

Reply

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We would like to thank Roger Pielke for his comment (Pielke 2010) on “A unified modeling approach to climate system prediction” (Hurrell et al. 2009), particularly for calling attention to Pielke (1998). We apologize for not citing this paper. We do not agree, however, with the main thrust of the comment, which is that there is little utility in climate predictions several decades and longer into the future due to limits on predictive skill from a given initial state and the presence of significant nonlinearities in the climate system.

Modeling evidence to date demonstrates long-term climate change is primarily a boundary value problem associated with changes in radiative forcing. Because the details of individual weather systems are not being tracked, the initial conditions of the system are much less important than for weather forecasts. This fact has long been recognized. For instance, in his tribute to Ed Lorenz and his pioneering work on chaos theory and the predictability of weather, Palmer (2008) noted, “Lorenz was keenly aware of the conceptual difference in predicting weather and predicting climate . . . predictions of the first kind are essentially initial value problems, predictions of the second kind are essentially boundary or forced problems. The problem of anthropogenic climate change is predominantly (but not exclusively) a prediction of the second kind: how are the statistics of weather affected by some prescribed change in

atmospheric greenhouse-gas concentration?” In his comment, Pielke (2010) confuses the distinction between predicting the evolution of individual weather events beyond two weeks or so and the possibility of predicting changes in the statistics of weather events. Changes in forcing external to the climate system, as well as the amplification or diminution of the resulting changes in climate due to feedback mechanisms, can lead to predictable changes in weather statistics.

Consider the proven ability of climate models to simulate the annual cycle of seasonal variations (i.e., the changes in climate from winter to summer) or their ability to capture past excursions of climate resulting from changes in both natural and anthropogenic forcing, including the amount of solar energy reaching Earth, the amount of particulate matter in the atmosphere from volcanic eruptions, and atmospheric concentrations of anthropogenic gases and particles. The impressive fidelity of the twentieth-century climate simulations assessed in the latest report of the Intergovernmental Panel on Climate Change (Solomon et al. 2007) is a good example, as are many published studies of model simulations of past, very different climate states such as the mid-Holocene and the Last Glacial Maximum. At continental and larger spatial scales, none of these simulations depends on the details of the initial climate state, but rather they are driven by changes in external forcing. Moreover, because these climate models encapsulate our best understanding of the physical processes involved in the climate system and their many interactions across scales, they already include many of the dynamical and nonlinear processes that concern Pielke (2010), and there is ample evidence that climate models can capture nonlinear change thresholds and feedbacks (e.g., Holland et al. 2006; Pitman and Stouffer 2006). Climate models are certainly not perfect, and cryospheric, biospheric, and biogeochemical processes just now being included will be critically important in further improving predictions of future climate, including possible abrupt changes. However, if initial conditions and nonlinearity were the essence of large-scale climate change, as Pielke implies, we would have had far less success in interpreting and modeling past climate.

We do agree with Pielke (2010) that the effects of initial conditions and the presence of significant nonlinearities become more important when regional climate change over the next few decades is considered, and we call attention in our paper (Hurrell et al. 2009) to the growing interest and the many challenges associated with initialized “decadal” climate predictions.

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Pielke does not take issue with this important aspect of our paper, nor does he offer useful commentary on the key issue we raise, namely, the benefits of a more unified modeling approach explicitly recognizing that many processes are common to predictions across time scales and the advantages of applying and testing similar models for predictions of both weather and climate. We thus conclude that he agrees with the major tenets of our paper.

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