A Skeptical View of Climate Models

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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 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.

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.

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."

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.

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.