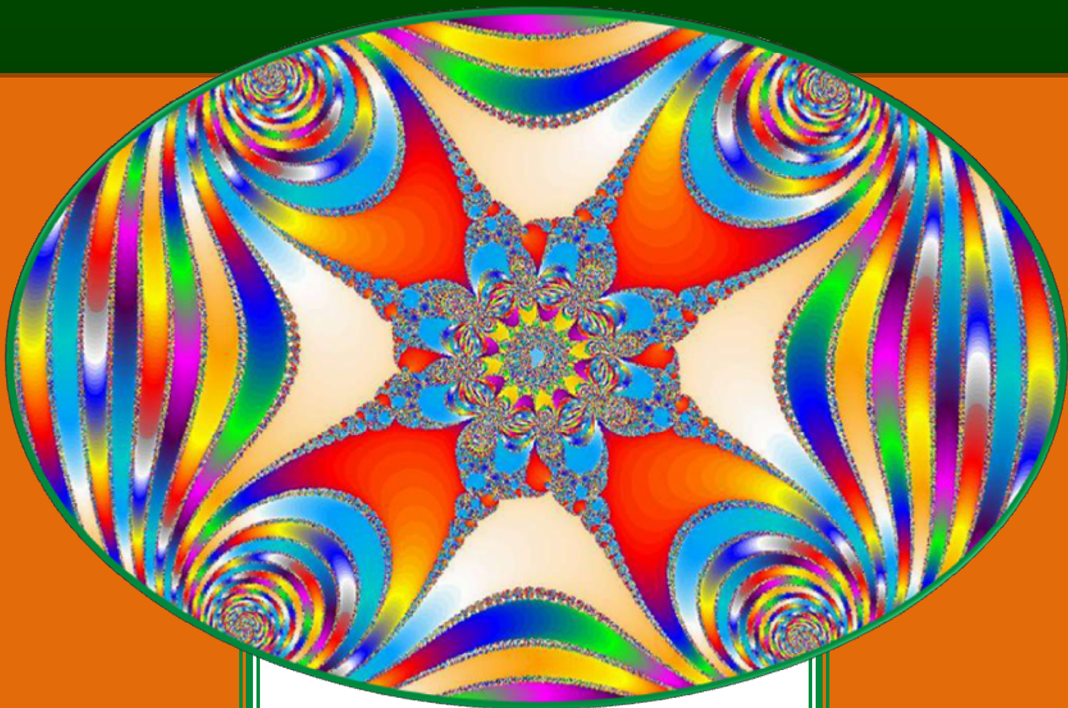
Three Essays on Climate Models



by

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SPPI COMMENTARY & ESSAY SERIES

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Real Climate Suffers from Foggy Perception

Roger Pielke Sr. has graciously invited me to add my perspective to his discussion with Gavin Schmidt at RealClimate. If this were not such a serious matter, I would have been amused by Gavin's lack of knowledge of the differences between weather models and climate models. As it stands, I am appalled. Back to graduate school, Gavin!

A weather model deals with the atmosphere. Slow processes in the oceans, the biosphere, and human activities can be ignored or crudely parameterized. This strategy has been very successful. The dominant fraternity in the meteorological modeling community has appropriated this advantage, and made itself the lead community for climate modeling. Backed by an observational system much more advanced than those in oceanography or other parts of the climate system, they have exploited their lead position for all they can. For them, it is a fortunate coincidence that the dominant synoptic systems in the atmosphere have scales on the order of many hundreds of kilometers, so that the shortcomings of the parameterizations and the observation network, including weather satellite coverage, do not prevent skillful predictions several days ahead.

A climate model, however, has to deal with the entire climate system, which does include the world's oceans. The oceans constitute a crucial slow component of the climate system. Crucial, because this is where most of the accessible heat in the system is stored. Meteorologists tend to forget that just a few meters of water contain as much heat as the entire atmosphere. Also, the oceans are the main source of the water vapor that makes atmospheric dynamics on our planet both interesting and exceedingly complicated. For these and other reasons, an explicit representation of the oceans should be the core of any self-respecting climate model.

> "How, for goodness' sake, can we then reliably compute their contribution to multi-decadal changes in the meridional transport of heat?"

However, the observational systems for the oceans are primitive in comparison with their atmospheric counterparts. Satellites that can keep track of what happens below the surface of the ocean have limited spatial and temporal resolution. Also, the scale of synoptic motions in the ocean is much smaller than that of cyclones in the atmosphere, requiring a spatial resolution in numerical models and in the observation network beyond the capabilities of present observational systems and supercomputers. We cannot observe, for example, the vertical and horizontal structure of temperature, salinity and motion of eddies in the

Gulf Stream in real time with sufficient detail, and cannot model them at the detail that is needed because of computer limitations. How, for goodness' sake, can we then reliably compute their contribution to multi-decadal changes in the meridional transport of heat? Are the crude parameterizations used in practice up to the task of skillfully predicting the physical processes in the ocean several tens of years ahead? I submit they are not.

Since heat storage and heat transport in the oceans are crucial to the dynamics of the climate system, yet cannot be properly observed or modeled, one has to admit that claims about the predictive performance of climate models are built on quicksand. Climate modelers claiming predictive skill decades into the future operate in a fantasy world, where they have to fiddle with the numerous knobs of the parameterizations to produce results that have some semblance of veracity. Firm footing? Forget it!

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Gavin Schmidt is not the only meteorologist with an inadequate grasp of the role of the oceans in the climate system. In my weblog of June 24, 2008,[1] I addressed the limited perception that at least one other climate modeler appears to have. A few lines from that essay deserve repeating here. In response to a paper by Tim Palmer of ECMWF, I wrote:

> "*Palmer et al. seem to forget that, though weather forecasting is focused on the rapid succession of atmospheric events, climate forecasting has to focus on the slow evolution of the circulation in the world ocean and slow changes in land use and natural vegetation. In the evolution of the Slow Manifold (to borrow a term coined by Ed Lorenz) the atmosphere acts primarily as stochastic high-frequency noise. If I were still young, I would attempt to build a conceptual climate model based on a deterministic representation of the world ocean and a stochastic representation of synoptic activity in the atmosphere.*"

From my perspective it is not a little bit alarming that the current generation of climate models cannot simulate such fundamental phenomena as the Pacific Decadal Oscillation. I will not trust any climate model until and unless it can accurately represent the PDO and other slow features of the world ocean circulation. Even then, I would remain skeptical about the potential predictive skill of such a model many tens of years into the future.

[1] http://climatesci.org/2008/06/24/seamless-prediction-systems-by-hendrik-tennekes/.

Seamless Prediction Systems

Roger Pielke gracefully invited me to write a brief essay on an interesting technical detail in the World Summit document issued by WCRP. According to the document, all time scales, from hours to centuries, all regional details, everything related to prediction should be dealt with by GCM technology. In this context, the term "seamless prediction" is used. That caught my attention. Let me quote the relevant paragraph:

> *"Advances in climate prediction will require close collaboration between the weather and climate prediction research communities. It is essential that decadal and multi-decadal climate prediction models accurately simulate the key modes of natural variability on the seasonal and sub-seasonal time scales. Climate models will need to be tested in sub-seasonal and multi-seasonal prediction mode also including use of the existing and improved data assimilation and ensemble prediction systems. This synergy between the weather and climate prediction efforts will motivate further the development of seamless prediction systems."*

The current use of the concept of seamless prediction is explained in a recent paper by Tim Palmer and others, published in the Bulletin of the AMS (see Palmer, T.N., F.J. Doblas-Reyes, A. Weisheimer, and M.J. Rodwell, 2008: Toward Seamless Prediction: Calibration of Climate Change Projections Using Seasonal Forecasts. Bull. Amer. Meteor. Soc., 89, 459–470). I quote:

> *"If essentially the same ensemble forecasting system can be validated probabilistically on time scales where validation data exist, that is, on daily, seasonal, and (to some extent) decadal time scales, then we can modify the climate change probabilities objectively using probabilistic forecast scores on these shorter time scales."*

> *"We propose that if the same multimodel ensemble is used for seasonal prediction as for climate change prediction, then the validation of probabilistic forecasts on the shorter time scale can be used to improve the trustworthiness of probabilistic predictions on the longer time scale. This improvement would come from assessing processes in common to both the seasonal forecast and climate projection time scales, such as the atmospheric response to sea surface temperatures. To reiterate, our basic premise is that processes, such as air–sea coupling, that are relevant for the seasonal forecast problem also play a role in determining the impact of some given climate forcing, on the climate system itself. The calibration technique provides a way of quantifying the weakness in those links to the chain common to both seasonal forecasting and climate change time scales."*

Apparently, the idea behind this application of the seamless prediction paradigm is that the reliability of climate models can be improved if they are used as extended-range weather forecast models. Experimental verification, which is impossible in climate runs, then becomes feasible. With a bit of luck, certain types of shortcomings in the model formulation can be detected this way. This process may lead to climate codes with fewer systematic errors.

> "Climate models cannot be verified or falsified (if at all, because they are so complex) until after the fact. Strictly speaking, they cannot be considered to be legitimate scientific products."

This sounds promising. Climate models cannot be verified or falsified (if at all, because they are so complex) until after the fact. Strictly speaking, they cannot be considered to be legitimate scientific products. Any methodology that would ameliorate this situation would be a step forward, however small and tentative. I am happy to grant Palmer et al the benefit of the doubt as far as this point is concerned.

But I wonder how short-term calibration of a long-term tool might help to unravel the long-period irregularities in the climate system. The original meaning of the term "seamless prediction" was to express the idea that weather forecasting technology can be usefully extended to climate problems. The term was coined to consolidate the monopoly of GCM technology in all kinds of weather and climate forecasting. However, in the paper by Palmer et al it refers to the reverse focus, where calibration is attempted by shrinking the time horizon. Alice gazing through the other side of the looking glass, as it were.

> I consider the seamless use of GCM technology a sign of intellectual poverty.

The tail wags the dog here. I know that dressed-up versions of weather forecast models are used to make climate prediction runs. I don't mind too much, though this methodology hides a chronic, distressing lack of insight in the statistical dynamics of the General Circulation. I consider the seamless use of GCM technology a sign of intellectual poverty. Gone are the days of Jule Charney's Geostrophic Turbulence, Ed Lorenz' WMO monograph on the General Circulation, and Victor Starr's early thoughts on Negative Eddy Viscosity Phenomena.

To turn the matter on its end is one step too far. Short- and medium-term forecast methods work quite well without an interactive ocean, interactive biosphere, interactive changes in the state of the world economy, and the like. I see no reason to burden a weather forecast model with the enormous complexity of climate models, and I see no way in which interactions of subordinate importance in weather forecasting can reliably be calibrated to improve crucial interactions in climate runs. I know I rub against the grain of the GCM paradigm, but so be it.

Palmer et al also seem to forget that, though weather forecasting is focused on the rapid succession of atmospheric events, climate forecasting has to focus on the slow evolution of the circulation in the world ocean and slow changes in land use and natural vegetation. In the evolution of the Slow Manifold (to borrow a term coined by Ed

Lorenz) the atmosphere acts primarily as stochastic high-frequency noise. If I were still young, I would attempt to build a conceptual climate model based on a deterministic representation of the world ocean and a stochastic representation of synoptic activity in the atmosphere.

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One example I am familiar with is the North Atlantic storm track, which guides the surface winds that drive the Gulf Stream and help to sustain the thermohaline circulation in the world ocean. The kind of model I envisage deals with the slow evolution of the ocean circulation deterministically, but with the convergence of the meridional flux of atmospheric eddy momentum in the way turbulence modellers do. In this view, the individual extra-tropical cyclones that feed the momentum of the jet stream can be represented by stochastic parameterizations, but the jet stream itself is part of the deterministic code. In a more general sense, I claim that stochastic tools of the kind proposed by Palmer *et al*. will have to be developed on the basis of a better understanding of the dynamics of the climate system. Purely statistical methods, however sophisticated, can be compared with attempts to kill a songbird with a shotgun.

There is yet another principal shortcoming in the paper by Palmer *et al*. I will grant them that the approach they advocate may be of some use as far as the possible deleterious effects of greenhouse gases are concerned. These gases are rapidly mixed through the entire atmosphere. That's what the turbulence in the general circulation is good at. But now think of slow forestation and deforestation, or the expected northward crawl of corn and wheat belts. And what about large hydropower projects or land-use changes as the peoples of India and China become wealthier, drive more cars, and become more urbanized? Can the reverse use of seamless prediction methods help to calibrate the response of the climate system to these elements of the Slow Manifold? I would not know how.

I offer a solution to Palmer's quandary. Seamless prediction may or may not have a glorious future, but it does have a history spanning almost twenty years. I propose that WCRP should initiate a Seamless Reprediction Program, as a kind of extension to the reanalysis efforts undertaken from time to time at ECMWF. That is, climate runs made in the past should be analyzed, restarted with the latest version of the stochastic feedback paradigm, and calibrated with accumulated observational evidence. Perhaps the latest versions of climate models cannot be investigated this way, but the great advantage is that working in a retrospective mode offers falsification prospects. Looking back, all data needed for calibration do exist. So do the computers and the software. Immediate, large-scale expansion of facilities is not needed if this path is taken. And I trust ECMWF will be permitted to participate in this effort.

Would Palmer not agree that evidence from such a Reprediction Program might turn out to become a cornerstone for the World Climate Computing Facility that he and the World Summit crowd are lobbying for? I wish them well.

A Skeptical View of Climate Models

Here in the Netherlands, many people have ranked me as a climate skeptic. It did not help much that I called myself a protestant recently. I protest against overwhelming pressure to adhere to the climate change dogma promoted by the adherents of IPCC. I was brought up in a fundamentalist protestant environment, and have become very sensitive to everything that smells like an orthodox belief system.

The advantages of accepting a dogma or paradigm are only too clear. One no longer has to query the foundations of one's convictions, one enjoys the many advantages of belonging to a group that enjoys political power, one can participate in the benefits that the group provides, and one can delegate questions of responsibility and accountability to the leadership. In brief, the moment one accepts a dogma, one stops being an independent scientist.

A skeptic, on the other hand, accepts both the burdens and the pleasures of standing on his own feet. One of the disadvantages a skeptic has to cope with is the problem of finding adequate research support. The other side of that coin is that an independent scientist has a great opportunity to think better and delve deeper than most of his or her colleagues. Let me take an example in which I have been involved for thirty years, the problem of a finite prediction horizon for complex deterministic systems.

This, the very problem first defined by Edward Lorenz, still is not properly accounted for by the majority of climate scientists. In a meeting at ECMWF in 1986, I gave a speech entitled "No Forecast Is Complete Without A Forecast of Forecast Skill." This slogan gave impetus to the now common procedure of Ensemble Forecasting, which in fact is a poor man's version of producing a guess at the probability density function of a deterministic forecast. The ever-expanding powers of supercomputers permit such simplistic research strategies.

Since then, ensemble forecasting and multi-model forecasting have become common in climate research, too. But fundamental questions concerning the

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prediction horizon are being avoided like the plague. There exists no sound theoretical framework for climate predictability studies. As a turbulence specialist, I am aware that such a framework would require the development of a statistical-dynamic theory of the general circulation, a theory that deals with eddy fluxes and the like. But the very thought is anathema to the mainstream of dynamical meteorology.

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Climate models are quasi-deterministic and have to simulate daily circulation patterns for tens of years on end before average values can be found. The much more challenging problem of producing a theory of climate forecast skill is left by the wayside. In IPCC-documents one finds phrases like "climate surprises", showing that the IPCC-staff is unaware of the ignorance it reveals by that choice of words, or unwilling to state forcefully that climate predictability research deserves much more attention than it has received so far.

This is no minor matter. A few years after launching my slogan on forecast skill I chanced upon a copy of Karl Popper's "Open Universe" and discovered that Popper had anticipated the problems caused by the Lorenz paradigm. His claim that scientists should be held accountable for the accuracy of their predictions boils down to the requirement that they have to compute in advance the reliability of their computations. For complex models, Popper wrote, this demand leads to "infinite regress": computations of forecast skill are much harder than the forecasts themselves, and the next level, forecasting the skill of the skill forecast, is insurmountable when a complex system such as the climate is involved. Popper concluded that the positivist claims of science are in general unwarranted. In 1992 I wrote an essay for Weather to explain the issue in detail.

The climate orthodoxy perpetrates the misconceptions involved by speaking, as IPCC does, about the Scientific Basis of Climate Change. Since then, I have responded to that ideology by stating that there is no chance at all that the physical sciences can produce a universally accepted scientific basis for policy measures concerning climate change.

Climate skeptics also face a sociological problem. They agree only in their protest against the prevailing dogma. Some base their protest on various versions of the neoconservative paradigm. Bjorn Lomborg, for example, ignores the many efforts of the environmental movement that have contributed to improving conditions in the industrialized world. Speaking scientifically, I submit he has overlooked a crucial social feedback mechanism. Other skeptics use other paradigms. Roger Pielke Jr. bases his work on the vulnerability paradigm, a choice very appealing to me. Lots of outsiders in the climate business employ a supremacy of physics paradigm, attacking one or more of the physical details of the climate problem, and hoping that they can prevail by proving the climate orthodoxy wrong.

In my view, their conceptual mistake is that the physics of complex systems does not provide opportunities for settling the climate debate that way. In 1987, I gave a speech in London entitled "Illusions of Security, Tales of Imperfection". I dealt with the shortcomings of numerical weather forecasting there, but similar arguments apply to climate forecasting. The climate orthodoxy perpetrates the misconceptions involved by speaking, as IPCC does, about the Scientific Basis of Climate Change. Since then, I have responded to that ideology by stating that there is no chance at all that the physical sciences can produce a universally accepted scientific basis for policy measures concerning climate change. In my column in the magazine Weather in February of 1990, I wrote:

> *"The constraints imposed by the planetary ecosystem require continuous adjustment and permanent adaptation. Predictive skills are of secondary importance."*

> "It goes without saying that I abhor such dogmas as various claims to Manage The Planet or Greenpeace's belief in Saving the Earth. These ideologies presuppose that the intelligence of Homo sapiens is capable of such feats."

Today I still feel that way. I cannot bring myself to accept any type of prediction paradigm, and choose a adaptation paradigm instead. This brings me in the vicinity of Roger Pielke Sr.'s emphasis on land-use changes and Ronald Brunner's modest bottom-up alternatives. It goes without saying that I abhor such dogmas as various claims to Manage The Planet or Greenpeace's belief in Saving the Earth. These ideologies presuppose that the intelligence of Homo sapiens is capable of such feats. However, I know of no evidence to support such claims.

Back to Lorenz. Complex deterministic systems suffer not only from sensitive dependence on initial conditions but also from possible sensitive dependence on the differences between Nature and the models employed in representing it. The apparent linear response of the current generation of climate models to radiative forcing is likely caused by inadvertent shortcomings in the parameterization schemes employed. Karl Popper wrote (see my essay on his views):

> *"The method of science depends on our attempts to describe the world with simple models. Theories that are complex may become untestable, even if they happen to be true. Science may be described as the art of systematic oversimplification, the art of discerning what we may with advantage omit."*

If Popper had known of the predictability problems caused by the Lorenz paradigm, he could easily have expanded on this statement. He might have added that simple models are unlikely to represent adequately the nonlinear details of the response of the system, and are therefore unlikely to show a realistic response to threshold crossings hidden in its microstructure. Popper knew, of course, that complex models (such as General Circulation Models) face another dilemma.

I quote him again: *"The question arises: how good does the model have to be in order to allow us to calculate the approximation required by accountability? (...) The complexity of the system can be assessed only if an approximate model is at hand."*

From this perspective, those that advocate the idea that the response of the real climate to radiative forcing is adequately represented in climate models have an obligation to prove that they have not overlooked a single nonlinear, possibly chaotic feedback mechanism that Nature itself employs.

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Popper would have been sympathetic. He repeatedly warns about the dangers of "infinite regress." As a staunch defender of the Lorenz paradigm, I add that the task of finding all nonlinear feedback mechanisms in the microstructure of the radiation balance probably is at least as daunting as the task of finding the proverbial needle in the haystack. The blind adherence to the harebrained idea that climate models can generate "realistic" simulations of climate is the principal reason why I remain a climate skeptic. From my background in turbulence I look forward with grim anticipation to the day that climate models will run with a horizontal resolution of less than a kilometer. The horrible predictability problems of turbulent flows then will descend on climate science with a vengeance.

References

Turbulent Flow in Two and Three Dimensions. Bulletin American Meteorological Society, 59, 22-28, 1978.

The Outlook: Scattered Showers. Bulletin American Meteorological Society 69, 368-372, 1988.

Numerical Weather Prediction: Illusions of Security, Tales of Imperfection. Weather 41, 165-170, 1988.

A Sideways Look at Climate Research. Weather 45, 67-68, 1990.

Karl Popper and the Accountability of Numerical Weather Forecasting. Weather 47, 343-346, 1992.

An Ecological Grammar for Meteorologists. Weather 51, 326-328, 1996.

Sources:

http://climatesci.org/2009/01/29/real-climate-suffers-from-foggy-perception-by-henk-tennekes/
http://climatesci.org/2008/06/24/seamless-prediction-systems-by-hendrik-tennekes/.
http://www.sepp.org/Archive/NewSEPP/Climate%20models-Tennekes.htm.

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