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January 31, 2007

A Personal Call For Modesty, Integrity, and Balance by Hendrik Tennekes

Filed under: Guest Weblogs — Roger Pielke Sr. @ 9:18 am

Hendrik Tennekes, retired Director of Research, Royal Netherlands Meteorological Institute, former Professor of Aeronautical Engineering at the Pennsylvania State University and internationally recognized expert in atmospheric boundary layer processes contributes another guest weblog today to Climate Science (see his first weblog on January 6, 2006). He has the professional qualifications and experience in climate science to comment on this issue. His guest weblog is given below

Seventeen years ago, I wrote a column for Weather magazine, expressing my concerns about the lack of honesty, integrity and humility of many climate scientists. "I worry about the arrogance of scientists who claim they can help solve the climate problem, provided their research receives massive increases in funding", reads one line from my text. Unknown to me, my friend Richard Lindzen was working on his famous paper "Some Cooling Concerning Global Warming", which appeared in the Bulletin of the AMS at the same time. This was early 1990. It is 2007 now, and I want to ring the alarm bell again. There is a difference, though: then I was worried, now I am angry. I am angry about the Climate Doomsday hype that politicians and scientists engage in. I am angry at Al Gore, I am angry at the Bulletin of Atomic Scientists for resetting its Doomsday clock, I am angry at Lord Martin Rees for using the full weight of the Royal Society in support of the Doomsday hype, I am angry at Paul Crutzen for his speculations about yet another technological fix, I am angry at the staff of IPCC for their preoccupation with carbon dioxide emissions, and I am angry at Jim Hansen for his efforts to sell a Greenland Ice Sheet Meltdown Catastrophe. Speaking of Hansen, Dick Lindzen and I wrote a lighthearted April Fools' Day parody of his concerns, which was published on Fred Singer's SEPP website (search for Greenland Green Again) last year (view pdf). I can go on much longer, but I will keep my anger in check.

I am more than a little bit worried about IPCC's preoccupation with CO<sub>2</sub>. The scientific rationale behind this choice is obvious. Sophisticated climate models have been running for twenty years now. It has become evident that these models cannot be made to agree on anything except a possible relation between greenhouse gases and a slight increase in globally averaged temperatures. The number of knobs that can be twiddled in the parameterization of the radiation budget is not all that large. Seemingly realistic results can be achieved without much intellectual effort. I agree with IPCC that there is a likely link between fossil fuel consumption and increased temperatures. But this is where the much proclaimed consensus ends. Just one example: the models do not include feedbacks between changing farming and forest harvesting practices and the atmospheric circulation. Partly for that reason, they cannot seem to agree on precipitation patterns. It so happens that precipitation is far more relevant to the world's food production than a slight increase in temperature. I owe this insight to my good friend Denny (Dennis W.) Thomson at Penn State. Like me, he speaks from decades of experience. Denny is the oldest son of a world-renowned arctic lichenologist. He and his wife had the good fortune to grow up on farms in southwestern Wisconsin. Still closely bound to the earth and its delicate ecosystems, they live on a 600-acre farm in Halfmoon Valley on the southeastern flank of Bald Eagle Ridge. A physicist/meteorologist, and former head of Penn State's meteorology department, Denny has witnessed climate change in progress for most of his life. At the same time he is deeply concerned about the veracity of "physics-challenged" climate models.

Why is it so difficult to make precipitation forecasts fifty years into the future? Most precipitation in the middle latitudes is associated with low-pressure systems, which move along storm tracks carved out by the jet stream. The ever-shifting meanders in the jet stream occur at the edge of the slab of cold air over the poles. The specialists call this slab the Polar Vortex, and have christened

the meandering behavior of the jet stream in the Northern hemisphere the Arctic Oscillation. Thirty years ago I worked with Mike (John M.) Wallace and his PhD student N.C. Lau at the University of Washington in Seattle on problems concerning eddy-flux maintenance in the North Atlantic storm track. It is evident to all turbulence specialists that the dynamics of very slowly evolving states is different from the dynamics of instantaneous states. So the moment one asks what keeps the jet stream going, one encounters the kind of problem that is at the core of all turbulence research. But the mainstream of dynamic meteorology refuses to study the slow evolution of the general circulation. It has become so easy to run General Circulation Models on supercomputers that most atmospheric scientists shy away from matters like a thorough study of the interaction between the Polar Vortex and the Arctic Oscillation. Mike Wallace mailed me a year ago, saying that there is not a beginning of consensus on a theory of the Arctic Oscillation. This was one of the highlights in an advanced senior-citizens' class on climate change I taught a year ago. It was announced as "A Storm in the Greenhouse", referring primarily to the increasingly bitter debates of the past fifteen years.

How does this problem affect climate forecasts? If there is not even a rudimentary theory of the Polar Vortex, much less an established relation between rising greenhouse gas concentrations and systematic changes in the Arctic Oscillation, one cannot possibly make inferences about changes in precipitation patterns. We do not know, and for the time being cannot know anything about changing patterns of clouds, storms and rain. Holland's national weather service KNMI circumvented this impasse last year by issuing climate change scenarios with and without changes in the position of the North Atlantic storm track. It did not occur to the KNMI spokesmen that they should have been forthright about their lack of knowledge. They should have said: we know nothing of possible changes in the storm track, so we cannot say anything about precipitation. But it is entirely consistent with the IPCC tradition to weasel around such issues. One of my contacts at KNMI recently explained to me that their choice was based on the increasing agreement between simulations run with different GCM's. I had to answer that the IPCC spirit of consensus apparently was invading their supercomputers as well. It is bad enough that computer simulations cannot be checked against observations until after the fact. In the absence of a robust stochastic-dynamic theory of the general circulation, one cannot even check climate simulations against fundamental insights.

Actually, the monopoly of GCM's in the climate research business is an interesting object of inquiry, and not just for sociological reasons. A GCM is a weather forecasting model in which the coefficients and parameterizations are tuned so as to obtain long-term results that have an air of realism. The model is then run for several tens of years. There are no penetrating studies of the way slight software mismatches might affect the average values of key output parameters fifty years from now. A forecasting model can make do with relatively crude parameterizations because the short-time evolution of the atmospheric circulation is primarily governed by its internal dynamics. Sloppy representations of boundary conditions, clouds, convection, evaporation and condensation do not mess weather forecasts up all that fast. But the long-term evolution of the general circulation is to a large extent determined by boundary conditions. This realization struck me with some force when I discovered last year that a simple algorithm for inversion rise above the daytime boundary layer I conceived in 1973 is still in wide use today. How can one be sure that an ancient forecasting algorithm is capable of performing the task assigned to it in climate models? At times it seems that no one in this business has learned about Karl Popper's falsifiability demand. This is why I cringe at WCRP documents promoting Forecasting at All Time Scales. The obvious purpose of such propaganda is to defend the monopoly position that GCM's have enjoyed for so long. It is strategy, not science. A whole generation of meteorologists is growing up with the idea that this is the only way to go. They were not exposed to Lorenz' WMO monograph on the General Circulation, their faces turn blank when the terms Available Potential Energy and Eddy Kinetic Energy are used. Since they are offered no alternatives, they join those who claim that they need higher resolution and bigger computers. The job of having to think on one's own feet is too hard to contemplate.

All of 2006 I have been corresponding with Tim Palmer, a leading scientist at the European Center for Medium-range Forecasts. The apparent focus of our discussion was the dynamics of vortex filaments around blocking highs. Palmer intuited that thin sheets of positive relative vorticity around a negative-vorticity core may serve to prolong the life of a high-pressure system. I felt this was an interesting hypothesis. For many years I have ridiculed the phraseology in which blocking highs are said to divert storms coming their way. More than once I have explained to a reporter that it would be equally appropriate to state that diverging storms sustain a blocking high.

Then came the rub. Thin vortex filaments can be simulated on a supercomputer only if the horizontal resolution is much improved. With the current mesh size of the ECMWF model at 40 kilometers if I am not mistaken, simulation of the vorticity microstructure in the troposphere would require a 10,000-fold increase in computer power. So this is how the propaganda for petaflop computing emanating from WCRP comes about, I thought. One hundred computers of the generation following the next would indeed generate the desired increase. This in turn would require a facility on the size of CERN, ITER, or the preposterous Superconducting Supercollider.

Is this what John Houghton, Bert Bolin, Martin Rees, the IPCC staff and the like are aiming for? I have parted the company of these power brokers many years ago, so I cannot begin to imagine what they are up to this time. Palmer has convinced me he is not their puppet, fortunately. We continued our correspondence. “So you’re really lobbying for a massive computer facility”, I wrote, “you participate in the same song and dance that has annoyed me for so long”. In my years as Director of Research at KNMI, the scientists around me honestly felt that my only job was to promote the early purchase of the next supercomputer. They were eager to collude behind my back with the hardware crowd at KNMI and salesmen from computer manufacturers. This often resulted in seemingly attractive discounts being offered around October, just when the salesmen had heard through the grapevine that a budget surplus would soon be reported to the Management Team.

I might have been sympathetic to Palmer’s ideas if he had argued in favor of a much better representation of ocean eddies, or the atmospheric boundary layer, or the climatic effects of changing farming practices. The dynamics of storm tracks and blocking highs is only one of many interactions demanding more research. It is certainly not appropriate to focus much climate computer power on just this one issue. That can be done better on specialized computers. In the case of blocking highs, a forecasting computer would fit best, because it is dedicated to the internal dynamics of the atmosphere. In my mind, a sense of balance was missing in Palmer’s appeal.

I want to lobby for decency, modesty, honesty, integrity and balance in climate research. I hope and pray we lose our obsession with climate forecasting. Climate simulations are best seen as sensitivity experiments, not as tools for policy makers. I said it in 1990 and I am saying it now: the constraints imposed by the planetary ecosystem require continuous adjustment and permanent adaptation. Predictive skills are of secondary importance. We should stop our support for the preoccupation with greenhouse gases our politicians indulge in. Global energy policy is their business, not ours. We should not allow politicians to use fake doomsday projections as a cover-up for their real intentions. If IPCC does not come to its senses, I’ll be happy to let it stew in its own juices. There is plenty of other work to do.

In 1976, Steve (Stephen H.) Schneider published a book entitled *The Genesis Strategy*. It made quite an impact on me at the time, primarily because Schneider did not promote technological fixes, but a global strategy of what is now called Adaptation, an idea reluctantly and belatedly embraced by IPCC. Those were the days of Nuclear Winter, weather modification, Project Stormfury, stratospheric ozone destruction, and the sick idea of seeding all Arctic ice with soot to prevent the next ice age. In the preface to his book, Schneider quotes Harvey Brooks, then Harvard dean of engineering:

“Scientists can no longer afford to be naïve about the political effects of publicly stated scientific

opinions. If the effect of their scientific views is politically potent, they have an obligation to declare their political and value assumptions, and to try to be honest with themselves, their colleagues and their audience about the degree to which their assumptions have affected their selection and interpretation of scientific evidence”.

I rest my case.

« Correction To Misstatement On My Views On Climate In A USA Today Article Entitled “Fossil fuels are to blame, world scientists conclude” The Difference Between Global Warming And Climate Change »  
32 Comments »

1.

Professor Tennekes: Thank you again for boldly speaking out. One thought in reading your comments.

It occurs to me that there is now a unique opening for some innovative researchers to break new ground in the science of weather and climate. The GCM craze will continue to gather a mob of adherents for a while. This is how we humans behave. Why join the crowd? Why rehash the same old rote line of research that the mob is following? Money in research is sometimes over-rated. What counts is a great hypothesis, a smart scientist, and the tenacity to pursue the idea with perseverance. It seems like a great time to begin knocking over old paradigms in the field of climate and weather in fundamental new ways. In the long run, the worst result of the current IPCC philosophy may be that it served as a temporary distraction to real scientific progress. Its now time for some new ideas!

Comment by Bryan Sralla — January 31, 2007 @ 2:27 pm

2.

An eloquent essay explaining GIGO.

Bigger models using faulty and incomplete algorithms run on faster computers will have the ability to produce garbage... faster.

Comment by Bruce Hall — January 31, 2007 @ 4:28 pm

3.

Re #1: I'm all ears!

Comment by Steve Bloom — January 31, 2007 @ 4:36 pm

4.

It's a problem of “crying wolf!” too many times. Journalists don't mind contradicting themselves from one day to the next, but the public will eventually get tired of the wildly flailing irrational hysteria that typifies most climate pronouncements in the public media.

When it becomes clear that serious errors have been made in the computer models, will the public and elected representatives be in a mood to listen, after all the hype?

Comment by Stephen Blum — January 31, 2007 @ 4:45 pm

5.

Dear Dr Tennekes,

Please publish this also in dutch.

Hans Erren  
The Netherlands

Comment by Hans Erren — January 31, 2007 @ 6:54 pm

6.

Re #3: Gosh Steve, I'm sure you would agree it would be nice if the NASA's GISS MODEL E didn't need to solve some of their computational difficulties by introducing unphysical longitudinal diffusivity for the velocity poleward of 80 degrees latitude. Right? (Particularly since the unphysical diffusivity can be as large as  $10^{**7} \text{ m}^{**2} \text{ s}^{**-1}$ , which is only, oh, 12 orders of magnitude larger than the actual kinematic viscosity of air).

The need to introduce that particular unphysical patch appears to be motivated by numerical considerations only.

But wouldn't it be nice if the GCM codes didn't need to rely on closures that didn't gloss over poorly understood physical processes either?

The fact is, the closures used in GCM codes appear to be comparable in sophistication to the types of closures commonly used in 1980's era CFD codes which were (and sometimes still are) widely used in the engineering community. Doubtless the current GCM models "work", in the same sense that late 1980s to mid 90s era CFD codes "worked."

Doubtless the codes can hindcast — the way 80-90s era CFD codes could hindcast.

But why would people familiar with both the good and bad points of 80s-90s era CFD codes be confident in detailed predictions spit out by codes containing 80s-90s era closures and parameterizations?

I for one would like to see code prediction checked both against the available data and against the behaviors we expected based on fundamental insights about climate dynamics. Checking code predictions against behaviors expected based on fundamental insights has always been one of the most effective ways to test codes. Why should we dispense with it now?

Comment by Margo — January 31, 2007 @ 9:40 pm

7.

Hear here.

What amazes me is the degree to which the carbon traders would hobble the economy, and how unaware climate scientists are of how much they are being taken advantage of.

Re #3, You don't get it.

Comment by Timothy Clear — January 31, 2007 @ 11:05 pm

8.

Hans Erren (#5), I was civil enough to mail an advance courtesy copy of my essay to the Director of KNMI. I trust he will be civil enough to have his staff translate my essay and publish it on the KNMI website. You may wish to lobby with the PR staff at KNMI that this be done.

I have also mailed advance copies to a few prominent Dutch science reporters (Karel Knip and

Simon Rozendaal). As far as media publicity is concerned, the choice is now theirs.

Comment by Henk Tennekes — February 1, 2007 @ 4:27 am  
9.

re 8:  
thank you

re 6:  
Is CFD computational fluid dynamics?

I get a bit worried when I see the global annual temperature ensemble model run for the 20th century ending up warmer than the observations, and this then being accepted as a valid model.

See my discussion on the ccs3.0 model at climateaudit.  
<http://www.climateaudit.org/?p=1103>  
(The site is having difficulties due to strong hit rate)

Comment by Hans Erren — February 1, 2007 @ 9:07 am  
10.

Steve (#3) and Margo (#6), allow me to add a few words to your dialogue. The diffusivity used in weather and climate models is not intended to replace the kinematic viscosity of air, but the eddy viscosity of the subgrid-scale motion. This is pretty primitive, but better than nothing. The main advantage of this approach is that the dissipation of kinetic energy by small-scale motions can be crudely simulated. The main disadvantage, however, is that destabilizing feedbacks between the subgrid-scale stuff and the large-scale motion are not represented at all. In fact, the knobs of the diffusive parameterization often are turned up quite a bit, just to make sure that the simulations do not run out of hand. A GCM does not operate in air, but in syrup.

Incidentally, you can find a comprehensive exposition of the stochastic dynamics of a very much simplified atmosphere in my 1977 J. Atmos. Sci. paper "The General Circulation of Two-dimensional Turbulent Flow on a Beta Plane". You can download it for free from the AMS website.

Also, some of the problems associated with having to deal with subgrid-scale motion are described in my 1978 Bulletin AMS paper entitled "Turbulent Flow in Two and Three Dimensions".

Comment by Henk Tennekes — February 1, 2007 @ 9:38 am  
11.

Re 9: Yes. CFD is computational fluid dynamics. Sorry for not being explicit.

Dr. Tennekes: Thanks!

When I was commenting on the "unphysical" aspect of some model, I wasn't really worried that eddy diffusivities might be used. Yes, those aren't perfect— but I can deal with eddy diffusivities provided someone tries to come up with a suitable velocity scale ( $u'$ ) and length scale ( $l'$ ) to create a diffusivity ( $D \sim C$ , with  $C$  some sort of fiddle factor.)

I didn't really elaborate on what I meant by "unphysical". I'll do so now. I'll be referring to some stuff in a paper available here: ([http://pubs.giss.nasa.gov/abstracts/2006/Schmidt\\_etal\\_1.html](http://pubs.giss.nasa.gov/abstracts/2006/Schmidt_etal_1.html))

On page 159, you will see text discussing modeling compromises used to deal with numerical problems associated with the poles. Then, you'll read this:

In Model E we apply a longitudinal diffusion directly to the velocity field at latitudes poleward of  $\sim 80$ , to eliminate these instabilities. The diffusion acts in addition to the velocity filtering employed at all latitudes to remove two point grid noise. The value of the diffusion coefficient  $K$  depends upon zonal wind speed. In velocity rows for which the maximum zonal Courant number is less than  $1/2$ ,  $K$  is set to  $10^3 \text{ m}^2/\text{s}$ , a value that required  $t = \Delta X^2 K^{-1} \sim 1$  day [blah blah] .... As the Courant number increases from  $1/2$  to unity,  $K$  increases linearly from  $10^3 \text{ m}^2/\text{s}$  to a maximum of  $10^7 \text{ m}^2/\text{s}$ . {blah, blah...}

Now, what I'm seeing here is a diffusivity that is introduced not for physical reasons, but solely to eliminate numerical instabilities. The diffusivity is not based on any length scale of velocity in the flow — it's based on the Courant number in the grid box. The diffusivity ranges from really big to whopping enormous. Is  $10^7 \text{ m}^2/\text{s}$  syrup or tar?

I could possibly even deal with this non-physical fudge — if I saw text to explain why one knows it's of little importance. (And if I believed the explanation.)

Anyway, that's what I meant by "unphysical".

Out of curiosity though, do you know if there are any "theory manuals" for some of these codes? The peer reviewed papers I've found so far are light on describing the actual physical models. They point to other papers that are similarly light... and so on. Presumably NASA has theory manuals, which, you would think would be cited in the peer reviewed papers, but I'm either not seeing or not recognizing the relevant citations. (In fairness to the code guys, I haven't looked far and wide. I'm just hoping you might point me in the right direction.)

Thanks.

Comment by Margo — February 1, 2007 @ 1:32 pm  
12.

Margo (#11): I am sorry, but I can't help you there. The peer reviewed literature abounds with papers that make no mention at all of the codes employed. And I have never seen papers that discuss the pros and cons of the various parameterizations (this is why the wide-spread use of my 1973 inversion-rise algorithm made me so uneasy). Most often, the authors merely borrow one of the big models from NCAR, GISS, Hadley Center or so, and base their papers on simulations run with software made by others. As a scientist raised in ancient times, I find this indigestible. How can one do science without having intimate knowledge of the tools employed? Denny Thomson put it succinctly in a recent e-mail: "I oftentimes feel that our modern model mechanics are filling their toolboxes before they know whether or not the task before them is one of carpentry or car repair".

Perhaps someone else participating in this discussion can help us out. Anyone out there?

Comment by Henk Tennekes — February 1, 2007 @ 2:15 pm  
13.

Henk,  
I'm in my 40s, and I also don't know how you can do science or engineering without knowledge of the tools employed.

I'm also a bit puzzled reading these papers in J. Climate and Weather because, for the most part, if similar papers were appearing in J. Fluid Mech. or Phys Fluids, or any of the Journal published by any engineering society (like AICHE, ASME, AIAA etc.) we'd likely see some equations describing mathematical representations of the physical processes that are actually contained in the

codes.

If you don't include equations, how in the heck do the peer reviewers even know what you did?

Is there some great leap that says when codes are used by climate modelers it's ok to just say nothing more than "The runs described here use a second-order scheme for momentum equations" and nothing more about conservation of momentum?

Comment by Margo — February 1, 2007 @ 3:09 pm  
14.

Here, a link for LMDZ (French model, Laboratoire de Météorologie Dynamique, used in AR4 I guess). Some files are codes, but unfortunately, the chapter "physical parametrisations of LMDZ4" (filetype: phylmd) is still under construction. I don't think it will be helpful, but who knows? (Climate computing is not my cup of tea, I probably misrepresent what you search).

<http://www.lmd.jussieu.fr/~lmdz/manuelGCM/main/node1.html>

Comment by Charles Muller — February 1, 2007 @ 4:34 pm  
15.

Professor Tennekes,

I applaud your stand. I got my introduction to boundary layers at a short-course you participated in teaching back in the 1970's. I am embarrassed by the lack of courage on the part of our profession for not standing up to the foolish confidence displayed in the IPCC, the over the top statements by ecologists, and the ingratiating comments of some of our peers fawning over Al Gore and his fantasy world.

I don't understand why real atmospheric scientists, who must have been humbled like I have been throughout my career on the complexity of the behavior of even simple atmospheric systems(e.g. McNider et al 1995 J.Atmos.Sci., can tolerate this confidence.

Your mention of Lorenz' Monograph brings to mind the original reasons for GCM's - they were constructed to see whether a computer model could capture the first order behavior of the global circulation system. Could they reproduce the mean circulation? Could they replicate the eddy flux and support the indirect circulation of the westerlies? They were not meant to be tuned to reconstruct an observed (and suspect) global temperature time series at the expense of all the other things we might learn from such a tool.

While you emphasize the shortcomings of models with good cause(I am a modeler), the observed surface temperature record and its use in detecting climate change is fraught with concerns. I helped spur the satellite temperature record produced by Spencer and Christy. Our original reason for trying to develop another and perhaps more robust measure of the global temperature was spurred by a consideration of all the difficulties of making measurements near the surface. Surface temperature observations can be influenced by the many things that impact the local surface energy budgets and the associated turbulence around surface sites. You cannot begin to imagine the personal and professional attacks this data set(which shows only modest warming) has generated by the keepers of the surface data set and the global warmers who use this contaminated and non-representative data set to advance their cause.

I hope your essay can be given wide distribution but I am skeptical that the keepers of the journals will let it pass. However, your stand is timely and I hope it will spur me and others to write similar discourses and stand up for our profession.

Comment by Richard McNider — February 1, 2007 @ 8:25 pm

16.

Thanks Charles! Qualitatively, that's the sort of thing I'm looking for. As you note, the closure page is "under construction". Since it's dated 2004, I'm not expecting anything to appear any time soon.

Still, it looks promising. Maybe that LMD will have a pdf version of the theory manual. Thanks!

Comment by Margo — February 1, 2007 @ 9:48 pm

17.

Thank you for your encouragement, Richard (#15). I am sure you are aware that Roger Pielke also feels the globally averaged surface temperature is a poor metric for global warming. I agree.

As to a wide distribution for my essay, I have submitted it to Steve Schneider for publication in Climatic Change. Schneider decided to publish the papers by Hansen and Crutzen, after all.

Your remarks on the original idea of using GCM's as a tool for investigating the stochastic dynamics of the General Circulation are very much to the point. It was in that spirit that I wrote my 1977 paper on the general circulation of two-dimensional turbulence (see # 10). That paper has an exciting genesis. My turbulence class at the University of Washington was attended by most of the atmospheric science faculty there; the graduate students got a first-hand experience of professional debate with power houses like Jim Holton, Dick Reid, and Mike Wallace. I had invented the model as an educational tool, but still felt it was worth publishing.

The principal shortcoming of my simplistic GCM is that it does not have a plausible analogue for concepts such as Available Potential Energy or Potential Vorticity. You are a modeler: dare I challenge you to find an alternative? If nothing else, it would soon find its way in all general-circulation courses worldwide.

Comment by Henk Tennekes — February 2, 2007 @ 1:57 am

18.

Professor Hendrik Tennekes' anger is balsam for many having difficulties with the performance of IPCC. I have high sympathy with his constraint in putting too much believe in the capability of improved computers, but wonder that he seem to have done little to look for cases to proof the modellers 'naïve'. During the last century occurred ample 'climatic events', all fairly well recorded. The freaks on computer modelling have to prove their capability on such events. Recently I raised the point as follows:

With some interest I followed the discussion on: "calibrate' the model " with data from past climate, wondering whether it is worth to argue about it a lot. It will not work. I have mentioned above frequently (#53 etc) the large 'field experiments' by naval war during WWI and WWII. If 'models' are not able to handle such very confined and decisive events (winter season in semi enclosed seas), they can surely not handle slowly 'creeping' events. If they cannot simulate the "swirling about in a baby pool", #33, neither model why "the WWI and WWII war winters in England had been the snowiest since observation", #16, nor model the big warming at Spitzbergen in winter 1918/19, #41, there is no need to try calibration of models with previous 'climate'; see: #77 <http://climatesci.colorado.edu/2007/01/01/a-breath-of-fresh-air-in-the-media-on-climate-an-interview-and-article-by-andy-revkin>

Asking IPCC to demonstrate that they are able to run models on recent climate events and

explain the ‘natural physics’ behind the forcing is a very promising way to get them ‘back on earth’.

Comment by Adrienne M — February 2, 2007 @ 3:24 am  
19.

A few observations from my very limited experience with models and attribution of causes using them.

The capabilities of computers have grown fast. The availability of computer power has grown as well. I started my studies of physics engineering in 1992 and since then what used to be “big number crunchers” are dwarfed by PC’s.

This is a good thing in general for science, as tedious work like computing transformations of data, doesn’t take as much time as it used to. I’ve noted some side-effects as well.

Some of my former colleagues used CFD models to try and figure out what was going on in laser systems. I remember that there were many discussions at the computer screen when the results came in. What astonished me was the wide variety of output as a result of different “gridding” of the areas of interest, and the almost-but-not-quite arguments that resulted from this lack of robustness.

What stuck in my mind though was that computers output is often considered as “truth”, and one easily forgets that it is just the outcome of a set of assumptions about the (modeled part of the) world.

Another thing that stuck in my mind because of this and other experience is that one easily loses track of basic fundamental theory if one has access to computer power and large datasets. Note that one can generate results that look passable by running a sufficiently complex model and not mentioning what the fundamental principles and assumptions are that govern the equations solved.

So it seems that I’m an equation fundamentalist. Prof. Tennekes seems to be too as thinking on your own two feet is only possible by letting go of computers (even if just for a while) and looking at what the system is really like.

Then again, I don’t know anything about meteorology, climatology, forecasting, atmospheric science or computational fluid dynamics...

Comment by Florens de Wit — February 2, 2007 @ 4:26 am  
20.

The House on the Sand went.....

With prolonged fanfare the UN panel on global warming IPCC, has finally released the 21 page “Summary for Policymakers”. After telling us what to do, then sometime this summer they will explain why in the rest of 400 page report. Insiders say there....

Trackback by rob — February 2, 2007 @ 8:55 am  
21.

My personal view is that every morning, before they start work, every climate modeller should take the time to spend a few minutes looking at the view of the earth from space. They should, for example, log onto the EUMETSAT website at [http://www.eumetsat.int/Home/Main/Image\\_Gallery/Real-time\\_Images/index.htm](http://www.eumetsat.int/Home/Main/Image_Gallery/Real-time_Images/index.htm) and run the Image Gallery Real-time Images 24 frame, 3 hour time lapse movie.

Here they can watch the vertical sun point “the solar pole” as it burns its way west through the atmosphere at supersonic speed, defining the seasonal locus of the ITCZ. Notice how the Andes, the

world's largest meridional barrier, blocks the westward progress of the afternoon cumulonimbus storms as they ceaselessly make their vain attempt to catch up with the speeding sun. Observe the bow shockwave tracks of the tropical jets as they define the upper-tropospheric exhaust trajectory emanating from the trapped storm systems in Amazonia.

They should notice how the cold polar air acts like a dynamical atmospheric wall that blocks to northward progress of the mid-latitude cyclones. Observe too how this same cold polar air regularly bursts equatorward as a basal surface density current, generating a complementary pair of gyres, cyclonic on its eastern flank, and anticyclonic (the so called blocking high) on its western flank. Observe how in Eurasia the latitudinal mountain belts of the Alpine - Caucasus - Himalayan chain block this equatorward flow and accentuate the gyres in a positive feedback loop. Notice too the vigorous katabatic outflows from the icecap, freeze drying the Norwegian Sea and morphing into the polar maritime air mass that carries winter cumulus south into Europe.

Then they should stop and think. This complex, dynamic variable system is the basis for all the world's climates. Is my hubris really sustainable? Should I not do everyone a big favour and pull out the electrical plug?

Comment by Philip Mulholland — February 2, 2007 @ 10:41 am  
22.

RE: #21 - For anyone, who like myself, truly loves the Earth in a way that is not artificially buoyed by the quasi religious froth of Gaia worship, Philip's words here bring a tear to one's eye. Bravo! Some of the best descriptive scientific writing I've seen in a very long time!

Comment by Steve Sadlov — February 2, 2007 @ 2:18 pm  
23.

Philip Mulholland (#21), in my mind your eloquence equals that of the author of the Old Testament book of Job, where the Lord himself gives the poor guy a thundering lesson in humility:

“Where were you when I laid the foundations of the earth? Tell me if you have understanding! ... Have you entered the storehouses of the snow, or have you seen the storehouses of the hail, which I have reserved for times of trouble? ... Has the rain a father, or who has begotten the drops of dew? ... Can you lift up your voice to the clouds, that a flood of waters may cover you? ... Who has put wisdom in the clouds?

These are fragments from chapter 38, RSV. Not all that much has changed in 3000 years. Scientific hubris clouds the issue. We have not the foggiest idea of the prediction horizon of climate models. With globally averaged temperatures slowly declining since 1998, the Doomsday hype may soon be over.

Comment by Henk Tennekes — February 2, 2007 @ 8:42 pm  
24.

The IPCC is pulling out all the stops. In their new report they say “The carbon dioxide radiative forcing increased by 20% from 1995 to 2005, the largest change for any decade in at least the last 200 years”.

How's that for a lack of integrity? “At least the last 200 years” throws the anthropogenic concept out, and discounts that 280 ppm had any ghg effect. The carbon traders are getting desperate.

Comment by Timothy Clear — February 2, 2007 @ 11:52 pm

#23's reference "Tell me if you have understanding", brought back to my mind a 14 years old article concerning IPCC, which is timely due to the gathering of IPCC in Paris now, and hopefully finds a wider interest:

#### WARMING UP—SCIENCE OR CLIMATE

The climatic change issue has recently become one of the most serious challenges facing humankind. As L.O.S. Lieder insists on brevity, even though this issue deserves to be discussed at length, I beg your forgiveness for formulating my thesis directly and perhaps somewhat dramatically: climatic specialists and those people who have contributed to recent debates are 'possibly as much of a threat to the climate as the pollution caused by industrialization. For almost one hundred years, science has failed to realize that climate and the oceans are one and the same thing. As a result, the 1982 U.N. Convention on the Law of the Sea, the only true treaty dealing with climatic change issues, was thwarted the moment it came into effect over ten years ago.

Although climate should long ago have been defined as "the continuation of the ocean by other means," the Framework Convention on Climate Change of June 1992 came up with an alternate definition: "The totality of the atmosphere, hydrosphere, biosphere and geosphere and their interactions." What this all boils down to is that climate is nature working in all its forms — a nonsensical definition and useless as a basis for legal regulations.

As recently as 1990, the Intergovernmental Panel on Climate Change (IPCC) came to the conclusion that CO<sub>2</sub> was altering the climate and that "understanding and detecting the earth's climate system must surely be the greatest scientific challenge yet to be faced by humankind. It is a worthy banner under which the nations of the world can unite" (IPCC, Working Group I, p. 328). Certainly not a bad thing for science. The 1992 Earth Summit resulted in an unprecedented success for the scientists working in the climatic area, forcing politicians to listen to them and paving the way for greater financial backing in an effort to understand and come to terms with the climate system.

Yet, what is good for scientists is not necessarily good for the climate. The simple fact of the matter is that meteorology has never been particularly interested in climate except for statistical purposes, defining it as the average weather over a given period of time. On the other hand, there are the mathematicians, physicists and chemists, who do little more than apply their laboratory findings, theoretical conclusions and abstract calculations performed on greenhouse gasses to a real natural system with little regard for the true essence of climate.

But while the seas continue to influence the climate, science is staring into the air (or, to be more precise, the atmosphere) in an attempt to find out what makes the climate tick. What is more, scientists have misled the international community of nations by claiming that greenhouse gases are the actual cause of climate change. This may yet prove to be the real tragedy of the climate change issue. After all, the oceans are still the part of the world about which the least is known. There is neither an "inventory" of the oceans nor an observation system. What is even sadder is that climate is still far from being acknowledged as the blue print of the oceans.

So beware of IPCC's call for unification in its attempt to come to terms with the climate. The climatic change issue is far too serious a matter to leave to those who should have known better for many decades and who were not interested in or aware of matters relating to the oceans. It is high time to enforce what is by far the best convention for understanding and protecting the climate—the 1982 U.N. Convention on the Law of the Sea—before it is too late. After all, it is the first global constitution and would therefore compel humankind to ensure that the planet remains a place worth living in. There is no need to "detect the earth's climate" and even less is there a need for a banner to serve IPCC's "greatest scientific challenge."

Text by Arnd Bernaerts, in: Professional correspondence from the Law of the Sea Institute William S. Richardson School of Law, University of Hawaii, LOS Lieder #28 Vol. 6, No. 5, January, 1993 (available at [www.oceanclimate.de](http://www.oceanclimate.de))

Comment by Adrienne M — February 3, 2007 @ 6:41 am  
26.

Oh my God, Adrienne (#25), you're so right!

A few more quotes, now starting with verse 1 of Chapter 38:

“Then from the heart of the tempest, God gave Job his answer: Who is this, obscuring my intentions with his ignorant words? ... I am going to ask the questions, and you are to inform me. Where were you when I laid the Earth's foundations? Tell me since you are so well-informed! ... Have you been right down to the sources of the sea and walked about at the bottom of the abyss?”

This episode ends in the first few verses of Chapter 40, where Job replies:

“My words have been frivolous: what can I reply? I had better lay my hand over my mouth. I have spoken once, I shall not speak again. I have spoken twice, I have nothing more to say”.

These quotes are from the New Jerusalem Bible, which I purchased today since my old RSV is coming apart. For those of you not raised in the Judeo-Christian tradition, the book of Job contains a long, circuitous, multi-layered discourse on crime and punishment, worthy of Dostoevski. That dialectic is abruptly replaced by the dialectic between hubris and ignorance. High drama indeed, exactly the drama that now is playing in the climate change theater.

All of you who participate in this discussion: now you know the ultimate source of my humility essay.

Comment by Henk Tennekes — February 3, 2007 @ 11:07 am  
27.

“Re #1: I'm all ears!

Comment by Steve Bloom”

No you are NOT Steve. But try this on for size anyway.

<http://graamebird.wordpress.com/2007/01/27/continental-layout-and-ice-ages/>

Comment by Graeme Bird — February 3, 2007 @ 2:07 pm  
28.

I thoroughly enjoyed reading the debate on this blog. I am impressed the way the contributors communicate. I regularly read the discussions on RealClimate.org and dislike their self-righteous or nasty tone (the sarcastic comment by Steve Bloom, is a typical example of that). As a (retired) geologist I cannot follow all of the arguments presented by Dr Tenneses but get enough of an impression that the science is complex and that the consensual conclusions of the IPCC have to be treated with suspicion.

I have noted that there are many scientists that over the years have disagreed with the IPCC reports, how then is it possible that the IPCC can come up with conclusions that are a display of widespread consensus? Who is responsible for selecting the scientists that contribute to these studies. For instance I have yet to come across a geologist who is not sceptical of all the alarmist predictions. I guess, geologists are used to a timescale beyond that of the normal human horizon.

Who are the geologists that the IPCC is relying on? Is the IPCC at all concerned about the frequency and recurrence of ice ages? Who are the astronomers that advise the IPCC on other cause of possible climate change (sun spots or earth's elliptical orbit, tilt and wobble of its axis) so as to ascertain that we are not just experiencing a normal trend related to interglacial warming or variation in solar radiation?

As a Dutchman living in Australia I have been fortunate enough to have several of my letters to the editor on the subject of climate change published in the national newspaper The Australian. It is a newspaper with a good reputation and open to arguments from both sides of the climate change hypothesis divide. I believe that an op-ed submission by Dr. Tennekes would be highly appreciated.

Comment by Chris Schoneveld — February 3, 2007 @ 10:36 pm  
29.

Chris - Thank you for your contribution to the Climate Science weblog.

On your excellent question

“I have noted that there are many scientists that over the years have disagreed with the IPCC reports, how then is it possible that the IPCC can come up with conclusions that are a display of widespread consensus? Who is responsible for selecting the scientists that contribute to these studies”;

the answer is that the same relatively small group are selecting, writing and reviewing the assessments. The process has become so inbred that I do not think many of the IPCC scientists even realize the blatant conflict of interest that exists. This has prevented truly independent assessments of the role of human climate forcings by the IPCC and other climate assessments, such as I have documented in

Pielke Sr., Roger A., 2005: Public Comment on CCSP Report “Temperature Trends in the Lower Atmosphere: Steps for Understanding and Reconciling Differences”. 88 pp including appendices.

Comment by Roger Pielke Sr. — February 4, 2007 @ 12:13 am  
30.

Chris Schoneveld, I have a response to your suggestion. My comment #27 in the discussion following Roger Pielke's blog of 18 January is a ready-made op-ed piece as it stands. You are welcome to submit it to the Australian on my behalf. You could also use it as a lengthy quote inside another op-ed piece by your own hand. I grant you permission to act as you see fit.

Also, thanks for your friendly words. I agree with you: the discussion here is civilized, courteous and mature.

Comment by Henk Tennekes — February 4, 2007 @ 12:20 am  
31.

Chris.

The IPCC doesn't even use a solar forecast in their predictions. They don't have a computer model that backtests. They don't seem to entertain any geological analysis into what leads to ice ages and warmer periods.... But other than that and their other handicaps in the looks, competency and character departments, they appear to be an alright bunch.

Good to see such a plain-speaker as yourself from my country.

Comment by Graeme Bird — February 4, 2007 @ 12:52 am  
32.

[...] February 5, 2007 | I want to lobby for decency, modesty, honesty, integrity and balance in climate research. I hope and pray we lose our obsession with climate forecasting. Climate simulations are best seen as sensitivity experiments, not as tools for policy makers. I said it in 1990 and I am saying it now: the constraints imposed by the planetary ecosystem require continuous adjustment and permanent adaptation. Predictive skills are of secondary importance. We should stop our support for the preoccupation with greenhouse gases our politicians indulge in. Global energy policy is their business, not ours. We should not allow politicians to use fake doomsday projections as a cover-up for their real intentions. If IPCC does not come to its senses, I'll be happy to let it stew in its own juices. There is plenty of other work to do. Hendrik Tennekes, retired Director of Research, Royal Netherlands Meteorological Institute [...]

Pingback by Climate Humility : Jay Currie — February 4, 2007 @ 7:18 pm