COMMENTS

Comments on "A Unified Modeling Approach to Climate System Prediction"

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The authors of Hurrell et al. (2009; and that of Palmer et al 2008) should be commended for moving toward a more integrated perspective of climate prediction. The authors of this paper, however, do not present a completely new perspective as they claim in their text when they write, "There is a new perspective of a continuum of prediction problems, with a blurring of the distinction between short-term predictions and long-term climate projection." They, also, unfortunately do not yet accept the implications of their new perspective to multidecadal and longer time period climate prediction.

There is an assumption in this paper (and that of Palmer et al. 2008) that predictions transition from an initial value problem for shorter-term time periods, but for multidecadal forecasts it becomes more of a boundary value problem dominated by the anthropogenic emissions of CO_2 and a few other greenhouse gases. Palmer et al. (2008), for example, write the following:

Figure 1 (in [Palmer's] paper) illustrates schematically some of the implications of such nonlinearity—in our view it is key to explaining the relevance of seamless prediction for studies of climate change. The figure shows a chain. One end of this chain represents humanity's forcing of climate through emissions of greenhouse gases into the atmosphere. The other end of the chain represents the impact of this forcing in terms of regional climate change (temperature, precipitation, wind, and so on).

The intermediate time period (e.g., decadal forecasts) in Hurrell et al. (2009) is hypothesized to be a mix between an initial and boundary value problem. Hurrell et al. write the following:

The errors induced by incorrect initial conditions should become less apparent as the simulations evolve as systematic "boundary" and external influences become more important, but they could still be evident through the course of the simulations.

A good rule of thumb for prediction is that an upper bound on predictability corresponds approximately to one life cycle of the phenomenon being considered. Hence, one could hope to predict a single convective element, cyclone wave, MJO cycle, ENSO warm event, or fluctuation of the Atlantic meridional overturning circulation (MOC) over its life cycle, but not the second-generation event. This rule of thumb is consistent with the climate system being a chaotic dynamical system with limited predictability. Additional predictability, however, could arise from the slowly evolving components of the climate system.

I agree with this perspective that the climate system is a chaotic dynamical system; however, Hurrell et al. (2009), after first correctly recognizing this, ignore this implication for multidecadal climate predictions where the assumption they seem to be making is that the added radiative effect of CO_2 and a few other greenhouse gases from anthropogenic emissions becomes the dominant "boundary" forcing and that skillful predictions on this longer time scale therefore become easier. They indicate this view of the dominance of 2050 and beyond emission scenarios in their Fig. 1 as well as in their selection of the inconsistent terminology "initialized forecasts," "initialized predictions," and "projections."

I discussed this subject more than a decade ago in Pielke (1998), where I wrote the following:

An important practical conclusion results if climate prediction is an initial value problem. This means that there are necessarily limits on the time into

the future that we can predict climate, since the feedbacks between the ocean, atmosphere, and land surface are large and nonlinear. These limits have not been determined, yet climate "predictions" are routinely communicated to policy makers on timescales of decades and centuries. Second, in the context of predicting what the future climate would be in response to an anthropogenic forcing such as carbon dioxide input, there are, as of yet, undefined limits on what aspects of future climate we can forecast even if all the important ocean-atmosphere-land surface feedbacks were included and also accurately represented in the models. This leads to the conclusion that weather prediction is a subset of climate prediction. Societally useful (i.e., reliable, accurate, etc.) climate prediction requires that all of the feedbacks and other physical processes included in weather prediction be presented in the climate prediction model. In addition, longer-term feedback and physical processes must be included. This makes climate prediction a much more difficult problem than weather prediction.

There are also a number of research papers in the past decade that have elaborated on this perspective as summarized in Rial et al. (2004), where we wrote the following:

The Earth's climate system is highly nonlinear: inputs and outputs are not proportional, change is often episodic and abrupt, rather than slow and gradual, and multiple equilibria are the norm . . . . It is imperative that the Earth's climate system research community embraces this nonlinear paradigm if we are to move forward in the assessment of the human influence on climate.

Hurrell et al. (2009) do recognize that skillful climate prediction is a difficult problem. They write that

[f]undamental barriers to advancing weather and climate prediction on time scales from days to years are partly attributable to our limited understanding and capability to simulate the complex, multi-scale interactions intrinsic to atmospheric, oceanic and cryospheric fluid motions.

However, they use the phrase "partly attributable," without identifying what is the remainder of this fundamental barrier.

In Pielke et al. (2009), we discussed some of the added "barriers," where we concluded the following:

Although the natural causes of climate variations and changes are undoubtedly important, the human influences are significant and involve a diverse range of first-order climate forcings, including, but not limited to, the human input of carbon dioxide (CO_2). Most, if not all, of these human influences on regional and global climate will continue to be of concern during the coming decades . . . . In addition to greenhouse gas emissions, other first-order human climate forcings are important to understanding the future behavior of Earth's climate. These forcings are spatially heterogeneous and include the effect of aerosols on clouds and associated precipitation [e.g., Rosenfeld et al. 2008], the influence of aerosol deposition (e.g., black carbon (soot) [Flanner et al. 2007] and reactive nitrogen [Galloway et al. 2004]), and the role of changes in land use/land cover [e.g., Takata et al. 2009]. Among their effects is their role in altering atmospheric and ocean circulation features away from what they would be in the natural climate system [NRC 2005]. As with CO_2, the lengths of time that they affect the climate are estimated to be on multidecadal time scales and longer.

Hurrell et al. (2009) also do not sufficiently recognize that natural longer-term variations in the climate have significant nonlinearities, as illustrated, for example, for drought in Meko et al. (2007) and for arctic climate by Ohashi and Tanaka (2010). Climate undergoes sudden and significant shifts on a wide variety of time and space scales (e.g., Rial et al. 2004), such that the concept of a "cycle" is thus an inaccurate way to explain how the actual climate system works.

Also, what role does "past radiative forcing" play in improving predictability, as claimed in their article? The effect of the past forcing is already in the initial conditions.

Finally, there is a candid admission in their paper that contradicts their text elsewhere in their paper. They write that "quantifying prediction skill becomes even more difficult for decadal and longer time scales" and that "we have no current method to prioritize or weight their [all possible climate metrics] impact in measuring uncertainty in predicting future climate change for temperature, precipitation, soil moisture and other variables of critical interest to society."

I agree with their statement. However, the consequence then is how will their proposed modeling approach satisfy the "[d]emand for more accurate predictions of regional climate" as written by the authors in their abstract?

Thus, although I commend the authors for starting to adopt a framework of climate modeling as an initial value problem, they are at serious risk of overselling what they will be able to provide to policy makers. A significant fraction of the funds they are seeking for prediction could more effectively be used if they were spent on assessing risk and ways to reduce the vulnerability of local/regional resources to climate variability and change and other environmental issues using the bottom-up, resources-based perspective discussed in Pielke and Bravo de Guenni (2004), Pielke (2004), and Pielke et al. (2009). This bottom-up focus is "of critical interest to society."

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