was not shown to serve as a standard of validation. This is not advisable because precipitation from the reanalysis is heavily dependent on model parameterization. Rather, the reanalysis is used in Cook (2000) to study the atmospheric moisture balance components over Africa, which is a way of validating the model at the process level. Although the moisture field in the reanalysis is a model product, the wind field is the result of assimilating station (upper air) data, and so it is a useful standard of comparison in this context.

We compare the modeled precipitation in our model with many sources, as many as we can have access to. In Cook (2000), a rain gauge–satellite blended product was shown in Fig. 1 as a comparison for the modeled precipitation. In Cook (1997), the modeled precipitation was compared with the Legates and Willmott (1990) rain gauge climatology, the Goddard Earth Observing System (GEOS) reanalysis (Schubert et al. 1993), Huffman et al.’s (1995) satellite–gauged product, and a pure satellite product (Huffman et al. 1997). Please see Fig. 1 of Cook (1997) to get a feel for how these various observations differ in structure and magnitude over Africa. We find that gauge–satellite blended products are particularly useful because the gauge data alone are confined to the land surface and does not give us a look at the large-scale structures in which we are interested.

As pointed out by Nicholson (2003), there is substantial disagreement between the satellite and gauge precipitation climatologies for Africa. I would expand this to say that there is also substantial disagreement
among the rain gauge climatologies that are available, and among the various satellite products as well. This makes model validation problematic for this region if one only examines the precipitation field. (In addition, the model climatologies of the various GCMs are very different from each other, although it is typical for modeled precipitation maxima to be too high over tropical continents.) The uncertainties generated by the disparities among the various observations makes it especially important to analyze models at the process level and to try to understand the physics of the response, rather than relying on the comparison with observed fields to tell one whether the model is providing a sensible response.

In her Fig. 2, Nicholson (2003) compares the modeled precipitation field for boreal summer from one of my GCM simulations (Fig. 2a in Cook 1997) with rain gauge observations from Nicholson et al. (1997). The GCM climatology shown here is from an idealized simulation, with perpetual July conditions, no topography, uniform surface albedo, and uniform soil moisture. This simulation was conducted to investigate the basic dynamics of how the presence of the African continent perturbs the precipitation field, and not to produce the most realistic simulation possible. I refer the reader to Fig. 2b in the same paper, which shows the precipitation from a model integration with more realistic boundary conditions, including topography, in which precipitation maxima over the Ethiopian and Cameroon highlands emerge. The model still is not simulating the precipitation maximum on the west coast very well, and this deficiency occurs in many GCMs [although not in our mesoscale model simulations; see Vizy and Cook (2002)].

Nicholson (2003) also compares the precipitation field for austral summer from one of my GCM simulations (Fig. 6a in Cook 2000) with rain gauge observations from Nicholson et al. (1997). I guess beauty is in the eye of the beholder! I am pleased to see three observations from Nicholson et al. (1997). I guess beauty is in the eye of the beholder! I am pleased to see three observations from Nicholson et al. (1997). I guess beauty is in the eye of the beholder! I am pleased to see three observations from Nicholson et al. (1997). I guess beauty is in the eye of the beholder!

All of this points to a need for more research on African precipitation and its variability. I disagree with Nicholson’s (2003) statement that “rainfall variability over southern Africa is reasonably well understood.” We need to develop a much better process-level understanding of African precipitation, its variability, and its relationship to the large-scale circulation. I would urge others to become involved in this exciting and useful area of research.

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REFERENCES