Isaac Held's
Climate Dynamics Blog (2011-2016)

Isaac Held

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Science is not a circle with a single center but an ellipse with two focal points of which facts are one and imagination is the other. (With apologies to Victor Hugo.)

Contents

1 Introduction

[Originally posted on Feb 17, 2011]

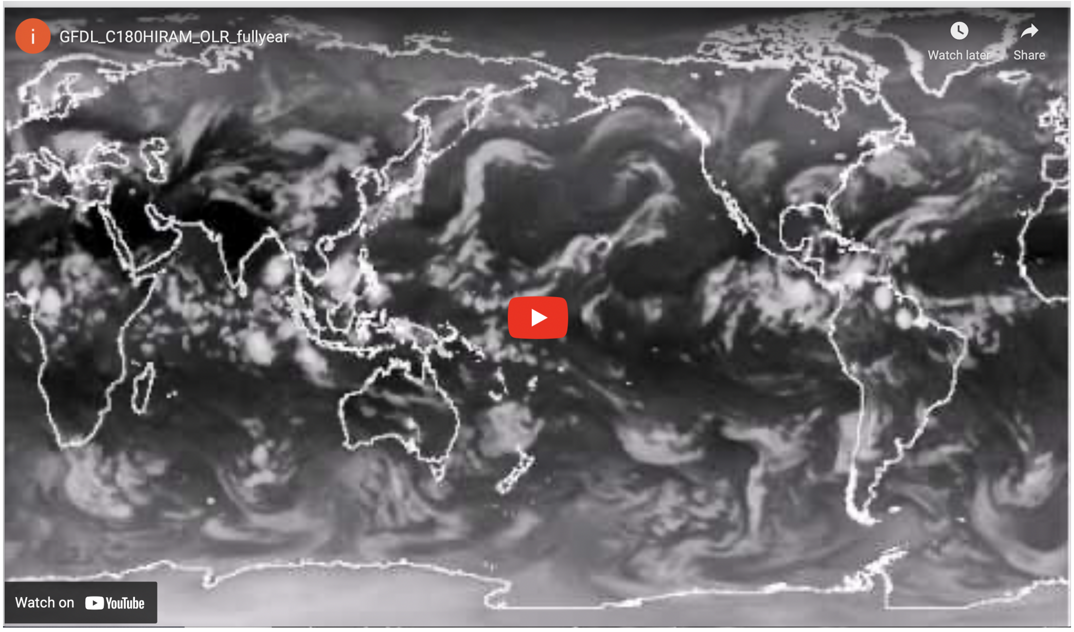


Figure 1.1: Snapshot of outgoing infrared radiation in the HiRAM global atmospheric model developed at GFDL, running freely with prescribed sea surface temperatures.

Click here for animation. (1 frame/day for one year, starting in January.)

I originally started this blog in 2011. My goal was to provide a forum for discussion of climate dynamics, with an emphasis on climate change. The level of discussion was meant to be appropriate for graduate students in atmospheric and oceanic sciences, but I hoped that this type of discussion would also be useful to students in other fields with good applied math, physics and/or engineering backgrounds, to practicing scientists in other fields, and to some of my own colleagues. Different threads were focused on different parts of this intended readership.

I have restructured this material as a series of essays, available in a single pdf file, to create a more stable record. I have made only minor modifications, fixing errors and eliminating a few sentences or paragraphs that no longer make any sense to me. There are a handful of posts that I don't care for now, but I retained all of them, with the same ordering as in the originals. Most significantly, I have eliminated all of the comments on the original posts, despite the fact that some of these comments were quite informative, so some of that flavor is lost.

I was employed by NOAA, working at the Geophysical Fluid Dynamics Laboratory GFDL and also lectured and advised graduate students and post-docs in the Atmospheric and Oceanic Sciences Program AOS at Princeton University) when these essay were originally posted. I retired in 2019 but retain an affiliation with AOS.

I call myself an atmospheric or climate dynamicist/theorist/modeler. I am sure that there are philosophers of science who distinguish between the terms "theory"; and "model", but I don't. I work with a range of theories of different kinds; when these reach a certain level of complexity they are typically referred to as computer models. The most relevant distinction relates to the purpose of the model. Some models are meant to improve our understanding of the climate system, not to simulate it with any precision. Often we use simpler models to see if we can capture aspects of the behavior of more complex models, as a test of our understanding. I like to talk about building a hierarchy of these models designed to improve and encapsulate our understanding. The most comprehensive models can be thought of as our best attempts at simulation, limited by available computer resources and our understanding of the effective governing dynamics on space and time scales resolvable with those resources.

Here is an example of a very simple model consisting of two coupled linear ordinary differential equations:

$$\begin{aligned} c\frac{dT}{dt} &= -\beta T - \gamma(T - T_O) + \mathcal{F}(t) \\ c_O\frac{dT_O}{dt} &= \gamma(T - T_O) \end{aligned} \tag{1.1}$$

This system is meant to represent the perturbations away from an initial equilibrium state of the global mean surface temperature T and deep ocean temperature T_O resulting from the radiative forcing $\mathcal{F}$. My colleagues and I used this model in this 2010 paper to help frame the discussion of what we refer to as the recalcitrant component of global warming. See also Gregory 2000.

The animation at the top is a small part of the output from another model, one developed at GFDL and finalized in 2009, a global atmosphere and land model living on a grid with approximately 50km spacing in the horizontal. One can think of the atmospheric component of this model as 37,519,200 coupled ordinary differential equations (not that this is necessarily a good measure of the complexity of the model.) Shown in the animation is a full year of the infrared energy emitted to space (black is high emission, white is low emission.) What one sees mostly are the simulated high clouds that provide cold weakly emitting surfaces, but if one looks carefully one can see the diurnal cycle in the emission from the surface, which provides a feeling for the rate at which time is passing. Notice the sharp distinction between the mid-latitude atmosphere (dominated by non-linear waves), the tropical atmosphere (dominated by smaller scale turbulence) and the intervening subtropical dry zones.

The model is initialized some years before this animation loop and is constrained only by imposed surface boundary conditions over the ocean and sea ice. No data is input into the model as it evolves other than these slowly evolving surface boundary conditions and the seasonal evolution of the incident solar flux. In a full climate model, the state of the oceans and sea ice would evolve freely as well. Comparing this particular simulated turbulent and chaotic space-time field with observations in ways that are most informative about model deficiencies and the reliability of the model for various applications is a formidable challenge.

The two-box model and this high resolution atmospheric model illustrate two very distinct elements in the hierarchy of climate models. I'll discuss both models in the next few essays. Much of my own work seems to have gravitated towards creating models intermediate in complexity between these two limits, in an attempt to both increase our understand of the climate and provide ideas on how to improve our high-end models. See this 2005 essay for a discussion of the importance of model hierarchies in climate science.

2 Hurricane-like Vortices

[Originally posted February 22, 2011]

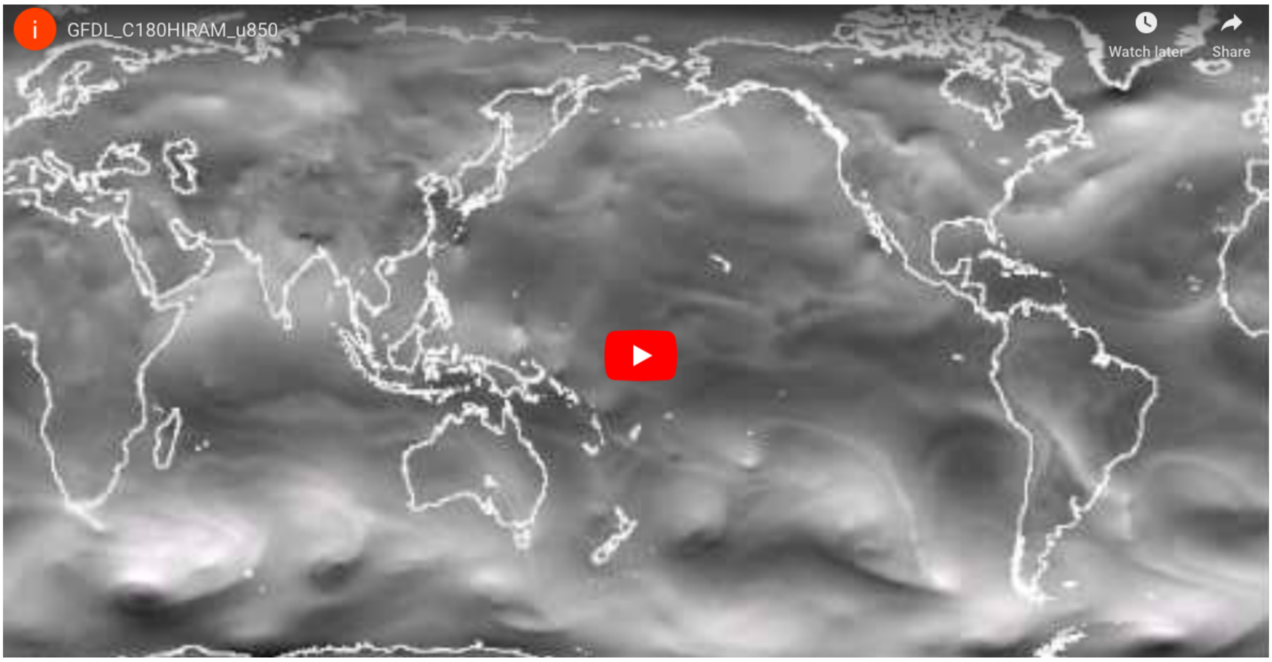


Figure 2.1: Snapshot of east-west component of winds at a lower tropospheric level in the HiRAM global atmospheric model developed at GFDL, the same model as discussed in Post 1.

Click here for animation.

I'm starting with an essay on a topic that continues to raise many open questions: the simulation of tropical cyclones in global atmospheric models. Climate models are gradually moving to finer spatial resolution. In recent years GFDL has been actively developing global atmospheric climate models with roughly 50 and 25 km grid spacing, with even higher resolution models being run experimentally), and a number of related efforts around the world continue to be pushed energetically. In comparison, horizontal resolutions in the majority of the global atmospheric models referenced in the IPCC's 4th and 5th assessments are of the order of 100-250 km. The animation above is from a 50km model, and shows the lower tropospheric winds over a year of simulation. More precisely, it shows the east-west component of the wind vector in the lower troposphere, on a constant pressure surface as is customary in meteorology. (White = westerly; that is, from the west; black = easterly) The actual surface protrudes above this level in places, especially the Tibetan plateau, and in those areas we use the near-surface wind instead, to avoid having to mark the data as missing in the animation. I like plotting the east-west (zonal) component like this, with a gray scale; it gives one the feeling of looking at a 3d contour map of sea-level pressure, lighted from the south in the Southern Hemisphere and from the north in the Northern Hemisphere, because the east-west component of the wind is proportional to the north-south gradient of the pressure, within the geostrophic approximation.

I want to focus here, not on the dominant mid-latitude waves that are associated with the highs and lows and jet stream meanders familiar from weather maps, but the small isolated vortices that form in the tropics. These seem to develop in a variety of ways and have sizes that are not well-resolved by the grid. Do they correspond with anything in the real world?

Ming Zhao, S.-J. Lin, Gabe Vecchi and I discuss this model in HiRAM 2009, where we describe 4 runs of the model specifying the observed sea surface temperatures (SSTs) over the period 1981-2005. The runs differ only in their 1980 initial condition. The animation is the simulation of 2005, the very active North Atlantic hurricane year, from one of these realizations. What makes this a simulation of 2005, rather than some other year, is simply the prescribed SSTs; no other information is provided to the model. Here are three figures from the paper, all related to strong vortices with winds in excess of 33 m/s near the surface, a standard definition of a hurricane in the Atlantic. The observations are from IBTrACS.

Picking one of these realizations, Fig.2.2 shows the paths of all tropical storms that reach hurricane strength at some point in their lifetime over this 25-year period. Observations on the top. Figure 2.3 shows the seasonal cycle in the number of these hurricane-like vortices forming in different ocean basins, averaging over all 4 realizations (1= Jan; 12 = Dec). Finally, Fig.2.4 is a year-by-year comparison of the number of hurricanes that formed in the North Atlantic with the hurricane-like vortices in the model simulations. The gray area spans the 4 realizations; blue is the mean of these; red is the observations. (The comparable figure in the paper extends from 1981-

2005; in this figure these runs have been extended through 2008. Also, in the paper we normalize the results so that the mean number of hurricanes is the same in the model and observations, but this doesn't make much difference in the Atlantic, so I have omitted this normalization here.) In

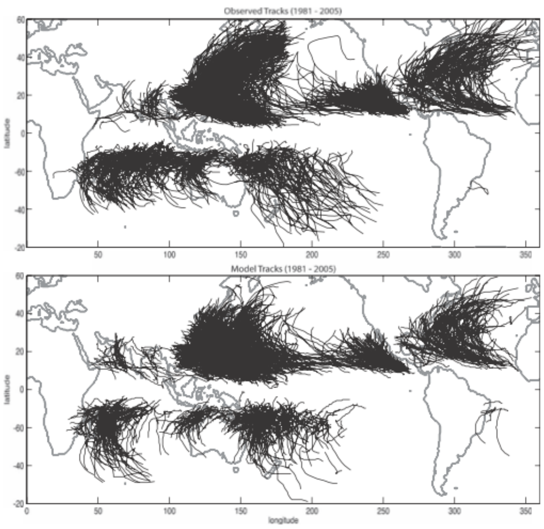


Figure 2.2:

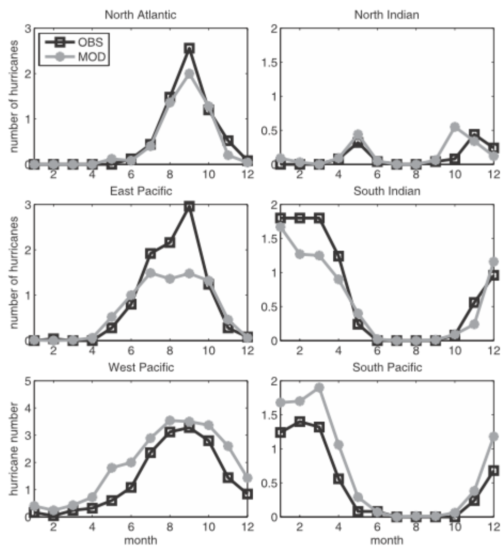


Figure 2.3:

light of these comparisons, I feel comfortable referring to the strong tropical

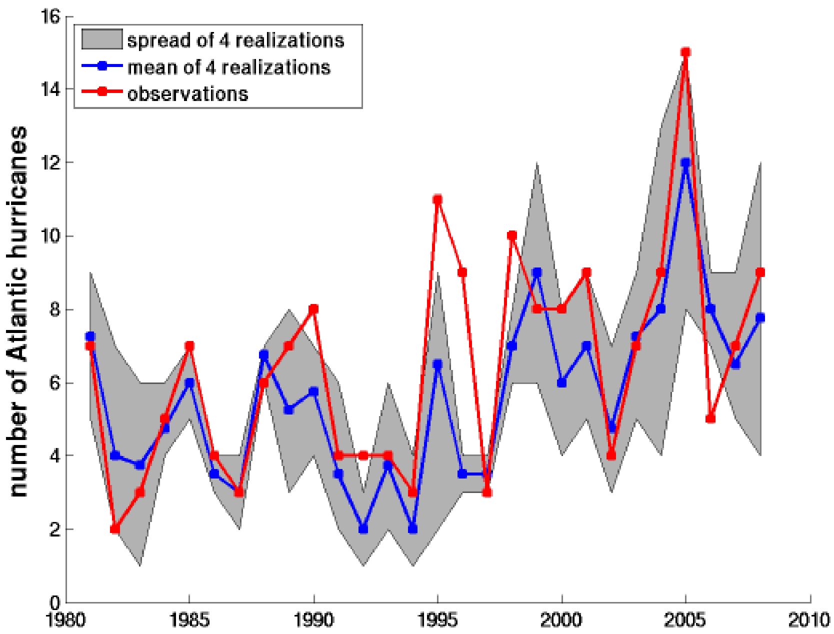


Figure 2.4:

vortices in this and similar models as the model-simulated hurricanes, rather than hurricane-like vortices.

Further analysis reveals a number of limitations. The model produces hardly any storms in the Atlantic beyond category 2. In effect, the model hurricanes resemble each other a lot more than do observed storms, and are mostly minimal hurricanes. In addition, the model storms have larger horizontal scales than observed hurricanes.

Here is a list of questions this study raised, some of which we and others have tried to address in other papers. The first question is more fundamental than the others, since it addresses the appropriateness of this model framework; the other questions concern the model results, given this framework.

- The model is run over prescribed ocean temperatures as a lower boundary condition. How much is this decoupling of atmospheric and ocean dynamics distorting these results? If we coupled a model like this to an ocean model and let it run freely, we could still use its climatology and seasonal cycle as tests (the first and second figures above), but we wouldn't have an analogue of the final figure, since a free-running coupled model would produce its own year-to-year variability in SSTs that, even if the model were perfect, would resemble the observed variability only in its statistics. We wouldn't be able to directly ask if the model captures the very active 2005 season, but only how often it produces 2005-like seasons.

- What aspects of the SST field control Atlantic hurricane numbers in this model? A related question is: if one is attempting seasonal forecasting of the Atlantic hurricane season, predicting statistics for the next season before it begins, what aspects of the evolution of the SST field is it most important to get right? What are the mechanisms within the model by which SSTs influence the interannual variability of tropical cyclone numbers, and are we confident that these are the same mechanisms operative in the atmosphere? Are these mechanisms the same as those underlying the trend in Atlantic storms over the time period shown?

- The problem of the simulation of tropical cyclone numbers seems to be more or less decoupled from the simulation of internal storm structure, such as intensity and size, since one seems to do well on the former with models that are inadequate for the latter. Does this make sense?

- The spread in storm numbers among the 4 realizations, the gray area in the figure, gives us an estimate of the noise in the fixed SST system, the part of the interannual variability associated with atmospheric chaos that exists even if SSTs are held fixed. Is the model's estimate of this fixed-SST noise reliable, assuming that we had a larger number of realizations to estimate the noise a bit more quantitatively?

- When we warm the oceans in this model uniformly, the number of tropical cyclones averaged over the tropics decreases. Why? See this paper for pre-2010 references on this counter-intuitive reduction in tropical cyclones (TCs) with warming.

- What kinds of observational tests, besides those illustrated above, would be useful in assessing the credibility of a model for simulating the response of TCs to global warming, or the credibility of an ensemble of models in spanning the range of uncertainty?

It should be clear from the animation that this is not the kind of model that consists of rules like [if conditions X,Y and Z are satisfied, then form a tropical cyclone]. Rather, these cyclones emerge from the underlying fluid dynamics and thermodynamics. If empirical rules are well-supported by observations one would hope that the model would obey them as well. But they are not built in. The parameters at our disposal invariably relate to assumptions concerning the sub-grid scale fluxes of energy, momentum and water.

3 The simplicity of the Forced Climate Response

[Originally posted March 5, 2011]

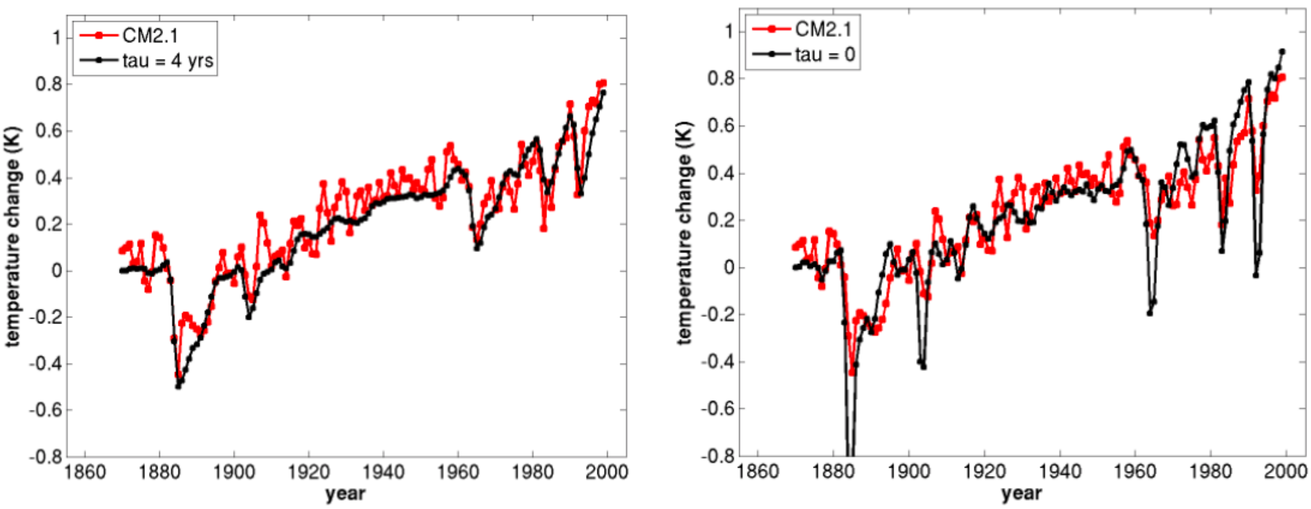


Figure 3.1: An estimate of the forced response in global mean surface temperature, from simulations of the 20th century with a global climate model, GFDL's CM2.1, (red) and the fit to this evolution with the simplest one-box model (black), for two different relaxation times. From Held et al (2010).

When discussing the emergence of the warming due to increasing greenhouse gases from the background noise, we need to clearly distinguish between the forced response and internal variability, and between transient and equilibrium forced responses. But there is another fundamental, often implicit, assumption that underlies nearly all such discussions: *the simplicity of the forced response.* Without this simplicity, there is little point in using concepts like *forcing* or *feedback* to help us get our minds around the problem, or in trying to find simple observational constraints on the future climatic response to increasing CO_2. The simplicity I am referring to here is emergent, roughly analogous to that of a macroscopic equation of state that emerges, in the thermodynamic limit, from complex molecular dynamics.

I'll begin by looking at some results from a climate model. The model (GFDL's CM2.1) is one that I happen to be familiar with; it is described in Delworth et al 2006. This model simulates the time evolution of the state of the atmosphere, ocean, land surface, and sea ice, given some initial condition. The complexity of the evolution of the atmospheric state is qualitatively similar to that shown in the videos in posts 1 and 2, although the atmospheric component of CM2.1 has lower horizontal spatial resolution (roughly 200km).

The input to CM2.1 includes prescribed time-dependent values for the well-mixed greenhouse gases (carbon dioxide, methane, nitrous oxide, CFCs) and other forcing agents (volcanoes, solar irradiance, aerosol and ozone distributions, and land surface characteristics). The model then attempts to simulate the evolution of atmospheric winds, temperatures, water vapor, and clouds; oceanic currents, temperature, and salinity; sea ice concentration and thickness; and land temperatures and ground water. It does not attempt to predict glaciers, land vegetation, the ozone distribution, or the distribution of aerosols; all of these are prescribed. Different classes of models prescribe and simulate different things; when reading about a climate model it is always important to try to get a clear idea of what the model is prescribing and what it is simulating.

Holding all of the forcing agents fixed at values thought to be relevant for the latter part of the 19th century and integrating for a while, the model settles into a statistically steady state with assorted spontaneously generated variability, including mid-latitude weather, ENSO, and lower frequency variations on decadal and longer time scales. Now perturb this control climate by letting the forcing agents evolve in time according to estimates of what occurred in the 20th century. Do this multiple times, with the same forcing evolution in each case, but selecting different states from the control integration as initial conditions. Average enough of these realizations together to define the *forced response* of whatever climate statistic one is interested in. Each realization from a particular initial condition consists of this forced response plus internal variability, but I want to focus here on the forced response. Observations are not expected to closely resemble this forced response unless the internal variability in the quantity being examined is small compared to the variations in the forced response.

The red curve in the figure is an average over 4 of these realizations of the annual mean and global mean surface temperature. A bigger ensemble would be needed to fully wash out the model's internal variability (CM2.1

has the interesting problem that its ENSO is too strong). Volcanic aerosols are the only part of the forcing that has rapid variations; besides these impulsive events, the impression is that the forced response would be smooth if estimated with a much bigger ensemble.

The black curve is a solution to the simplest one-box model of the global mean energy balance

$$cdT/dt = -\lambda T + \mathcal{F}(t) \tag{3.1}$$

where $\mathcal{F}$ is the *radiative forcing*, λ is the strength of the relaxation of global mean surface temperature back to equilibrium, and c is an effective heat capacity. The global mean temperature T is the perturbation from the control climate. Where does $\mathcal{F}$ come from? Here we follow the approach labelled $\mathcal{F}_S$ by Hansen et al 2005. It is the net energy flowing in at the top of the model atmosphere, in response to changes in the forcing agents, after allowing the atmosphere (and land) to equilibrate while holding ocean temperatures and sea ice extent fixed. That is, we use calculations with another configuration of the same model, constrained by prescribing ocean temperatures and sea ice, to tell us what "radiative forcing" it feels as a function of time. This estimate is sometimes referred to as the "radiative flux perturbation", or RFP, rather than "radiative forcing", but I think it is the most appropriate way of defining the forcing $\mathcal{F}$ to be used in this kind of energy balance emulation of the full model. (Why do we fix only ocean and sea ice surface boundary conditions and not land conditions? This is an interesting point that I probably should come back to in another post.) This estimate of the forcing $\mathcal{F}$ felt by this particular model increases by about 2.0 W/m^2 over the time period shown.

The relaxation time $\tau \equiv c/\lambda$ is set at 4 years for the plot in the left panel, a number that was actually obtained by fitting another calculation in which CO_2 is instantaneously doubled, which isolates this fast time scale a bit more simply. Not surprisingly, being this short, decreasing this time scale by reducing the heat capacity, or even setting it to zero, has little effect on the overall trend over the century; all that happens is that the response to the volcanic forcing has larger amplitude and a shorter recovery time (conserving the integral over time of the volcanic cooling), as one can see from the lower panel, where the black line is simply $\mathcal{F}/\lambda$. To get the time scale of 4 years with this value of λ, the heat capacity needs to be that of about 70 meters of water.

If we compute the forcing due to doubling of CO_2 with the same method that we use to compute $\mathcal{F}(t)$ above, we get 3.5 W/m^2. The equilibrium response $\mathcal{F}/\lambda$ to doubling using the value of λ obtained from the fit in the figure would be roughly 1.5K. However, if we double the CO_2 in the CM2.1

model and integrate long enough so that it approaches a new equilibrium, we find that the global mean surface warming is close to 3.4K. Evidently, the simple one-box model fit to the 20th century evolution in CM2.1 does not work on the time scales required for full equilibration. Heat is taken up by the deep ocean during this transient phase, and the effects of this heat uptake are reflected in the value of λ in a one-box fit. Longer time scales, involving a lot more than 70 meters of ocean, come into play as the heat uptake saturates and the model equilibrates. I will be discussing this issue in the next few posts.

Emulating GCMs with simpler models has been an ongoing activity over decades. Most of these simple models are more elaborate than that used here and typically do more than just emulate the global mean temperature evolution in GCMs (MAGICC is a good example). Not all GCMs are this easily fit with simple global mean energy balance models. In particular, different forcing agents can have different *efficacies*, that is, they force different global mean temperature responses for the same global mean radiative forcing (Hansen et al 2005).

Additionally, there exist components of the oceanic circulation with decadal to multi-decadal time scales that have the potential to impact the evolution of the forced response over the past century. (This is a different question than whether oceanic internal variability contributes significantly to individual realizations.) I would like to clarify in my own mind whether the ability to fit the 20th century evolution in this particular GCM with the simplest possible energy balance model, with no time scales longer than a few years, is typical or idiosyncratic among GCMs. Other GCMs may require simple emulators with more degrees of freedom to achieve the same quality of fit. There is no question that more degrees of freedom are needed to describe the full equilibration of these models to perturbed forcing, as already indicated by the difference in CM2.1's transient and equilibrium responses described above, but my question specifically refers to simulations of the past century. I would be very interested if this is discussed somewhere in the literature on GCM emulators. The problem seems to be that accurate computations of radiative frocing felt by individual models are not generally available.

Forced, dissipative dynamical systems can certainly do very complicated things. But you can probably find a dynamical system to make just about any point that you want (there may even be a theorem to that effect); it has to have some compelling relevance to the climate system to be of interest to us here. We will have to return to this issue of linearity-complexity-structural stability, and the critique of climate modeling that we might call "the argument from complexity" (the opposite of Occam's razor), the essence of which is often simply "Who are you kidding?; the system is far too complicated to model with any confidence".

In the meantime, the goal here has been to try to convince you that the transient forced response in one climate model has a certain simplicity, despite the complexity in the model's chaotic internal variability. (Admittedly, we have only talked up to this point about global mean temperature.) But is there observational evidence for this emergent simplicity in nature? In the limited context of fitting simple energy balance models to the global mean temperature evolution, convincing quantitative fits are more difficult to come by due to uncertainties in the forcing and the fact that we have only one realization to work with. Fortunately, we have other probes of the climate system. The seasonal cycle on the one hand and the orbital parameter variations underlying glacial-interglacial fluctuations on the other are wonderful examples of forced responses that nature has provided for us, straddling the time scales of interest for anthropogenic climate change. In both cases the relevant change in external forcing involves the Earth-Sun configuration, and we know precisely how this configuration changes. Both have a lot to teach us about the simplicity and/or complexity of climatic responses. Some of the lessons taught by the seasonal cycle are especially simple and important. Watch out for a future post entitled "Summer is warmer than winter".

4 Transient vs Equilibrium Climate Responses

[Originally posted March 11. 2011]

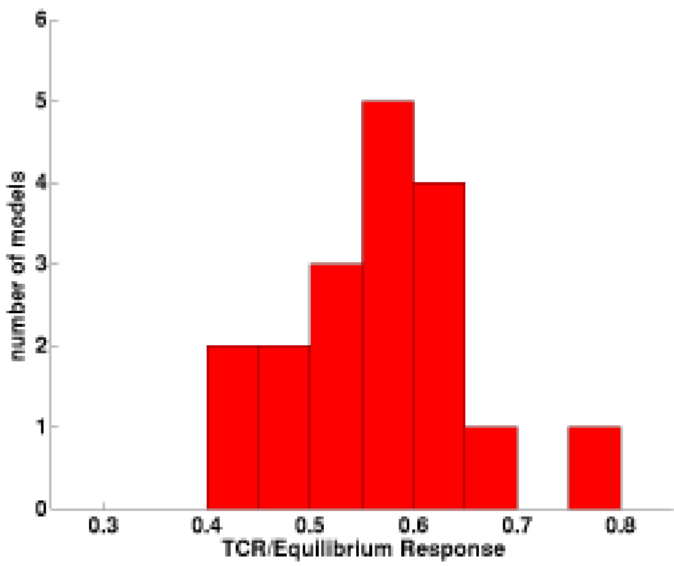


Figure 4.1: Histogram of the ratio of the transient climate response (TCR) to equilibrium climate response in global mean temperature in the 18 models in which both values are provided Ch. 8 of the WG1/AR4/IPCC report.

I find the following simple two degree-of-freedom linear model useful when thinking about transient climate responses:

$$\begin{aligned} c\frac{dT}{dt} &= -\beta T - \gamma(T - T_O) + \mathcal{F}(t) \\ c_O\frac{dT_O}{dt} &= \gamma(T - T_O) \end{aligned} \tag{4.1}$$

T and T_O are meant to represent the perturbations to the global mean surface temperature and deep ocean temperature resulting from the radiative forcing $\mathcal{F}$. The strength of the radiative restoring is determined by the constant β, which subsumes all of the radiative feedbacks – water vapor, clouds, snow, sea ice – that attract so much of our attention. The exchange of energy with the deep ocean is assumed to be proportional to the difference in the temperature perturbations between the surface and the deep layers, with constant of proportionality γ. The fast time scale is proportional to c, representing the heat capacity of the well-mixed surface layer, perhaps 50-100m deep on average (the atmosphere's heat capacity is negligible in comparison), while c_O crudely represents an effective heat capacity of the rest of the ocean. Despite the fact that it seems to ignore everything that we know about oceanic mixing and subduction of surface waters into the deep ocean, I think this model gets you thinking about the right questions.

The two box model reduces to the classic one-box model if $\gamma = 0$:

$$c\frac{dT}{dt} = -\beta T + \mathcal{F}(t) \tag{4.2}$$

The equilibrium response in this model, the response eventually achieved for a fixed forcing, is $\mathcal{F}/\beta \equiv T_{EQ}$ The equilibrium response in this particular two-box model takes this same value, independent of γ.

These kinds of models are commonly used to help interpret the forced responses in much more elaborate GCMs. They are also often relied upon when discussing observational constraints on climate sensitivity. That is, one has a simple model with one or more parameters that control the magnitude of the response to a change in CO_2. One then uses the same model to simulate some observations (the response to a volcano, perhaps) to constrain the values of these parameters. Appreciating the limitations of the underlying model (there is always an underlying model) is often the key to understanding competing claims concerning these constraints on sensitivity.

You can write down the solution to the two-box model, but let's just look at the special case in which c_O is so large that the change in T_O is negligible. We then have

$$c\frac{dT}{dt} = -(\beta + \gamma)T + \mathcal{F}(t) \tag{4.3}$$

so the time scale of the response to an abrupt change in forcing is $\tau = c/(\beta + \gamma)$. Surface temperature perturbations decay not just by radiating to space but also by losing energy to the deep ocean. A typical value of τ that one could use to mimic the behavior of a GCM might be about 4 years, if we can use the results described in Essay 3 as a guide.

But now suppose that $\mathcal{F}$ varies only on time scales longer than τ, continuing to assume that these time scales are short compared to the time required to modify T_O significantly. Then

$$T(t) \approx \mathcal{F}/(\beta + \gamma) \tag{4.4}$$

On these time scales both heat capacities drop out. The fast component is equilibrated, while the slow component is acting as an infinite reservoir. What is left is a response that is proportional to the forcing, with the deep ocean uptake acting as a negative feedback with strength γ. I'll refer to the time scales on which this balance holds as the *intermediate regime*. Gregory and Forster (2008) call $\lambda \equiv \beta+\gamma$ the *climate resistance*. Dufresne and Bony (2008) is also a very useful reference.

The *transient climate response*, or TCR is traditionally defined in terms of a particular calculation with a climate model: starting in equilibrium, increase CO_2 at 1% per year until the concentration has doubled (about 70 years). The amount of warming around the time of doubling is referred to as the TCR. If CO_2 is then held fixed at this value, the climate will continue to warm slowly until it reaches T_{EQ}. To the extent that this 70 year ramp-up qualifies as being in the intermediate regime, the ratio of TCR to T_{EQ} would be $\beta/(\beta + \gamma)$ in the two-box model.

The median of this ratio in the particular ensemble of GCMs referred to in the figure at the top of this post is 0.56. For several models the ratio is less than 0.5. Interestingly, it is very difficult to get ratios this small from the two-box model tuned to the equilibrium sensitivity of the GCMs and to their rate of heat uptake in transient simulations. (There is a fair amount of slop in these numbers – equilibrium responses are typically estimated with *slab-ocean* models in which changes in horizontal oceanic heat fluxes are neglected, and the transient simulations are single realizations – but I doubt that the basic picture would change much if refined.

The *heat uptake efficiency* Γ is defined to be the rate of heat uptake by the planet (the oceans to a good approximation) per unit global warming. Typical values in GCMs are $0.7 \pm 0.2 W/m^2$ Dufresne and Bony 2008. For a warming of 0.8K over the past century, this magnitude of Γ implies a rate of heat uptake at present of about $0.6W/m^2$. This value is consistent with Lyman et al, 2010 (their best estimate of the rate of heat uptake by the upper 700m of the ocean over the period 1993-2008 is $0.64W/m^2$. In the two-box model, $\Gamma \approx \gamma$ if the heat stored in the surface layer is small compared to that in the oceanic interior.

For the particular GCM CM2.1 discussed in Essay 3 (which has one of the smaller ratios of TCR to equilibrium response), using the numbers in that post, we need $\beta + \gamma \approx 1W/(m^2K)$ to explain the model's TCR and equilibrium responses. This would seem to require $\gamma \approx 1.3$, much larger than the GCM's value, which is close to 0.7. I'll return to this discrepancy in the next essay.

A few thoughts about heat uptake:

- When the ocean's mean climate is perturbed by a small amount, and given that heat transport is a product of the flow and the temperature, one can think of the unperturbed flow as transporting the perturbed temperatures, and the perturbed flow transporting the unperturbed temperatures. If the former is dominant we expect the uptake to be proportional to the temperature perturbation. We might also conceivably be able to think of the change in circulation as determined by the temperature response (if the changes in other things that affect the circulation, like salinities and wind stresses, can themselves be thought of as determined by the temperature field) but the circulation takes time to adjust and these time scales could destroy the simplicity of the intermediate regime if the circulation responses are dominant.

- Even if circulation changes are not dominant, the coupling to the deep oceans is strongest in the North Atlantic and the Southern Oceans, so the temperature anomalies in those regions presumably have more to do with the uptake than the global mean. Only if the forced warming is separable in space and time, $T(x,t) \approx A(x)B(t)$ do we have any reason to expect the uptake to scale with the global mean temperature. A changing mix of greenhouse gas and aerosol forcing is one way to break this separability, but even for CO2 forcing in isolation, there is potential for the temperature pattern to change in time (as discussed in upcoming chapters)

- Do we have any simple theories explaining the rough magnitude of γ or Γ to supplement GCM simulations.

5 Time-dependent Climate Sensitivity

[Originally posted on March 19, 2011]

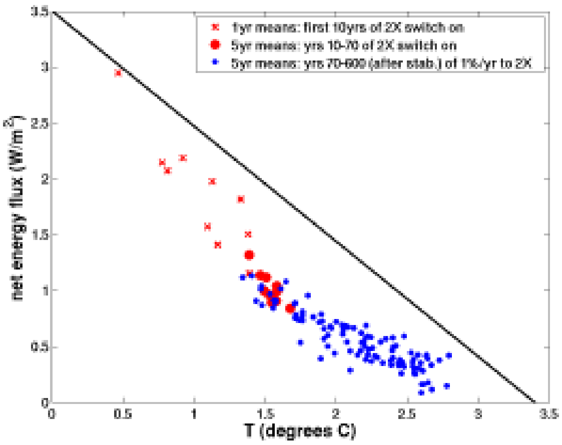


Figure 5.1: The co-evolution of the global mean surface air temperature (T) and the net energy flux at the top of the atmosphere, in simulations of the response to a doubling of CO_2 with GFDL's CM2.1 model. Slightly modified from Winton et al 2010.

Global climate models typically predict transient climate responses that are difficult to reconcile with the simplest energy balance models designed to mimic the GCM's climate sensitivity and rate of heat uptake. This figure helps to define the problem.

Take your favorite climate model, instantaneously double the concentration of CO_2 in the atmosphere,and watch the model return to equilibrium. I am thinking here of coupled atmospheric-ocean models of the physical climate system in which CO_2 is an input, not models in which emissions are prescribed and the evolution of atmospheric CO_2 is itself part of the model output.

Now plot the globally-averaged energy imbalance at the top of the atmosphere $\mathcal{N}$ versus the globally-averaged surface temperature T. In the most common simple energy balance models we would have $\mathcal{N} = \mathcal{F} - \beta T$ where both $\mathcal{F}$ (the radiative forcing) and β, the strength of the radiative restoring are constants. The result would be a straight line in the $(T, \mathcal{N})$ plane, connecting $(0, \mathcal{F})$ with $(T_{EQ} \equiv \mathcal{F}/\beta, 0)$ as indicated in the figure above. The particular two-box model discussed in Essay 4 would also evolve along this linear trajectory; the different way in which the heat uptake is modeled in that case just modifies how fast the model moves along the line.

The figure at the top shows the behavior of GFDL's CM2.1 model. The departure from linearity, with the model falling below the expected line, is common if not quite universal among GCMs, has been discussed by Williams et al 2008 and Winton et al 2010 recently. These papers cite some earlier discussions of this issue as well. Our CM2.1 model has about as large a departure from linearity as any GCM that we are aware of, which is one reason why we got interested in this issue.

As indicated in the somewhat cryptic legend, we use two different types of simulations to make this plot. One is the instantaneous doubling of CO_2 referred to above. We show annual means for the first 10 years (with each cross in the figure an average over 4 realizations to knock down the noise, branching off at different times from the control simulation) and then show 5 year means up till year 70, again averaging over 4 realizations. Because these integrations do not go out far enough to probe the slower long term evolution, we then append a single realization of the standard calculation in which CO_2 is increased at 1%/year until the time of doubling (year 70) after which it is held fixed until year 600. We plot 5-year averages from this calculation, starting in year 70, so all points in the figure correspond to the same value of CO_2. 600 years still isn't enough to equilibrate, but as long as something fundamentally new doesn't happen in the model on longer time scales, one we extrapolate to $\mathcal{N} = 0$ to get an estimate of the equilibrium temperature response. The two simulations match up nicely in year 70, as we expects if the 1%/yr case resides during its ramp-up phase in the intermediate regime (Essay 3). Because of the curvature of this trajectory, the temperature change at year 70, about 1.5-1.6K (the transient climate response, or TCR) is smaller than we might expect from the model's equilibrium sensitivity and the model's value of $\mathcal{N}$ at that same time.

One's first reaction might be to say – well, there is nonlinearity in the model in the sense that β is effectively a function of time. But I think there is agreement that the underlying dynamics is still best described as linear; it's just that the global mean energy balance is not a function of the global mean surface temperature. A more general linear model assumes that the global mean energy balance is a linear functional of the entire spatial distribution of the surface temperature response, with different structures in the surface temperature perturbations, even if they have the same global mean, generating different perturbations in the global mean energy balance.

Think of some atmospheric model equilibrated over a prescribed surface temperature distribution. This temperature field is the input to the atmoospheric model. The model outputs climate statistics, including the global mean energy balance. If the relation between the input perturbation (surface temperature distribution) and output (global mean energy balance) is linear, we can write

$$\mathcal{N} = \mathcal{F}(t) - [\mathcal{B}(x)T(x,t)] \tag{5.1}$$

Brackets denote a spatial average over the surface and $x = (lat, lon)$ is the position on the surface. The scalar radiative restoring constant β has been replaced by $\mathcal{B}(x)$. (By the way, I am not assuming here that the top-of-atmosphere energy balance in some small region is only a function of the surface temperature in that same region – the relation between these two is non-local due to the fact that many of the infrared photons escaping to space are emitted by the atmosphere, and the response in the atmosphere to a localized temperature perturbation at the surface is not localized in general.)

The simplest case is when temperature evolves in a self-similar manner, ie, growing with a fixed spatial structure:

$$T(x,t) = \mathcal{G}(x)g(t) \tag{5.2}$$

I have normalized things so that $[\mathcal{G}] = 1$. The effective radiative restoring strength for this structure is

$$\beta_G \equiv [\mathcal{B}\mathcal{G}] \implies \mathcal{N} = \mathcal{F} - \beta_G[T] \tag{5.3}$$

But suppose that the temperature perturbations are the sum of two patterns with relative contributions varying in time:

$$T(x,t) = \mathcal{G}(x)g(t) + \mathcal{H}(x)h(t) \tag{5.4}$$

where both patterns are normalized so that $[\mathcal{G}] = [\mathcal{H}] = 1$ and, as a result, $[T(t)] = g(t) + h(t)$. This gives us enough freedom to get evolution off the classic linear trajectory. But we haven't learned anything yet about how and why the ratio of g to h is evolving in time.

One way of analyzing any linear system is through the frequency-dependence of the response to perturbations. Low frequency and high frequency forcing can result in different radiative restoring strengths if they result in different spatial structures in the response. Evidently, the low frequency component controlling the late time evolution in the response to doubling of CO_2 is characterized by a structure than is restored less strongly that is the fast, early response. Why would that be?

The story seems to be something like this. The atmosphere tends to be most unstable to vertical mixing in the tropics, where the surface temperatures are the warmest, but the oceans are most unstable to vertical mixing in high latitudes, where the surface temperatures are the coldest. It is in the subpolar oceans that the mixing between surface and deeper waters is the strongest. One expects these regions to be a major source of the difference between fast and slow responses. Given that the fully equilibrated response in models has substantial polar amplification, this large polar response will be held back by the large heat uptake in the initial fast response.

We now have to argue why a pattern with larger high latitude amplification is restored less strongly. A part of the explanation seems to be that the surface is less strongly coupled to the atmosphere in high than in low latitudes, so the surface warming has a harder time affecting the radiation escaping to space. But differing cloud responses to warming, as well as the positive feedback from snow and sea ice in high latitudes, also play a role.

One can still try to save the global mean perspective. Winton et al 2010 pursue this line of reasoning by referring to the *efficacy of ocean heat uptake*. The idea is that the difference in spatial structure of the fast and slow responses can be attributed to the heat being transferred from shallow to deeper ocean layers. Putting aside the question of how this heat transfer is controlled, once can try to think of it as a different kind of "forcing" of the near-surface layer, alongside the radiative forcing. The response to heat uptake, being focused in high latitudes, naturally has a spatial structure that is more polar amplified that the response to CO_2 with the heat uptake fixed, so it experiences a smaller restoring strength. The effects of the surface cooling due to heat uptake by deeper layers slows the initial fast warming preferentially, since the heat uptake decreases with time. This picture has the nice feature that it ties the timing of the change in spatial structure directly to the saturation of the heat uptake. You may want to think about how to capture this effect with a simple modification of the two-box model described above and in earlier essays.

One moral of this story is that forcing a global mean perspective on the system can make things look more complicated that they actually are, making a response look superficially nonlinear when it is still linear. Another moral is that the connection between transient and equilibrium responses may not be as straightforward as we might like, even when only considering the consequences of the physical equilibration of the deep ocean, leaving aside things such as the slow evolution of ice sheets.

6 Transient Response to the Well-mixed Greenhouse Gases

[Originally posted on March 28, 2011.]

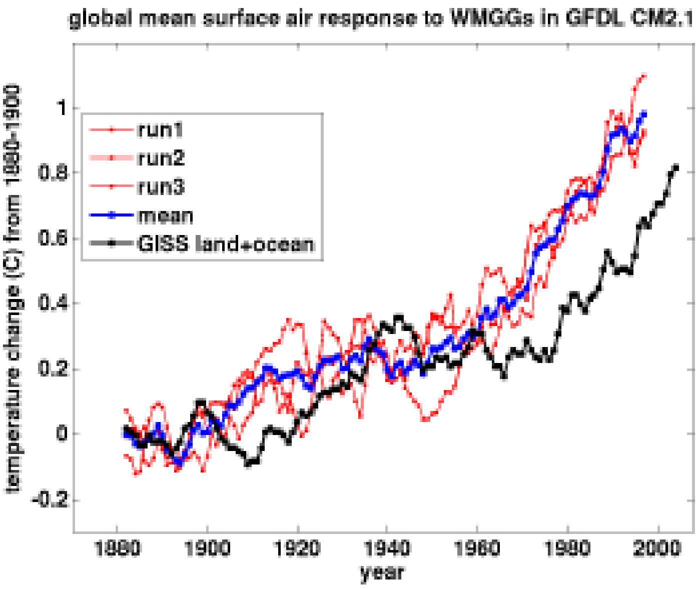


Figure 6.1: Global mean surface air warming due to well-mixed greenhouse gases in isolation, in 20th century simulations with GFDL'a CM2.1 climate model, smoothed with a 5yr running mean. Blue is the mean of the three red individual realizations, while black is the observations

"It is likely that increases in greenhouse gas concentrations alone would have caused more warming than observed because volcanic and anthropogenic aerosols have offset some warming that would otherwise have taken place" (AR4 WG1 Summary for Policy Makers)

One way of dividing up the factors that are thought to have played some role in forcing climate change over the 20th century is into 1) the well-mixed greenhouse gases (WMGGs): essentially carbon dioxide, methane, nitrous oxide, and the chlorofluorocarbons) and 2) everything else. The WMGGs are well-mixed in the atmosphere because they are long-lived, so they are often referred to as the long-lived greenhouse gases (LLGGs). Well-mixed in this context means that we can typically describe their atmospheric concentrations well enough, if we are interested in their effect on climate, with one number for each gas. These concentrations are not exactly uniform, of course, and studying the departure from uniformity is one of the keys to understanding sources and sinks.

We know the difference in these concentrations from pre-industrial times to the present from ice cores and modern measurements, we know their radiative properties very well, and they affect the troposphere in similar ways. So it makes sense to lump them together for starters, as one way of cutting through the complexity of multiple forcing agents.

When only the WMGGs are included in simulations of the 20th century, what do models predict? A result from the GFDL CM2.1 model is shown in the figure above. (Among other things, this model prescribes ozone and it also does not include the effects of methane oxidation on stratospheric water — so the effects of WMGGs through their perturbations to stratospheric ozone, and the trend in the methane source of stratospheric water, are not included here.) The identical model is run three times with different conditions taken from a pre-industrial control simulation, so these three realizations produce different details of the chaotic internal variability in the model. The three red lines in the figure are the global mean surface air temperature in these simulations, the blue line is their average, an estimate of the forced response (post 3). The black line is the land-plus-ocean global mean as estimated by the GISS product. In each case, temperatures are plotted as anomalies from the 1880-1900 mean and a 5 year running mean is applied to each time series, removing much of the effects of ENSO variability, in particular. These model runs continued only until the year 2000, so the 5yr running mean ends in 1998.

When forced with the concentrations of WMGGs, this particular model overestimates the warming by about 30%. With the WMGG concentrations used in the model and standard expressions for radiative forcing due to these WMGGs, I estimate that the forcing increased by about 57% of the forcing due to CO_2 doubling over the period shown in the figure (1880-2000), and by 65% of doubling if we were to extend to 2009 (using the forcing growth tabulated at this very useful NOAA web site for the years after 2000). Let's use 60% as a round number for this ratio of the forcing in the recent period, which I will call the "20th century" for short, to the forcing due to doubling of CO_2. This model's equilibrium climate response to doubling of CO_2 is about 3.4K. Shouldn't we expect the warming over the 20th century in this model to be about $0.6 \times 3.4K \approx 2K$?

No, because the model does not fully equilibrate on the time scale of a century. As already discussed in previous posts, a more useful point of comparison is the transient climate response (TCR), the warming at the time of doubling in a simulation in which CO_2 is increased at 1%/year. The model used here has a TCR of about 1.5-1.6K. Rescaling by 0.6 we get about 0.9-1.0K, which evidently explains the model result in the figure to a first approximation.

A convenient archive of results from the world's climate models for precisely this WMGG-only computation did not exist when this essay was written, but the standard 1%/year simulations used to define model TCR's are consistently archived. Multiplying by 0.6, you get a median of about 1.1-1.2K for the CMIP3 models, with a range (for 20 models) from 0.8 to 1.7K. This is a little rough, but I think it is a pretty good estimate of what these models would give in a WMGG-only simulation for the 20th century. (As mentioned in previous posts, CM2.1's TCR is below the median of the CMIP3 models.)

So most models generate trends from the WMGG forcing that are larger than the observed trend in the 20th century. A simple point of reference to keep in mind is that the least sensitive models in the CMIP3/AR4 archive roughly match the observed trend when forced with WMGGs only.

We will need to return to the centrally important question of the amplitude of internal variability, but I just want to point the reader to the figure again to get a sense of the magnitude of the internal variability in this particular model. Note how one of the red lines dips substantially in the 1950's, for example. Evidently this model can generate internal fluctuations in global mean temperature that produce substantial departures from a smooth warming trend, but it does not come close to generating variability comparable to the 20th century trend itself.

Foregoing a critique of this result for the time being, if we assume that natural variability does not, in fact, confound the century-long trend substantially, the observed warming provides us with a clear choice. Either

- the transient sensitivity to WMGGs of the CMIP3 models is too large, on average, or
- there is significant negative non-WMGG forcing– aerosols being by far the most likely culprit.

Two simple things to look out for when this issue is discussed:

- Watch out for those who estimate the expected 20th century warming due to WMGGs by rescaling equilibrium sensitivity rather than TCR.
- Conversely, watch out for those who compare the observed warming to the model's response to CO_2 only, rather than the sum of all the WMGGs. If we scale our expectations for warming down by the fraction of the WMGG forcing due to CO_2, the model results (without aerosol forcing) happen to cluster in a pleasing way around the observed trend, but one cannot justify isolating CO_2's contribution in this way.

In preparing this essay, I was struck, as some readers may have been as well, by the magnitude of the warming early in the century in these WMGG-only simulations.

7 Why Focus So Much On Global Mean Temperature

[Originally posted on April 5, 2011.]

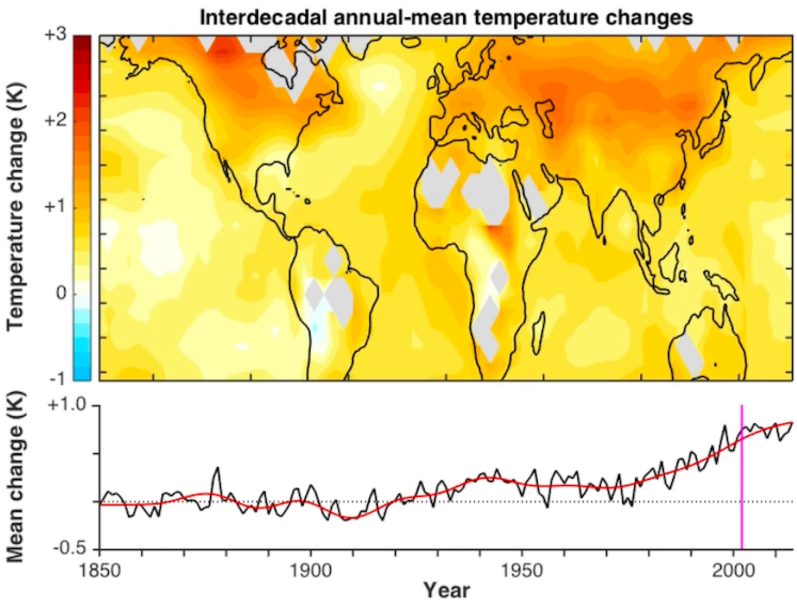


Figure 7.1: Upper panel: Interdecadal component of annual mean temperature changes relative to 1890–1909. Lower panel: Area-mean (22.5°S to 67.5°N) temperature change (black) and its interdecadal component (red). Based on the methodology in Schneider and Held 2001 and using HadCRUT3v temperatures.

Here is the animation.

Perhaps the first thing one notices when exposed to discussions of climate change is how much emphasis is placed on a single time series, the globally averaged surface temperature. This is more the case in popular and semi-popular discussions than in the scientific literature itself, but even in the latter it still plays a significant role. Why such an emphasis on the global mean?

Two of the most common explanations involve 1) the connection between the global mean surface temperature and the energy balance of the Earth, and 2) the reduction in noise that results from global averaging. I'll consider each of these rationales in turn.

The energy balance of any sub-portion of the atmosphere-ocean system is complicated by the need to consider energy fluxes between this selected portion and the rest of the system. It is only for the global mean that the balance simplifies to one involving only radiative fluxes escaping to space (since geothermal heating is negligible in comparison), providing a basic starting point for a lot of considerations. But is there a tight relationship between the global mean surface temperature and the global mean energy budget?

I have already indicated in post 5 that this coupling is not very tight in many climate models. In these models, the pattern of temperature change in response to an increase in CO_2 evolves in time, becoming more polar amplified as equilibrium is approached. And, as a consequence of these changes in spatial pattern, the relationship between the global mean temperature and global mean top-of-atmosphere (TOA) flux changes as well. Among other things, the dynamics governing the vertical structure of the atmosphere is very different in low and high latitudes, and one needs to know how the vertical structure responds to estimate how radiative fluxes respond. There are also plenty of reasons why cloud feedbacks might have a different flavor in high and low latitudes, and might be controlled more by changes in temperature gradients than in local temperature. The potential for some decoupling of global mean surface temperature and global mean TOA flux clearly seems to be there.

There is a tendency, especially when discussing "observational constraints on climate sensitivity", to ignore this issue — assuming, say, that interannual variability is characterized by the same proportionality between global mean temperature and TOA fluxes as is the trend forced by the well-mixed greenhouse gases. This is not to say that the internanual constant of proportionality is irrelevant to constraining climate sensitivity. One can imagine, if interannual variability is characterized by one spatial pattern, and the response to CO_2 by another pattern, that one might be able to compensate for this difference in pattern when trying to use this information to constrain the magnitude of the response to CO_2.

Turning now to the noise reduction rationale: there is plenty of variability in the climate system due to underlying chaotic dynamics, in the absence of changing external forcing agents. To the extent that a substantial part of this internal variability is on smaller scales than the forced signal, spatial averaging will reduce the noise as compared to the signal. But is global averaging the optimal way to reduce noise?

Suppose one has a time series at each point on the Earth's surface. There are a lot of different linear combinations of these individual time series that one could conceivably construct; the global mean is just one possibility. Some of these linear combinations will have the property of reducing the noise more than others. One can turn this around and ask which linear combination reduces the noise most effectively.

Tapio Schneider and I examined this question in a paper in 2001. One has to first define what one means by "minimizing noise". In our case, we define a "signal" by time- filtering the local temperature data to retain variations on time scales of 10 or 15 years and longer and then define the "noise" to be what is left over. We are not saying that this signal is forced by external agents; it is presumably some combination of forced responses and free low-frequency variations. But the forced response due to slowly varying external agents is presumably captured within this signal. We then maximize the ratio of the variance in the "signal" to the variance of the "noise". This is an example of discriminant analysis, in which you group the data and look for those patterns that best discriminate between the data in different groups. (Roughly speaking, the different decades are different groups for our analysis, although we do not actually use non-overlapping decadal groups.) The result is a ranked set of patterns and a time series associated with each pattern. The most dominant pattern, the one that reduces the noise most effectively, turns out to be quite different from uniform spatial weighting. The animation at the top of the blog shows the evolution of annual mean temperatures filtered to retain the 4 most discriminating patterns (this is the number of patterns with a ratio of signal to noise greater than one.)

A more popular approach to multivariate analysis of the surface temperature record, complimentary to discriminant analysis, is "fingerprinting". Here models provide one or more patterns (starting with the pattern forced by the well-mixed greenhouse gases) and, using multiple regression, we test the hypothesis that these patterns are discernible in the observed record. These approaches are complimentary because discriminant analysis does not start with a given pattern and test the hypothesis that it is present in the data; it is just a way of describing the data. A purely descriptive analysis can only take you so far, but for some purposes it is advantageous to let the data tell you what the dominant patterns are, rather than having models suggest how to project out interesting degrees of freedom. In any case, you can do better than take a global mean if you want to reduce the noise level in the data.

The information content in the global mean depends on how many distinct patterns are present. Let's assume that one has already isolated from the full time series what one might call "climate change", either through a discriminant analysis or some other algorithm. If the evolution of the signal is dominated by one perturbation pattern $T(x,t) \approx A(t)B(x)$, and if we normalize the pattern B so that it has an integral over the sphere of one, we can just think of the perturbation to the global mean as equal to A, the amplitude of the pattern. If 2 (or more) things are going on that contribute to observed climate changes, you are obviously going to need 2 (or more) pieces of data to describe the observations, and the value of the global mean is more limited.

If the response to CO_2, or the sum of the well-mixed greenhouse gases, is linear, the spatial response of surface temperature could still be a function of the frequency of the forcing changes. If one assumes in addition that this frequency dependence is weak, as in the "intermediate" regime discussed in earlier posts, then one can expect evolution of the forced response that is approximately self-similar, with a fixed spatial structure, in which case the global mean is a perfectly fine measure of the amplitude of the forced response.

It is easy to come up with examples of how an exclusive emphasis on global mean temperature can be confusing. Suppose two different treatments of data-sparse regions such as the Arctic or the Southern Oceans yield different estimates of the global mean evolution but give the same results over data rich regions. And suppose, for the sake of this simple example only, that the actual climate change is self-similar, $T(x,t) \approx A(t)B(x)$ and is, in fact, entirely the response to increasing well-mixed greenhouse gases. One is tempted to conclude that the method that gives the larger global mean warming suggests a larger "climate sensitivity". But both would be providing the same estimate of the response to greenhouse gases in data-rich regions.

There are other interesting model-independent multivariate approaches to describing the instrumental temperature record besides the discriminant analysis referred to above. Typically one needs to choose something to maximize or minimize. For example, DelSole 2006 maximizes the integral time scale, $\int \rho(\tau)d\tau$, where ρ is the autocorrelation function of the time series associated with a particular pattern. I encourage readers to think about other alternatives.

8 The Recalcitrant component of Global Warming

[Originally posted on April 16, 2011]

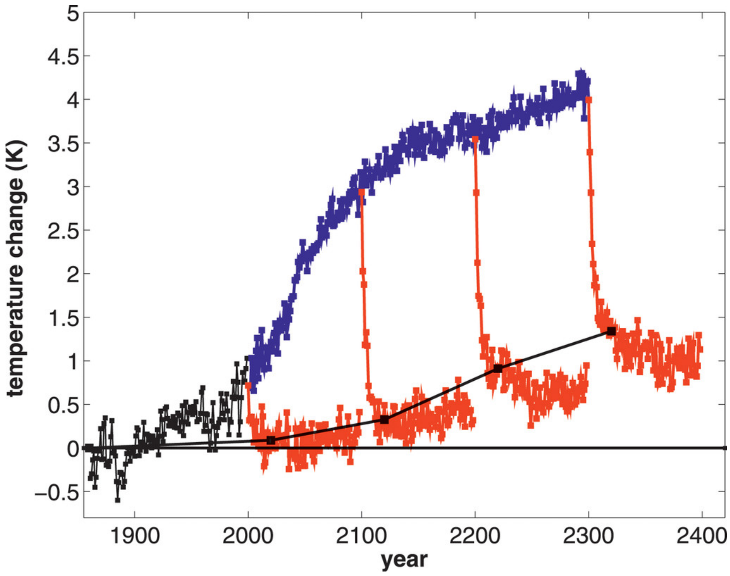


Figure 8.1: Evolution of global mean near-surface air temperature in GFDL's CM2.1 climate model in simulations designed to separate the fast and slow components of the climate response in simulations of future climate change, as described in Held et al 2010.

Continuing our discussion of transient climate responses, I want to introduce a simple way of probing the relative importance of fast and slow responses in a climate model, by defining the recalcitrant component of global warming, effectively the surface manifestation of changes in the state of the deep ocean.

The black curve in this figure is the evolution of global mean surface air temperature in a simulation of the 1860-2000 period produced by our

CM2.1 model, forced primarily by changing the well-mixed greenhouse gases, aerosols, and volcanoes. Everything is an anomaly from a control simulation. (This model does not predict the CO_2 or aerosol concentrations from emissions, but simply prescribes these concentrations as a function of time.) The blue curve picks up from this run, using the SRES A1B scenario for the forcing agents until 2100 and then holds these fixed after 2100. In particular, CO_2 is assumed to approximately double over the 21st century, and the concentration reached at 2100 (about 720ppm) is held fixed thereafter. The red curves are the result of abruptly returning to pre-industrial (1860) forcing at different times (2000, 2100, 2200, 2300) and then integrating for 100 years. The thin black line connects the temperatures from these four runs averaged over years 10-30 after the abrupt turn-off of the external forcing agents.

One can think of the red lines as simulations of what we might call instantaneous perfect geoengineering, in which one somehow contrives to return the CO_2 (and all of the other forcing agents in these simulations) to pre-industrial values. Perfect geoengineering so defined must be clearly distinguished from two other simple hypothetical scenarios discussed in the literature. (Let's simplify things by just thinking of CO_2 as the only relevant forcing agent.) One such scenario consists of just holding the CO_2 fixed after a certain time, as in the A1B scenario after 2100 (the blue line) in the figure. The warming that occurs after 2100 as the system approaches its final equilibrium is referred to as the *committed warming* but it might be better to refer to it as the *fixed concentration commitment*. A second, in many ways more interesting, simple scenario (e.g. Solomon et al 2009; Matthews and Weaver 2010) consists of abruptly setting the emissions to zero. This is another definition of commitment, which we might call the past emissions commitment, the study of which requires a coupled carbon-climate model. Unlike the fixed concentration commitment, it often results in temperatures that stay roughly unchanged for centuries — the warming due to the reduction in ocean heat uptake is roughly balanced by the ocean uptake of CO_2. Perfect geoengineering is much harder than even setting emissions to zero, of course, since one would have to take enough CO_2 out of the atmosphere to return to its pre-industrial value. Our interest in this scenario is not primarily because of its practical relevance but rather as a convenient probe of climate models.

There are similarities in the evolution after the turnoff of the radiative forcing for the 2100, 2200, and 2300 cases (these all have the same CO_2 at the time of the turn-off). At first the temperature decays exponentially, with an e-folding time of 3-4 years. An exponential fit yields a cooling in this fast phase of 2.6-2.7K in each case, leaving behind what we refer to as the recalcitrant warming. The spatial structure of the fast response is very similar in these three cases as well, and differs substantially from the spatial structure of the recalcitrant remnant. These are single realizations so some of the slow evolution after the turnoff of radiative forcing could be due to background internal variability. See Held et al 2010 for some further discussion of these simulations. Wu et al 2010 discuss aspects of the response of the hydrological cycle in similar model setups.

In thinking about the recalcitrant warming, it is useful to return once again to our two box model (post 4), ignoring the limitations of this model discussed in essay 5:

$$\begin{aligned} c\,dT/dt &= -\beta T - \gamma(T - T_O) + \mathcal{F}(t) && (8.1)\\ c_0\,dT_O/dt &= \gamma(T - T_O) && (8.2) \end{aligned}$$

On time scales long compared to the fast relaxation time of the surface box with temperature T, we have

$$T = (\mathcal{F} + \gamma T_O)/(\beta + \gamma) \tag{8.3}$$

When the forcing $\mathcal{F}$ is turned off, the solution relaxes on the fast time scale to

$$T_{\mathcal{R}} \equiv \gamma T_O/(\beta + \gamma), \tag{8.4}$$

so the response is the sum of the recalcitrant part $T_{\mathcal{R}}$ and fast response proportional to the forcing

$$T_{\mathcal{F}} \equiv \mathcal{F}/(\beta + \gamma) \tag{8.5}$$

An important implication of this plot, taking it at face value, is that the recalcitrant component of surface warming is small at present, implying that the response up to this point can be accurately approximated by the fast component of the response in isolation, which consists of rescaling the TCR with the forcing.

Another implication is that acceleration of the warming from the 20th to the 21st century is not primarily due to saturation of the heat uptake (this only accounts for the 0.4K growth of the recalcitrant component), but is primarily just due to acceleration of the growth of the radiative forcing.

It is important to keep in mind the limitations of this idealized picture. There is no reason to expect the slow response to be characterized by one time scale. Most importantly for this line of argument, there is no obvious reason why intermediate time scales, related to sea ice or the relatively shallow circulations that maintain the structure of the main thermocline, could not play more of a role in the transient response of surface temperature, filling in the spectral gap between our fast and slow time scales, and requiring a more elaborate analysis of the linear response in the frequency domain.

9 Summer is Warmer than Winter

[Originally posted on April 27, 2011]

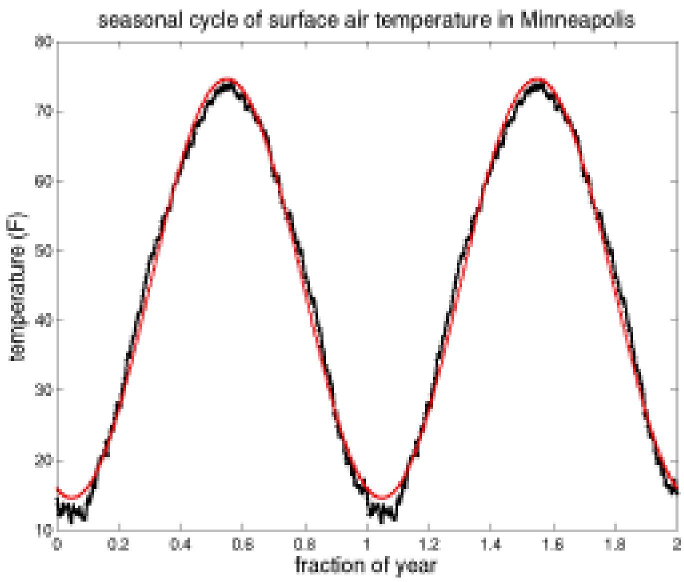


Figure 9.1: Black: Climatological seasonal cycle of temperature in Minneapolis, averaging (Tmax +Tmin)/2 over more than 100 years for each calendar day. The mean annual cycle is shown twice for clarity. Data available here. Red curve is a fit with annual mean plus fundamental annual harmonic.

Two common questions that I (and many others) often get are "How can you predict anything about the state of the atmosphere 100 years from now when you can't predict the weather 10 days in advance?" and "How do you know that the climate system isn't far more complicated than you realize or can possibly model?" I often start my answer in both cases with the title of this post. It may sound like I am being facetious, but I'm not;

the fact that summer is warmer than winter is an excellent starting point when addressing both of these questions.

Regarding the first question, we all successfully and continually predict the state of the atmosphere several months in advance whenever we plan our summer or winter vacations. Of course, the seasonal cycle is externally forced; no one can predict the chaotic day-to-day weather months in advance. The forced seasonal cycle is large enough that we are not tempted to change our summer vacation plans because of a cold snap in March, and it is not relevant to these plans whether we can forecast the duration of the cold snap for only, say, 5 days.

Analogously, when we talk about predicting the trend in the climate over the next 100 years due to a projected increase in carbon dioxide, we are talking about a forced response, fully analogous to predicting the extent to which summer is different from winter on average. The term "analogous" is potentially misleading since analogies are often vague and subjective. But here we are talking about the response of the same climate system to two changes in external forcing agents. The forcing in the global warming case manifests itself through a reduction in the net radiation escaping to space rather than a redistribution of the solar flux, so the details are different. And the time scales are different. And — the biggest difference of all, we have experienced a lot of seasonal cycles and don't have to rely on imperfect theories/models to tell us what the forced response is going to be. But is there any fundamental reason why one forced response should be less linear than the other?

There is a lot of interest in *seasonal forecasting*, predicting whether next summer will be warmer or wetter than average in some region. Skill in this arena is to a substantial degree due to ENSO, the evolution of which can be predicted with some skill months in advance, The phase of ENSO influences the atmospheric circulation around the globe substantially, which in turn influence the statistics of higher frequency variations, that is, the weather. There is a continuing search for other slowly evolving degrees of freedom that might be sources of predictability on this seasonal time scale, the state of the polar vortex in the stratosphere being another candidate. The analogous challenge in regards to global warming is that of predicting the decadal-to-multi-decadal internal variability, generated by the oceans, that has the potential to substantially modify the emerging forced signal.

Moving on to the question of complexity, I grew up in the Twin Cities of St. Paul and Minneapolis, so I enjoyed making some plots based in the data here and here of the seasonal cycle of surface air temperature in Minneapolis. See the figure above. In this figure, the max and min temperatures are averaged together for each day; the individual days are then averaged over 120 years or so. *No other smoothing is applied.* The mean seasonal cycle appears to be very smooth. In fact, it is almost exactly sinusoidal. (While growing up, I always thought that it was colder than it had any right to be in mid-winter; I can now point to the small mid-winter departure from a pure sine wave as support for this claim.) Despite the potential for complexity (I can assure you that clouds, for example, have a very different character in summer and winter in Minneapolis), I think we can agree that this is a pretty simple and intuitive temperature response.

Not all seasonal cycles of temperature are this sinusoidal. In the Arctic, for example, the summer gets truncated because temperatures are pegged to freezing and the energy goes into melting ice. Over parts of the oceans with a large seasonal cycle in the depth of the surface well-mixed layer, the warm season is more peaked and the cold season flatter because the heat capacity of the part of the ocean that is tightly coupled to the surface is larger in winter. More counter-intuitive is the spatial structure of the phase of the seasonal cycle near the equator in the eastern Pacific (see Horel 1982).

Why is the seasonal cycle in Minneapolis temperatures so simple despite the nonlinear chaotic behavior of the weather making up these averages? Is it because the seasonal cycle is so large compared to internal variability, so that it just overpowers any attempt of the internal variability to couple with it and create more counter-intuitive behavior. This sort of thing can happen in periodically forced nonlinear oscillators. Would the seasonal cycle get more complicated if one reduced its amplitude — by decreasing the obliquity of the Earth (the angle between the axis of rotation and the normal to the orbital plane), leveling the playing field between the seasonal cycle and internal modes of variability? I doubt it, primarily because one still would have a lot of separation in frequency between the bulk of the intrinsic variability, with characteristic time scales of days, and the seasonal cycle. (Are there any modeling studies with very small obliquity?) The deep tropics may be a counterexample, involving the interplay between intrinsic ENSO dynamics and the seasonal cycle, in which "Devil's staircase" type of complexity is a possibility — see Jin et al 1994 but even here the system seems too noisy for this kind of complexity to dominate.

What kind of internal dynamics might plausibly couple nonlinearly with the response to the anthropogenic carbon pulse? The decadal to multi-decadal variability typically associated with the thermohaline circulation in the Atlantic is a candidate. But my impression is that it is a lot harder to generate serious nonlinearity when internal variability interacts with a monotonic forced response (global warming) than with, a periodic forcing (seasonal cycle)

At the extreme, there is the possibility that the climate system, and climate models, exhibit structural instability — that climate does not vary smoothly as parameters are varied, not just at isolated bifurcations but more generically. See here, here and here for different perspectives on this issue. This is not an easy topic, and one that I have a lot to learn about. But I wouldn't advise you to cancel your summer vacation plans just yet.

10 Atlantic Hurricanes and Differential Tropical Warming

[Originally posted on May 11, 2011]

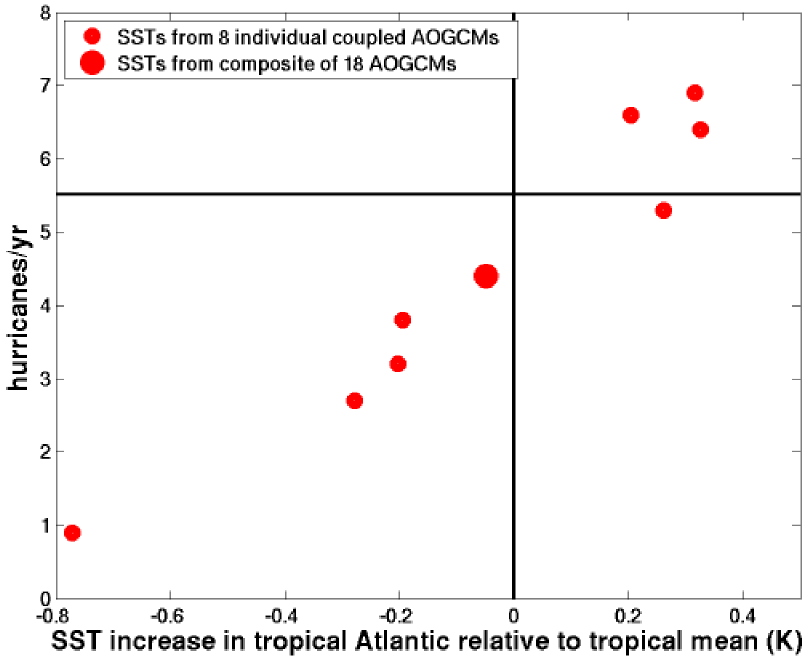


Figure 10.1: The number of Atlantic hurricanes simulated by the HIRAM model of Zhao et al 2009, when boundary conditions are altered to correspond to the changes in sea surface temperature (SST) simulated in 8 of the CMIP3/AR4 models for the A1B scenario by the end of the 21st century (small red dots), and to the ensemble mean of the changes in SST in 18 CMIP3/AR4 models (big red dot). The horizontal line indicates the number of hurricanes/yr in the control simulation.

Given the global atmospheric/land model described in Essay 2, which appears to simulate certain aspects of the statistics of tropical cyclones in the Atlantic quite well, what does the model predict for the change in these statistics in the future? And how seriously should we take the result?

Calculations in which the evolution of the ocean and sea ice are computed by one model, from which boundary conditions are extracted for a (higher resolution) atmosphere/land model, are often referred to as "time-slices". This is the setup that we are using here. It has important limitations, which I will eventually get around to discussing.

We have taken the SST anomalies generated by 8 different coupled atmosphere-ocean models in the CMIP3 archive, looking at the trend over the 21st century in the A1B scenario, in which CO_2 doubles over the century, ending up at about 720ppm. We use a 20 year control simulation of the atmospheric model in which the prescribed SSTs repeat every year and then perturb these SSTs by the spatially and seasonally varying warming trend from these 8 models in turn. (We also double the CO_2, but the response of the model to this change in CO_2 with fixed SSTs, although interesting, is comparatively modest and, in any case, the same in each experiment. CO_2 and the other forcing agents active in this scenario can be thought of as exerting their effect on Atlantic storms in this model primarily through the SST perturbation.)

The results for Atlantic hurricanes are presented on the vertical axis in the figure at the top. From a control value in the model of about 5.5/yr the warmed climates produce numbers ranging from close to zero to a 30% increase over the century. Also shown as a big red dot is the result from running the model over the ensemble mean of the ocean warming patterns from 18 different AR4 models.

At first sight this spread is a bit discouraging. It is noteworthy that none of these simulations produces a dramatic (factor of 2 say) increase in frequency. But more interesting is the correlation with the quantity plotted on the horizontal axis: the differential warming of the Atlantic with respect to the warming of the tropical mean ocean surface — more precisely, the increase in SSTs averaged over Aug-Sept-Oct and over the Atlantic Main Development Region (MDR) (defined here as [80W-20W, 10N-25N]) minus the average in the same time period for the tropical mean SSTs (30S-30N). The modeled SSTs that produce more storms in our time-slice simulation warm the Atlantic MDR more than the tropical mean, and vice-versa. Close inspection of the plot also suggests a modest reduction in hurricane frequency even when the warming is uniform (a result we have confirmed by increasing SSTs uniformly over the globe — and simultaneously increasing the CO_2).

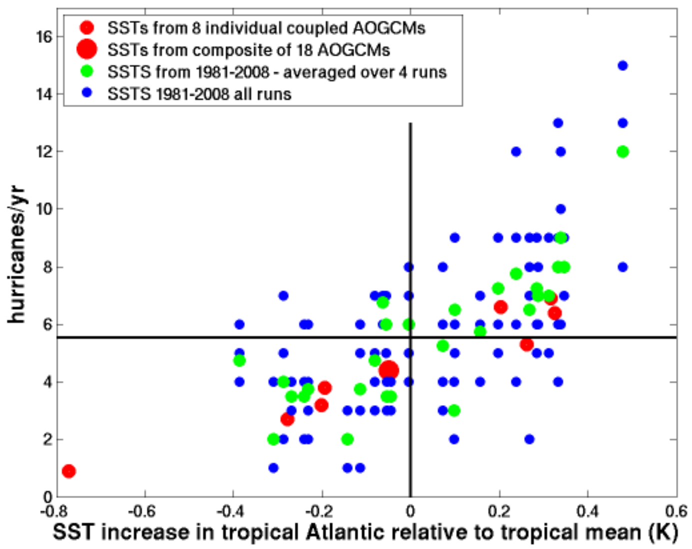


Figure 10.2:

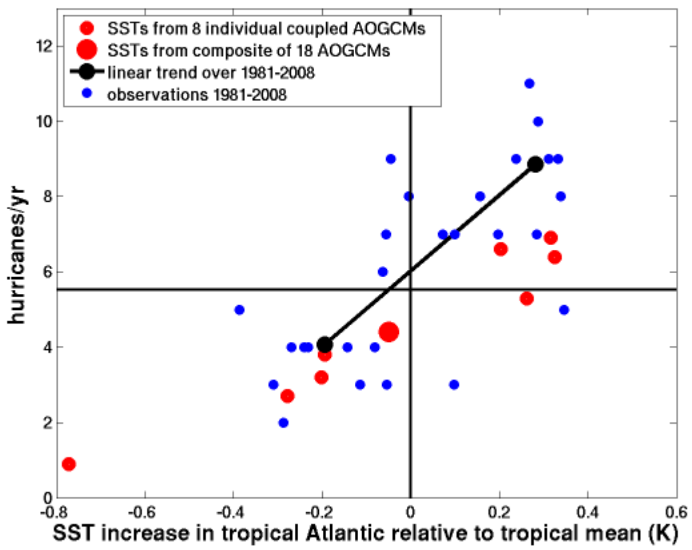


Figure 10.3:

It is informative to plot all of our simulations that use observed SSTs (from HadISST) from each year (discussed in post 2) on the same figure, as in Fig. 10.2 (top: blue dots are 4 individual realizations for each year; green dots ensemble means for each year; red dots are future projections using A1B scenario SSTs as before.) One can make the same plot for observations - Fig.10.3 (blue dots, using IBTrACS and HadISST). Ideally, the observations would look like a sample of the blue dots in Fig. 10.2.

The simulated and observed variations over the 1981-2008 period show about the same relationship between hurricane counts and differential warming of the Atlantic MDR as do the climate change simulations. So it does not look like the global warming case introduces any new dynamics that is not present in the interannual variability. What is the underlying dynamics?

My picture of the tropics is that of a circus tent (the temperature in the tropical upper troposphere) held up by poles (deep tropical convection) of different heights (the SST determining how high the pole is). The tent is made of a stiff material (horizontal temperature gradients are hard to maintain in the tropics above the boundary layer), so if your pole is too short and/or too close to a bigger pole, it will not stand (the convection will fizzle). If Atlantic SSTs are not high enough, deep convection is muted, losing the competition to the Pacific. (A complication is that the shape of the tent held up by a single pole is not symmetric, but similar instead to the classic Matsuno-Gill model of the response to localized tropical heating.)

One implication is that if the Atlantic SSTs are not high enough, there won't be enough deep convection to organize into tropical storms. But possibly just as important, the vertical shears of the horizontal wind will be bigger, a classic factor suppressing storm development. Very roughly, vertical shears are larger the farther away one is from the bulk of the tropical convection. (In the vicinity of the convection there is low shear typically but a lot of net upward motion and therefore upper level divergence of air and lower level convergence. As one moves away from the center of this convergence/divergence pattern, differences in the flow in the upper and lower troposphere get larger — even more so because the Coriolis force turns the upper and lower level flows to create additional shear in the wind component perpendicular to the inflow/outflow.)

The differential warming index defined here is not necessarily the best one could come up with (and admittedly works much better in the Atlantic than in other basins.) Among other things, it does not take into account the shape of the "tent", or of the spatial structure of the winds related to this shape. Even more simply, rather than the tropical mean as reference, one would probably be better off with a (climatological precipitation)-weighted mean, as in Sobel et al 2002 — it doesn't matter what the SST is in regions where the poles are always too short to contribute to holding up the tent..

If we accept this differential warming hypothesis, it implies that the upward trend in hurricane frequency over the past few decades in the Atlantic exists because the tropical Atlantic has warmed faster than the tropics as a whole over that period. If you compute linear fits to the time evolution of hurricane count and of this differential warming index from 1981-2008 the result is that one moves along the black line in Fig. 10.3

If we scale down the differential warming produced by the different GCMs over the 21st century by the ratio of the projected global or tropical mean warming over the century to that observed over this 30 year period, we see that none of the model results approach the magnitude of the observed trend. These projections are dominated by the increase in the well-mixed greenhouse gases. The implication is that either 1) models are grossly underestimating the potential for large differential warming of the tropical oceans forced by increasing well-mixed greenhouse gases, or 2) the observed differential warming, and the corresponding increase in Atlantic hurricanes, is primarily due to something else, either multi-decadal variability in the Atlantic, or a reduction in aerosol forcing over this period, or some combination. If 2) is the case, as these results suggest, we should not assume that this trend will continue into the future.

These differential SST changes are small and powerful. The slope we are talking about is roughly 1 hurricane/0.1K, or a doubling for a 0.5K differential warming! Can we trust the SST observations at the level needed to test this hypothesis adequately?

If one accepts this differential warming hypothesis, we are punting back to the coupled atmosphere-ocean models. We need more confidence in the spatial pattern of warming in the tropics, not just the overall level of tropical warming, to infer changes in hurricane frequency in the Atlantic. Better downscaling strategies will not, in themselves, reduce uncertainties much.

Let me emphasize that I am addressing here the narrow question of the frequency of all Atlantic hurricanes, not changes in the hurricane intensity distribution. Several lines of argument, not discussed here, suggest that changes in the intensity distribution could result in changes in the frequency of extreme (category 4-5) hurricanes that are very different from changes in the total number of hurricanes. Also not addressed are possible shifts in the locations of hurricane activity within the Atlantic basin that might change the fraction of hurricanes that make landfall.

[The GCM results, and the discussion of their impications, are based on collaborative work with Ming Zhao, Gabriel Vecchi, Tom Knutson and other GFDL colleagues.]

11 Is Continental Warming a Slave to Warming of the Ocean Surface

[Originally posted May 24, 2011]

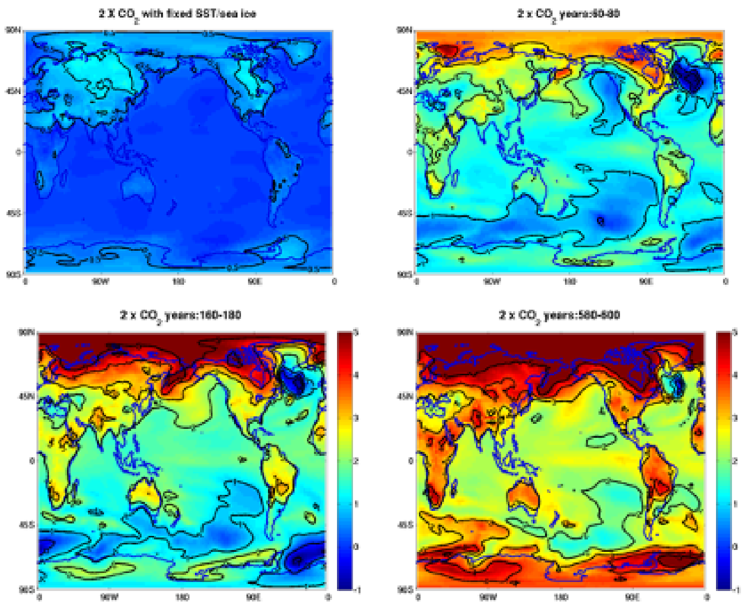


Figure 11.1: Annual mean surface air temperature response to a doubling of CO_2. Upper left – atmosphere/land response (GFDL AM2.1/LM2.1) with fixed sea surface temperatures (SSTs) and sea ice. Other plots are coupled model (CM2.1) responses in a single realization with CO_2 increasing at 1%/year till doubling (year 70) then held fixed. Upper right – average over years 60-80;; lower left – years 160-180; lower right – years 580-600. Contour interval is 0.5C in upper left and 1C elsewhere.

Returning to our discussion of the time scales of the climatic response, it is interesting to take a closer look at the evolution of the warming in a GCM for the standard idealized scenario in which, starting from an equilibrated state, CO_2 is increased at 1% per year until it doubles and is then held fixed. Fig. 11 shows the results from GFDL's CM2.1 model.

I want to focus especially on the upper left panel, which is not generated from the fully coupled model, but from the atmosphere/land components of this model in isolation, holding the sea surface temperature (SST) and sea ice distribution fixed at their unperturbed climatological seasonal cycles, while doubling the CO_2. This model equilibrates to a change in CO_2 in a couple of months (there is no interactive vegetation or even permafrost in this model, both of which would create the potential for longer time scales). The response depends on the season, so one has to integrate for at least a year before this annual mean pattern emerges. We might call this the ultra-fast response, distinguishing it from fast (oceanic mixed layer), slow (oceanic interior), and ultra-slow (anything slower than the thermal adjustment time of the interior ocean, such as aspects of glacier dynamics). One can visualize this as the first step in the response, but one that is dramatically modified over time by the ocean warming and sea ice retreat.

This ultra-fast response is weak, far smaller than the transient climate response (defined as the global mean of the upper right panel). Averaged over all of the continents, the surface air warming with fixed SSTs and sea ice is about 0.35C in this model, rising above 1C only in the interiors of Eurasia and North America. Therefore, most of the warming over land results from warming of the oceans (and, especially in high latitudes, the retreat of sea ice). This qualitative result is robust across all models. Transport of heat from the oceans to the centers of continents takes place on time scales of a few weeks to a month, which is comparable, or perhaps a bit shorter, than the radiative relaxation time scale for the atmosphere/land system in isolation. A key takeaway is that the warming over the land surface is tightly coupled to the oceanic warming on time scales longer than a month.

Using a GCM, can we regenerate the land temperature record from the ocean record using observed SSTs and sea ice distribution as a boundary condition? This is not simply a question of heat transport by the atmosphere but also of wave-like "teleconnections" propagating away from regions of tropical convection that are altered by the pattern of tropical warming. Compo and Sardeshmukh 2009 provide a recent discussion, strongly supporting the strength of the oceanic constraint on land temperature trends. So there does seem to be considerable redundancy between the observed land and ocean records of temperature trends. This does not make the land record any less important. Redundancy is critical when data sets and models are imperfect.

There is something else that one can do with the atmosphere/land only computation with fixed SSTs and sea ice: one can compute the globally averaged net energy flowing in at the top of the atmosphere (TOA), or, what is essentially equivalent, the energy flux into the ocean. This is how we compute the "forcing" $\mathcal{F}(t)$ for use in the simple energy balance model described in Essay 3. There are things that happen on this ultra-fast time scale in response to the increase in CO_2 other than the modest warming in the continental interiors, one of the most important being that the stratosphere cools. If one tries to compute radiative forcing without taking into account the stratosphere cooling, one has to deal with a big difference in the energy imbalance between the TOA and the tropopause — this imbalance being precisely what causes the stratospheric cooling.

But focusing on the flux imbalance at the tropopause is a bit awkward, partly because the definition of the tropopause can be fuzzy. It can be simpler to just let the model adjust the stratosphere as it sees fit, then see how much energy is flowing into the system. This is an essential feature of the classical 1-D radiative convective model. In a GCM, fixing the SSTs and sea ice is a simple approximate way to do the same thing. It seems to have the drawback that one is allowing the land to adjust a bit — and other things happen to the hydrological cycle as well — introducing some model-dependence into the definition of forcing. But why allow some of the ultra-fast adjustment to occur (the stratospheric part) and not others? Defining a "forcing" before the TOA, tropopause, and surface ocean fluxes come into agreement can be confusing — for many purposes it is simpler to wait for the ultra-fast adjustment to occur to bring these three in line.

12 Using Model Ensembles to Reduce Uncertainty

[Originally posted June 13, 2011]

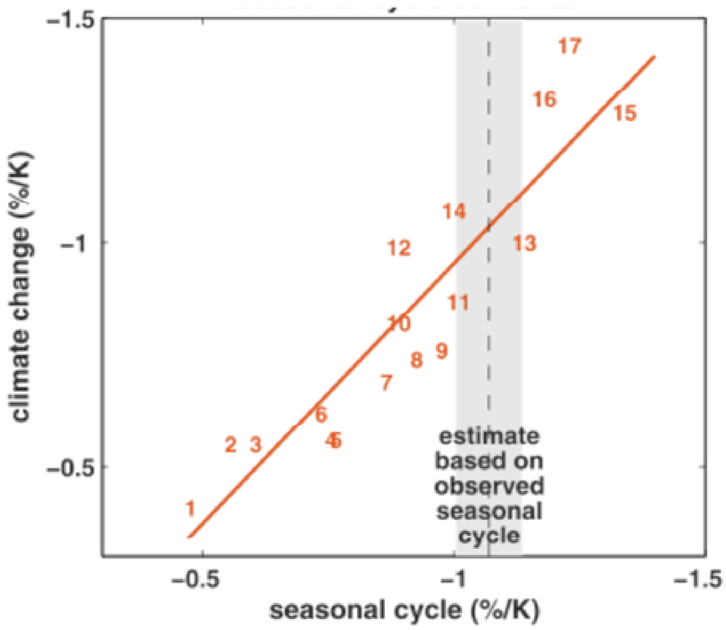


Figure 12.1: From Hall and Qu 2006. Each number corresponds to a model in the CMIP3 archive. Vertical axis is a measure of the strength of surface albedo feedback due to snow cover change over the 21st century (surface albedo change divided by change in surface temperature over land in April). Horizontal axis is measure of surface albedo feedback over land in seasonal cycle (April to May changes in albedo divided by change in temperature). The focus is on springtime since this is a period in which albedo feedback tends to be strongest.

There are a lot of uncertainties in how to simulate climate, so, if you ask me, it is self-evident that we need a variety of climate models. The ensembles of models that we consider are often models that different groups around the world have come up with as their best shots at climate simulation. Or they might be "perturbed physics" ensembles in which one starts with a given model and perturbs a set of parameters. The latter provides a much more systematic approach to parametric uncertainty, while the former give us an impression of structural uncertainty— ie, these models often don't even agree on what the parameters are. The spread of model responses is useful as input into attempts at characterizing uncertainty, but I want to focus here, not on characterizing uncertainty, but on reducing it.

Suppose that we want to predict some aspect P of the forced climate response to increasing CO_2 and that we believe a model's ability to simulate an observable O (let's think of O as just a single real number) is relevant to evaluating the value of this model for predicting P. For the i'th model in our ensemble, plot the prediction P_i on the y-axis and the simulation O_i on the x-axis. (Average over multiple realizations if needed to isolate the forced response.) The figure shows a case in which there is rather good linear relationship between the P_i's and O_i's. So in this case the simulation of O discriminates between model futures.

Now we bring in the actual value of O — the vertical shaded region in the figure. Because the simulated value of O discriminates between different predicted values of P, the observations potentially provide a way of decreasing our uncertainty in P, possibly rather dramatically, The relationship between O and P need not be linear or univariate, but there has to be some relationship if we are to learn anything constructive about P from the observation of O. And the observed value of O need not lie in the range of model values — if the relationship is simple enough extrapolation might be warranted. This will all seem obvious if you are used to working with simple models with a few uncertain parameters. But when working with a global climate model, and especially an ensemble of global models differing in many details, the problem is often finding the appropriate O for the P of interest.

Consider the effects of global warming on Sahel rainfall. This problem grabbed my attention (and that of several of my colleagues) because our CM2.1 climate model predicts very dramatic drying of the Sahel in the late 21st century (see Held et al 2005). But this result is an outlier among the world's climate models, many of which increase rainfall in the Sahel in the future. Using different criteria, one can come to very different conclusions about CM2.1's relative fitness for this purpose. For example, if one just looks at the evolution of Sahel rainfall over the 20th century, the model looks pretty good (the quality of the simulation is quite stunning if one runs the atmosphere/land model over the observed sea surface temperatures) — on the other hand, if one looks at some specific features of the African monsoonal circulation, this model does not stand out as particularly impressive. But no criteria, to my knowledge, has demonstrated the ability to discriminate between models that decrease and increase rainfall in the Sahel in the future.

For an example of an attempt at using observations and model ensembles to constrain climate sensitivity, see Knutti et al. 2006, who start with the spread of sensitivities within an ensemble and look for observations that distinguish high and low sensitivity models, in this case using the seasonal cycle of surface temperature. This is far from the final story, but I like the idea of using the seasonal cycle for this purpose — there is something to be said for comparing forced responses with forced responses. A closer look at the seasonal cycle of Sahel rainfall in models and observations might be warranted to help reduce uncertainty in the response of Sahel rainfall to increasing CO_2. I also suspect that attempts at constraining climate sensitivity with satellite observations of radiative fluxes might also benefit from more of a focus on the seasonal cycle as opposed to, say, interannual variability.

13 The Strength of the Hydrological Cycle

[Originally posted June 29, 2011]

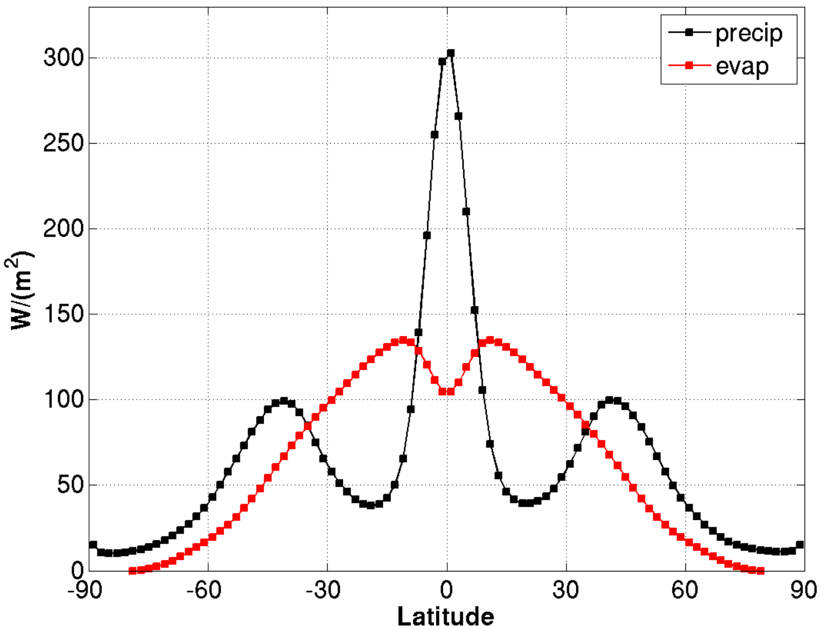


Figure 13.1: Time-mean precipitation and evaporation as a function of latitude as simulated by an aqua-planet version of an atmospheric GCM (GFDL's AM2.1) with a homogeneous "slab-ocean" lower boundary (saturated surface with small heat capacity), forced by annual mean insolation.

One often hears the statement that the "strength of the hydrological cycle" increases with global warming. But this phrase seems to mean different things in different contexts.

The total amount of water vapor in the atmosphere has been increasing over the oceans, as clearly seen in the SSM/I microwave measurements of column integrated water vapor. The increase is as expected from the observed increase in ocean surface temperatures and the assumption of fixed relative humidity in the lowest 2-3 kms of the atmosphere, where most of the vapor resides, and is very consistent with atmospheric model simulations in which one uses the observed ocean temperatures as a boundary condition (e.g., Fig. 1 in Held and Soden 2006). This increase is a consequence of the increase in saturation vapor pressure with temperature; it has nothing directly to do with the "strength of the global hydrological cycle" — by which I mean the globally averaged value of precipitation or evaporation. We need to avoid statements like "water vapor increases because of increased evaporation as climate warms".

Globally averaged, precipitation balances evaporation (plus transpiration from plants) to an excellent approximation. To produce something like the observed rate of increase in vapor requires a sustained imbalance of roughly 0.002%. This minsicule difference obviously cannot be directly observed. In any case, it should be thought of as caused by the increase in atmospheric water, rather than causing it.

Think of a bucket (containing vapor) with "evaporation" filling the bucket, the overflow being "precipitation". Now make the bucket bigger — but very slowly as compared to the time it takes evaporation to fill the bucket. If we hold evaporation fixed, precipitation will adjust by decreasing a tiny bit as the content of the bucket slowly increases while remaining full. Or let evaporation decrease; the content of the bucket will still increase as before as the bucket slowly grows. Analogously, it is not hard to generate a situation in a climate model in which temperatures warm and water vapor increases while evaporation and precipitation decrease in the global mean (one can increase carbon dioxide while increasing the amount of absorbing, non-scattering aerosol).

A better picture results if we assume that there are a lot of buckets moving around, each representing the saturation vapor pressure of different air parcels, and that they change their size depending on the temperature of the air in which they find themselves. If a bucket makes an excursion upwards it cools and contracts, then expands to its original size when it returns to its previous pressure and temperature. (Think Alice in Wonderland.) A lot of vapor is lost, to precipitation, when the bucket shrinks, so it is only partially filled when it returns. We should also assume that the rate of evaporation depends on the fullness of the bucket when it nears the surface — an empty bucket results in high evaporation, with the rate of evaporation slowing as the bucket fills. The strength of the global hydrological cycle can then be thought of as regulated by the frequency and amplitudes of the up and down excursions of the buckets, which control how empty the buckets typically are when they approach the surface. But what determines the statistics of these vertical motions?

Without trying to paint a full picture, I'll just note that the dominant terms in the energy budget of the free troposphere, the region above the planetary boundary layer, are the radiative cooling and the latent heating associated with condensation. The easiest way to increase the strength of the global hydrological cycle in a model is to increase the net radiative cooling of the free troposphere (for example, by reducing absorbing aerosol or decreasing cirrus cloud cover). The dynamics of how this radiative cooling affects vertical motions, near surface humidities and evaporation might be intricate (more cooling $\Longrightarrow$ destabilization of the troposphere $\Longrightarrow$ more vertical excursions $\Longrightarrow$ more precipitation to balance the cooling, and also drier air entrained into the boundary layer $\Longrightarrow$ more evaporation) but you can't easily avoid energy balance constraints. The controls on the strength of the global mean hydrological cycle are very different from the controls on the water vapor itself.

But when people say that the strength of the hydrological cycle is increasing, they are not necessarily referring to global mean evaporation or precipitation; sometimes they are referring to the redistribution of water by the atmosphere from one region to another.

Starting near the surface in the subtropics, where a lot of evaporation occurs, there are various kinds of air trajectories that can be thought of determining the pattern of precipitation. Some trajectories stay close to the surface as they move equatorward and then move upwards as they reach the "intertropical convergence zone (ITCZ)" marking the tropical rain belts. Others are swept polewards and upwards by midlatitude storms, creating the precipitation maxima marking the storm tracks. Buckets return relatively empty to the subtropical surface from either direction, helping to create an evaporation maximum in the subtropics. The distribution of precipitation and evaporation in an idealized version of GFDL's AM2 atmospheric model with a zonally symmetric (longitude independent) lower boundary and, therefore, a zonally symmetric climate, is shown in the figure at the top.

Now increase CO_2 in this idealized model and let it's climate equilibrate (which it does in a few years because the ocean has been replaced by a thin vertically homogeneous immobile slab of water). The result is that temperatures and water vapor increase while relative humidity doesn't change much. Neither do the statistics of air trajectories change very dramatically, so buckets are still moving polewards and upwards from the subtropics into midlatitudes, shrinking by about the same proportions as before, but now dropping out more water because the buckets are bigger. So we expect the pattern of precipitation minus evaporation (P-E), which balances the convergence of the net atmospheric water flux, to be enhanced. As shown in the following figure, this is what happens in this model. Evaporation changes are relatively small (but see Wentz et al 2007), so the change in P-E is a good first approximation to the change in P. And one can predict

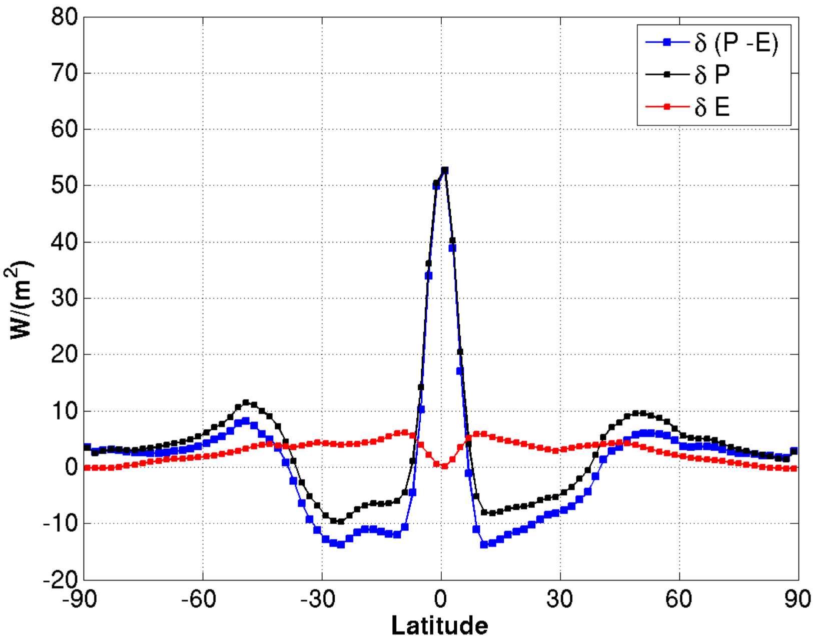


Figure 13.2:

the change in P-E pretty well from the change in temperature and the increase in saturation vapor pressure. The “prediction” in fig.13.3 refers to the assumption that the model’s moisture fluxes increase proportionally to the increase in lower tropospheric (700-100omb) saturation vapor pressure. This argument is more compelling outside of the tropics, where the motions carrying water vapor around are created by dynamics that is only secondarily affected by the latent heat release occurring in this process. In the tropics, latent heat release is an integral part of the dynamics causing these vertical motions, so there is no particular reason to assume that trajectories in the tropics will remain more or less the same if latent heating changes substantially. The figure suggests that the polar edges of the subtropics, near 30 degrees latitude, are drying out more that expected on the basis of this simple fixed flow, fixed relative humidity argument. In fact the Hadley circulation is expanding a bit, and, it seems, air trajectories are now bring-

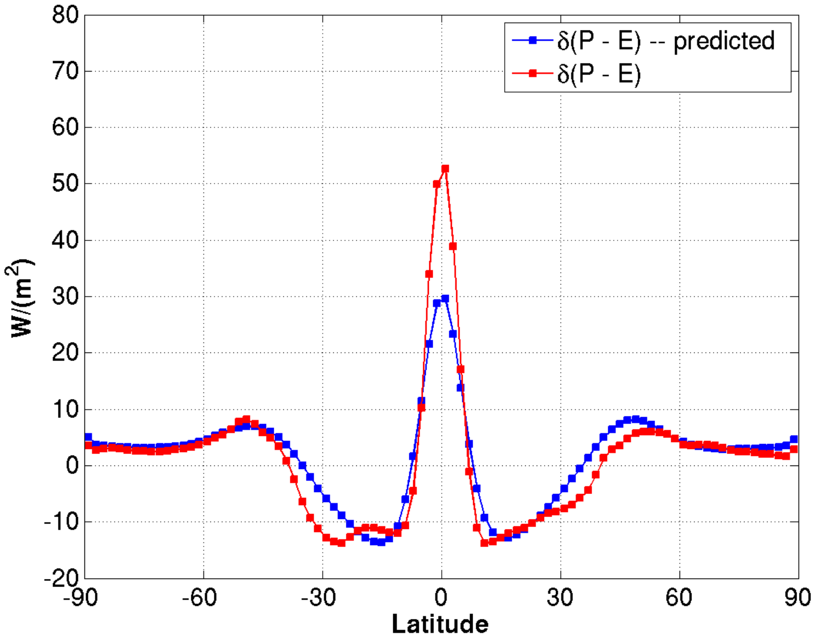


Figure 13.3:

ing water into the ITCZ from somewhat further away, enhancing the ITCZ precipitation.

It is confusing to refer to this increase in water fluxes, causing increasing gradients in P-E, as an “increase in strength of the hydrological cycle”. An “increase in water fluxes within the atmosphere” is more understandable and more accurate.

The regional redistribution of precipitation due to the increase in atmospheric flux scales with the change in temperature. In fact, since the pattern of temperature change tends to be quite similar in different models, this redistribution tends to scale, across models, with global mean temperature change — that is, with climate sensitivity. One often hears the question “who cares about global mean temperature?”, but regional precipitation changes are more closely related to changes in global mean temperature than they are to changes in the global mean hydrological cycle.

Regional precipitation changes are certainly sensitive to regionally specific changes in circulation as well. An idealized GCM such as the one used here for illustration, with its homogeneous lower boundary, serves to isolate those aspects of the hydrological response that do not depend on local meteorology and boundary conditions.

14 Surface Salinity Trends

[Originally posted July 8, 2011]

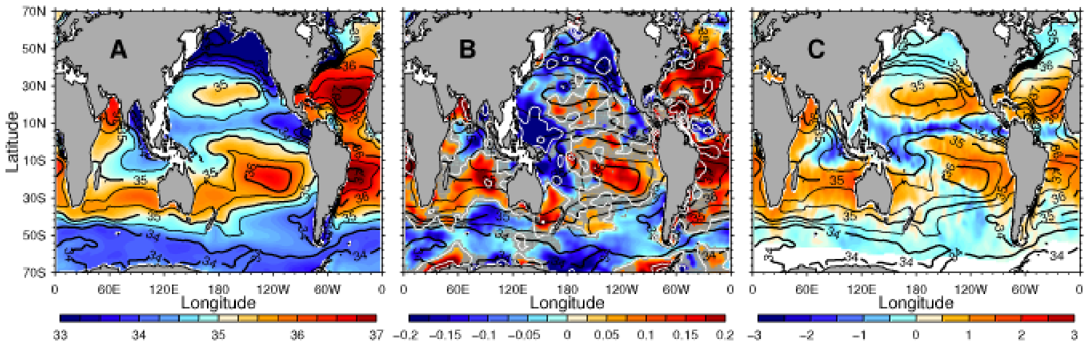


Figure 14.1: From Durack and Wijffels 2010: A) Climatological surface salinity (0.5 pss contour), averaged over 1950-2000; B) the linear trend over these 50 years (pss/50 years) ; and C) the NOCS Southampton estimate of net climatological freshwater flux from ocean to atmosphere (m/yr).

In post 13, I discussed the argument that warmer temperatures $\Longrightarrow$ more water vapor in the atmosphere $\Longrightarrow$ more transport of water away from regions from which the atmosphere habitually extracts water, and more transport to regions into which the atmosphere habitually adds water. The consequence is the expectation that "the wet get wetter and the dry get drier" if by wet/dry we mean regions with precipitation (P) greater than/less than evaporation (E). In that post, I effectively ignored the presence of land. Land introduces a variety of complications that make this kind of argument more difficult, most obviously because of the constraint that P must be greater than E on the time scales of interest (ie. changes in water storage on land can be ignored and runoff must be positive). I am going to continue to ignore the existence of land (and glaciers) in this post!

What is the evidence for trends in P or E or P-E over the oceans? Trends in the ocean salinity field promise to provide a test of our understanding — it is also helpful that the oceans provide a low-pass filter to noisy precipitation signals.

Oceanic salinity is driven by changes in the net fresh water input at the surface, P-E. (It is also forced by river discharge from the land and from melting icecaps.) Salinity is a tracer, carried by the oceanic flow and ultimately subject to molecular diffusion on the smallest scales. It cares only about the redistribution P-E and not P and E individually. This is convenient since, as discussed briefly in essay 13, the constraints on how the the global mean P or E might change with warming are very different from the constraints on the changes in P-E, and it is helpful to test our understanding of these things separately.

The oceanic flow redistributes salinity through a mix of advection by steady circulations and transport by coherent eddies, chaotic advection, and turbulent diffusion. But salinity is also an active scalar which affects the density of seawater and, through the equation of motion, changes the flow. This is especially important in sub-polar regions — because of the form of the equation of state of seawater, salinity is, in fact, the primarily factor controlling density in the relatively cold subpolar oceans. If we could also ignore this inconvenient fact, salinity would becomes a passive tracer and its dynamics linear, driven only by the surface flux of freshwater P-E.

Suppose that we are given the statistically steady salinity distribution of a control climate and then assume that P -E is perturbed. In fact, consider the simplest possible case in which P-E is simply multiplied by a global constant, as the crudest possible representation of increased atmospheric water transport accompanying warming. If the circulation changes significantly, there isn't much we can do except to try to solve the full problem, coupling the salinity with the temperature and momentum equations — that is, to work with an oceanic GCM. But suppose we arbitrarily assume that the circulation does not change much, so that we can think of the salinity as passive and its equation as linear. Then the solution is just that the salinity is multiplied by the same constant multiplying P-E. The total salt in the ocean is unchanged, so what this means is that all spatial salinity gradients are multiplied by a constant once the system settles into a new steady state. (There are some small inconsistencies in this argument, partly resulting from the existence of the Goldsborough circulation — the circulation has to be perturbed to some extent to balance a change in the mass sources and sinks — but this circulation is small compared to the dominant oceanic currents driven by density gradients.)

Note that this does not mean that the distribution of P-E simply imprints itself on the surface salinity distribution without change in shape, even if circulation changed can be ignored. The advection-diffusion operator can make this relationship non-local. If the Atlantic is much saltier than the Pacific, then scaling P-E up, in this passive limit, will create an even larger gradient between the Atlantic and the Pacific, whatever combination of P-E distribution and circulation asymmetry can be thought of as generating this salinity gradient in the mean climatology.

Durack and Wijffels have recently presented an important new analysis of salinity trends over the past 50 years (see Figure and link at top of post.) The comparison of panel A, showing the climatological surface salinity gradients, with panel B, their estimated 50 year salinity trends, is striking. (Panel C shows an estimate of the climatological P-E pattern. The expectation is that estimates of oceanic salinity trends are more robust that any attempts at directly estimating trends in P-E over the oceans.) There is a correspondence in all major features between panels A and B, the upshot being that surface salinity gradients are, indeed, increasing throughout the world ocean. In particular, the average salinity difference between the saltier Atlantic and the fresher Pacific is increasing, as one would expect from the quasi-steady advective-diffusive response to a scaled up P-E pattern, with little change in ocean circulation. In the North Atlantic, the sub-polar gyre has a positive salinity trend (except along its western boundary), with no obvious sign of the freshening that might result from the reduced poleward salt transport from the subtropics that would accompany a weakening of the overturning.

To my eye, the trends in panel B are substantially larger than expected from Clausius Clapeyron and the roughly 0.5C warming over this time period. I am also a bit confused as to how long we should expect it to take for the Atlantic-Pacific salinity difference to respond to an increase in the magnitude of P-E, assuming that part of this difference is associated with the asymmetry of the circulation. (I would have guessed that this inter-basin gradient would be enhanced at a slower rate than the intra-basin gradients.)

This salinity trend analysis is deserving of close scrutiny (especially regarding effects of inhomogeneities in data coverage through this time period) given its potential to serve as a centerpiece for discussions of changes in the hydrological cycle associated with warming.

15 Fluctuations and Responses

[Originally posted July 26, 2011]

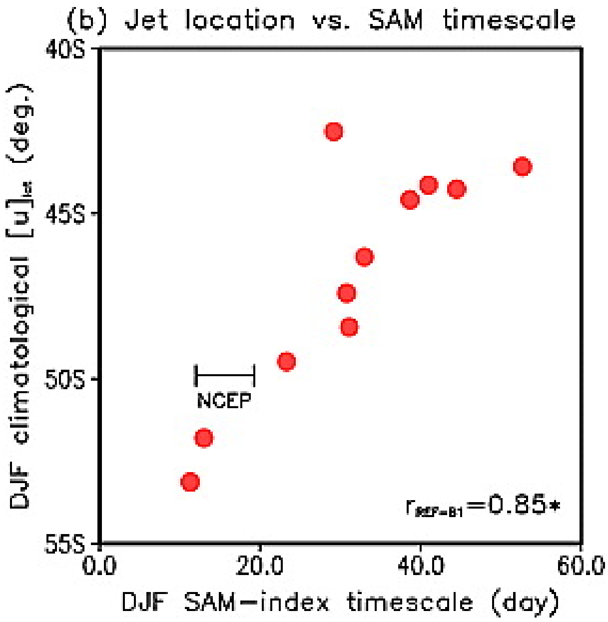


Figure 15.1: From Son et al 2010, based on the CCMVal ensemble of models, the decorrelation time of the Southern annular mode (SAM) plotted against the simulated latitude of the surface westerlies. Also included is an estimate from NCEP-NCAR reanalysis.

A series of studies over the past decade, starting with Thompson and Solomon 2002, have built a very strong case that the ozone hole in the Southern Hemisphere (SH) stratosphere has caused a poleward shift in the SH surface westerlies and associated eddy fields, especially during the southern summer. The poleward shift is often described as a trend towards a more positive phase of the Southern Annular Mode (SAM). The SAM is a mode of atmospheric internal variability characterized by north-south shifts in the surface westerlies.

The mechanism by which the ozone hole causes this poleward shift is a hot topic in dynamical meteorology. Not only is this response to the ozone hole important in itself, but related mechanisms likely govern the effects on the troposphere of stratospheric perturbations due to volcanic eruptions, the solar cycle, and internal variability. The starting point is the cooling of the lower stratosphere in the vicinity of the ozone hole, due to loss of UV absorption, thereby changing the north-south temperature gradient and associated wind fields in the lower stratosphere. But there are a lot of competing ideas about how altered lower stratospheric winds and temperatures in turn affect the fluxes of angular momentum that maintain the surface westerlies. (Some of my own lectures on the basic dynamics controlling the surface westerlies, including the key role of transport of angular momentum associated with the midlatitude storm tracks, can be found here.) GCMs consistently simulate a poleward shift in response to the ozone hole but of varying magnitude. They also consistently simulate a poleward shift due to increasing CO_2.

The IPCC AR4 rreport was noncommittal on the relative importance of greenhouse gases and the ozone hole for the observed SH wind shift. But a number of more recent papers have argued that this shift is primarily a response to ozone depletion, rather than CO_2 (see Son et al 2010, Polvani et al 2011). Does this mean that the westerlies will bounce back as the ozone hole heals, assuming that we can continue to avoid emitting CFCs? When models are used to project into the future, this bounce back typically does not occur; the tendency for the winds to return equatorward is compensated, or overcompensated in some models, by the effects of the CO_2 increase — the healing is projected to occur more slowly than did the generation of the ozone hole, so the CO_2 -induced trend in these models is more competitive with the ozone-induced trend in the future.

But how do we judge the ability of a model to simulate forced shifts in the midlatitude westerlies?. Gerber et al 2010 provide a nice summary of a variety of tests applied to the CCMVal model ensemble to evaluate the realism of the simulated stratosphere-troposphere coupling. I focus here on one specific test, related to the internal variability of the Southern Annular Mode.

Projecting atmospheric variability in observations or models onto the SAM index, one gets a covariance that can be approximated by exponential decay with a decorrelation, or persistence, time in the summer of 15-20 days. But this time scale varies quite a bit from model to model. Plotting the decorrelation time in the CCMVal set of models against the latitude of the mean westerlies as simulated in each model, one gets the very nice result in the figure at the top of the post: models that place the climatological westerlies further equatorward have internal variability with more persistence. Importantly, the decorrelation time of the observed SAM (in reanalyses) and the observed latitude of the westerlies falls nicely on the model ensemble regression line.

One can try to think of the SAM index ($\mathcal{S}$) as a damped degree of freedom forced by weather noise $\mathcal{W}$ with a white spectrum and zero time mean,:

$$d\mathcal{S}/dt = -\mathcal{S}/\tau + \mathcal{W} \tag{15.1}$$

We can estimate the decorrelation time τ from observations of $\mathcal{S}(t)$, since the covariance for this simple model is exponentially decaying in time:

$$[S(t)S(0)] = [S(0)^2]exp(-t/\tau) \tag{15.2}$$

Adding a time-independent eternal "force' $\mathcal{F}$ to the RHS of this equation, the time mean response is $\delta\mathcal{S} = \tau\mathcal{F}$, whch is proportional to the decorrelation time.

This one-degree-of-freedom linear analysis seems simplistic, but it has been supported in this context in a variety of climate models of differing complexity, at least qualitatively. There is a considerable and growing literature on the application of more general "fluctuation-dissipation" relations in the analysis of climate responses.

So the decorrelation time of the SAM index may be one of the most important things to get right if we want to model the circulation responses to the ozone hole and to CO_2. As the plot at the top indicates, it seems that if one's SH summertime westerlies are simulated too far equatorward, this correlation time will be too large, and the response of the SAM to the ozone hole will be too large, holding everything else fixed.

The CCMVal model ensemble used for the plot at the top of this essay is biased, with the latitude of the SH summer westerlies too far equatorward on average, suggesting that we should reduce the magnitude of the response estimated from the mean of the whole ensemble accordingly. It is interesting that this adjustment happens to takes us in the wrong direction compared to the observatioal estimates of the SAM secorrelation time and the latitude of the maximum midlatitude westerlies. Both of these climate model biases are subjects of curremt research; they seem to be related.

It is also interesting that the surface westerlies in SH summer are harder to get right than in any other season/hemisphere, as evidenced by the spread in model results (and personal experience). It also turns out that the annular mode time scale is largest here compared to other seasons and hemispheres. So it is hard to simulate for a good reason — the restoring forces are weak and the westerlies relatively easily perturbed by other model bisses.

Why is the latitude of the westerlies important for the SAM time scale? This restoring strength is known to be related to the interaction between storm track eddies and the midlatitude jet (see Lorenz and Hartmann 2001). There are some recent papers analyzing why this feedback is weaker when the surface westerlies and storm tracks are further polewards (e.g. Barnes et al 2010). One interesting implication of this relationship is that as one pulls the westerlies polewards it should get more and more difficult to pull them even further polewards.

Can one take a similar approach, using the covariance structure of internal variability, to constrain global climate sensitivity? The case of the SAM index and the tropospheric response to the ozone hole is nice in that this response has a structure that is very close to that of the SAM, a dominant mode of internal variability, so the argument that the decorrelation time of this mode is relevant is pretty compelling. The question in the global warming context is whether there are aspects of internal variability that resemble the forced response sufficiently — so we can have confidence that we are looking at the relevant restoring forces.

16 Heat Uptake and Internal Variability

[Originally posted August 23, 2011]

Suppose that most of the global mean surface warming in the past half century was due to internal variability rather than external forcing, contrary to one of the central conclusions in the IPCC/AR4/WG1 Summary for Policymakers. Let's think about the implications for ocean heat uptake. Considering the past half century in this context is convenient because we have direct, albeit imprecise, estimates of ocean heat uptake over this period.

Set the temperature change in question, T, equal to the sum of a forced part and an internal variability part: $T = T_F + T_I$, with $T_F = \xi T$, so ξ is the fraction of the temperature change that is forced. The assumption is that this is a linear superposition of two independent pieces, so I'll write the heat uptake as $H = H_F + H_I$.

When the surface of the Earth warms due to external forcing, we expect the Earth to take up heat. But what do we expect when the surface warms due to internal variability? Can we use observations of heat uptake to constrain ξ?

The strength of the radiative heat loss to space per unit warming of global mean surface temperature is a key quantity of interest, as usual. In Essay 5 I tried to emphasize that this parameter, which I denote by β, should depend, among other things, on the horizontal structure of the surface warming. This issue is of vital importance when discussing observational constraints on climate sensitivity, since the natural changes we observe – due to ENSO, AMO, volcanoes – do not all share the same horizontal structure with the forced response to CO_2.

But consider the two limiting cases: either the forced response dominates the half-century trend or internal variability is dominant. If both of these limiting cases are going to be viable, then they both have to have the same spatial structure, that of the observed warming. (In actuality, I am very skeptical that internal variability can create this spatial structure, but I am suspending this skepticism for the moment.) So, within the confines of this argument, with the intent of focusing on the limiting cases, it is interesting to assume that the strength of the radiative restoring is the same for the forced and the internal components.

For the forced response, I'll use the framework for discussing the transient climate response in Essay 4 in which the forcing F is balanced by the radiation to space and heat uptake, both of which are assumed to be proportional to T: $F = \beta T_F + \gamma T_F$. So $T_F = F/(\beta + \gamma)$, and the heat uptake H_F associated with the forced response is $\gamma F/(\beta+\gamma)$. A fraction $\gamma/(\beta+\gamma)$ of the radiative forcing is taken up by the Earth, the rest is radiated away due to the increase in temperature. This fraction can be quite modest. For example, using the numbers that mimic the behavior of GFDL's CM2.1, a GCM discussed in Essay 4, this ratio is $0.7/(1.6 + 0.7) \approx 0.3$. In this sense the forced response is rather inefficient at storing heat.

I am going to assume that F and γ are given and that the value of β is the point of contention. The fraction of the response that is forced depends on the value of the radiative restoring β according to

$$F = \xi(\beta + \gamma)T \tag{16.1}$$

or, expressing β as a function of ξ,

$$\beta = \frac{F}{T}\frac{1}{\xi} - \gamma. \tag{16.2}$$

Meanwhile suppose there exists internal variability with the spatial structure of the warming trend. As discussed above, I assume that internal variability radiates energy to space at about the same rate as the forced response of the same magnitude. So the contribution of this internal component to the heat uptake is $H_I = -\beta T_I$, and the total heat uptake is

$$H = -\beta T_I + \gamma T_F = -\beta(T - T_F) + \gamma T_F = F - \beta T \tag{16.3}$$

Substituting for β, the heat uptake as a function of ξ is

$$\frac{H}{T} = \frac{F}{T} - \beta = \frac{F}{T} - (\frac{F}{T}\frac{1}{\xi} - \gamma) = \gamma - \frac{1-\xi}{\xi}\frac{F}{T} \tag{16.4}$$

or, in a non-dimensional form,

$$\frac{H}{F} = \gamma\frac{T}{F} - \frac{1-\xi}{\xi} \tag{16.5}$$

The first term is the uptake per unit forcing computed as if the entire temperature change were forced – the second term is the correction needed if internal variability contributes. It is important that this second term is more or less inversely proportional to ξ; a bigger β (smaller climate sensitivity) is required to make room for the internal contribution, resulting in stronger radiative restoring of this internal component and greater heat loss.

A typical value for the first term, $\gamma T/F$, might be ≈ 0.3 as already discussed above. Using this estimate, the value of ξ needed to produce near zero heat uptake by the oceans is $\xi \approx 0.75$, so internal variability need only contribute about 25% of the total warming to fully compensate for the heat uptake due to the forced response. If internal variability contributes 50% of the warming, then the heat lost by the oceans would be more than twice as larger as the heat gain computed by the alternative model in which the internal variability contribution is small. This heat loss increases more and more rapidly as ξ is reduced further.

While the specifics of the calculations of heat uptake over the past half century continue to be refined, the sign of the heat uptake, averaged over this period, seems secure – I am not aware of any published estimates that show the oceanic heat content decreasing, on average, over these 50 years. Accepting that the the sign of the heat uptake is positive, one could eliminate the possibility of $\xi < 3/4$ — if one could justify using the same strength radiative restoring for the forced and internal components.

But this little derivation cannot be taken at face value when ξ is large. If one accepts that the forced response dominates, one can consistently free up the horizontal structure of the internal component, potentially producing a dramatically different, and possibly much weaker, radiative restoring for the internal component– and allowing ξ to be reduced more than indicated by this calculation before the heat uptake changes sign.

I have recently looked at 1,000 years of a control run of CM2.1 (with no time-varying forcing agents) and located the 50 year period with the largest global mean warming trend at the surface, which turns out to be roughly $0.5K/50yrs$. This warming is strongly centered on the subpolar Northern oceans, diffusing over the continents, but with little resemblance to the observed long-term warming pattern. (We don't have a lot of confidence in the model's simulation of these low frequency variations, but you can argue on very general grounds that these low frequency structures should emanate from the subpolar oceans. I'll try to return to this issue of the spatial structure of low frequency internal variability in another post.) CO_2, due in large part to positive feedback from polar ice and snow (and low clouds over the oceans) in the model. As discussed in post 5, it seems that the more polar concentrated the response the weaker the radiative restoring.

I am not aware of any study summarizing the strength of the global mean radiative restoring of low frequency variations in control simulations in the CMIP3/AR4 archive. It would be interesting to look at these if someone has not already done so. Supposing that we accept the model results for this radiative restoring of low frequency internal variability,what does this yield for the value of ξ at which the heat uptake changes sign?

Like many others, I am watching with great interest and, I hope, an open mind, as the heat storage estimates from ARGO and the constraints imposed on steric sea level rise by the combination of altimeter and gravity measurements slowly emerge. And I would like to understand the effects of internal variability on heat uptake a lot better. But I see no plausible way of arguing for a small- ξ picture. With a dominant internal component having the structure of the observed warming, and with radiative restoring strong enough to keep the forced component small, how can one keep the very strong radiative restoring from producing heat loss from the oceans totally inconsistent with any measures of changes in oceanic heat content?

17 Structure of Internal Low Frequency Variability in Models

[Originally posted September 15, 2011]

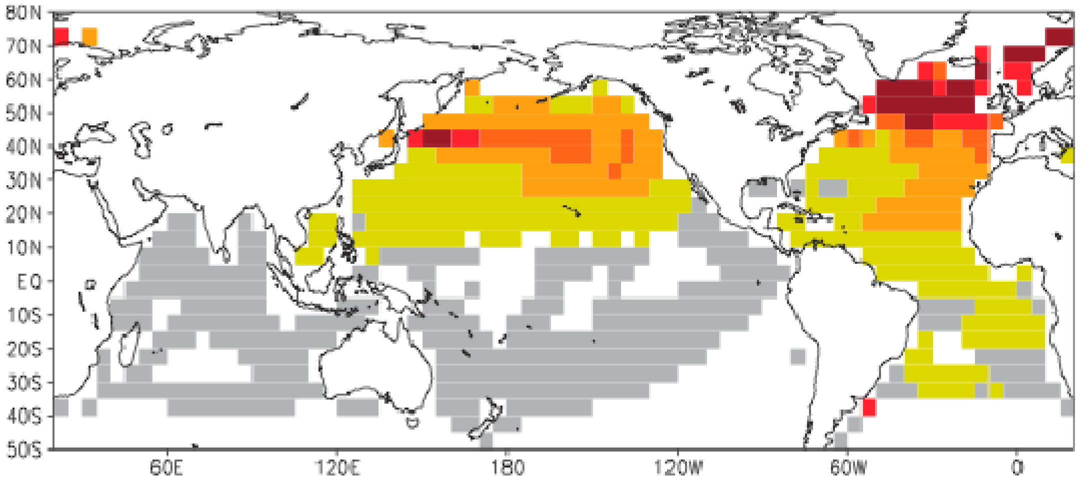
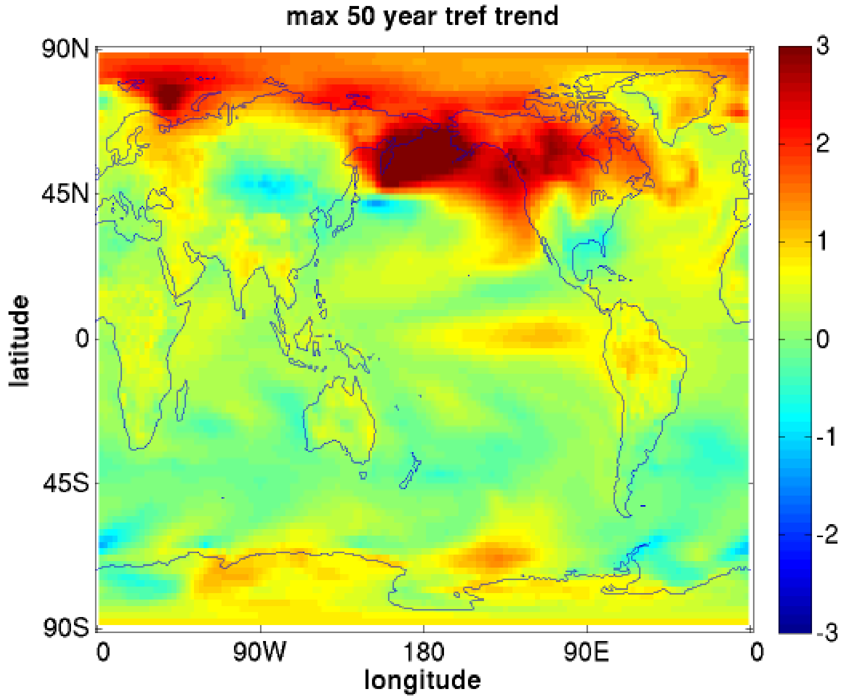


Figure 17.1: From DelSole et al 2011. The component of sea surface temperature variability that maximizes its integral time scale, obtained from the combination of 14 control runs of CMIP3 climate models.

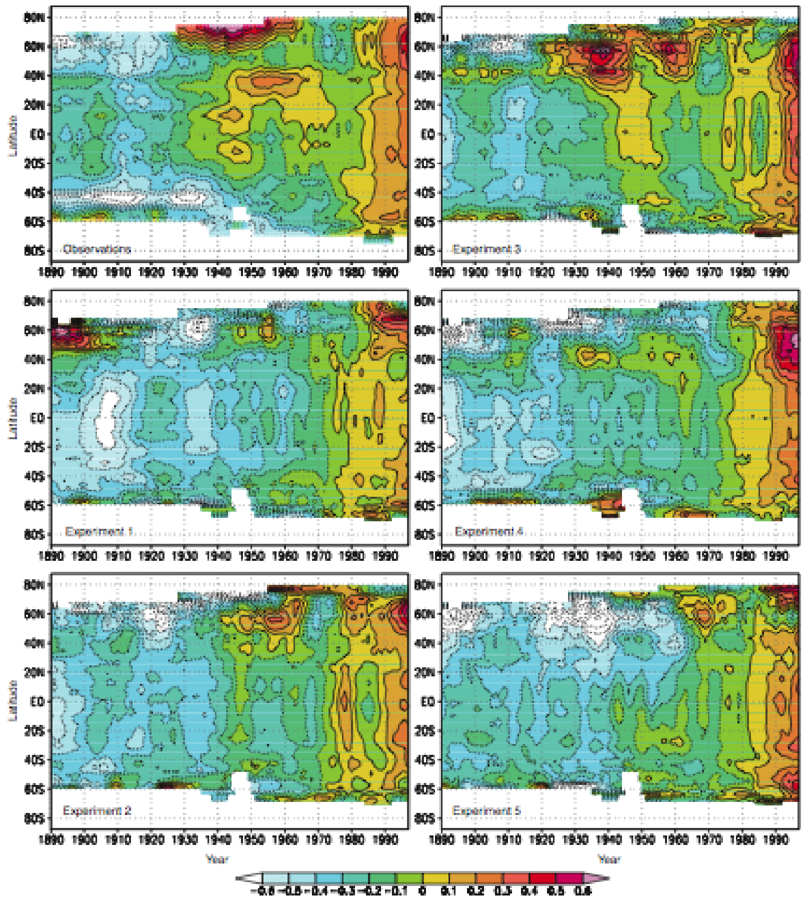
This is a continuation of the previous post in which I analyze the sources of my confidence that the warming trend of the past half-century is dominated by external forcing.

Taking a long control integration of CM2.1, a GCM that I have talked about here before, I've used the last 2,000 years from the simulation described by Wittenberg 2009, and located the period with the largest positive 50-year trend in global mean surface air temperature. The picture below is of the trend at each point, the global average of which is 0.41C. The average over the Northern Hemisphere only is about twice as large. I think we can agree that this looks nothing like the observed trends in the past half-



century. The maximum amplitude is in the subpolar North Pacific, with little trend in the tropics. Changes in the vertical mixing and transport between surface waters and deeper layers undoubtedly play a key role in the generation of this pattern. It is interesting that the North Atlantic does not play a more important role in this largest-trend case, since it does dominate the oceanic variability on somewhat shorter 20 year time scales in this model. (The northern Pacific is too active, due to a cold bias and excessive ice formation, resulting in too much communication with deeper oceanic layers.) But there is one aspect of this pattern that does not surprise me — that the center of action is in the subpolar oceans.

Here's another plot, making the same qualitative point, from an older GFDL model– from Delworth and Knutson, 2000: Shown as a function of latitude and time are averages over longitude of surface temperature from observations (here taken from Parker et al 1994) in the upper left and from 5 realizations of the climate model's simulation of the 20th century. The differences between realizations is the internal variability, and one can see quite a lot at high Northern latitudes in this model. In fact, one of the realizations (upper right) happens to capture considerable early century warming, peaking near mid-century in high northern latitudes, comparable to that observed. (This ability to capture early 20th century warming due



to internal variability remains rare in models, with this result a bit of an outlier –whether this is due to underestimation of variability by models or the presence of other forcings remains the obvious question.) In any case, internal variability is unable to compete with the more uniformly distributed warming trend throughout the tropics and midlatitudes of both hemispheres in the latter half of the century.

DelSole et al 2011 provide a convenient figure (top of post) summarizing the spatial structure of low frequency variability of sea surface temperature in an ensemble of GCMs. They gather the control simulations from 14 models together into one pot and decompose the variability into patterns, isolating that pattern whose time series has the largest integral time scale (or decorrelation time). This is not the only way to summarize this information, but it serves my limited purpose here of illustrating how low frequency variability tends to be concentrated in the subpolar oceans across the model ensemble. (They exclude the Southern Oceans from their analysis.)

Water columns are much more strongly stratified in the tropics than in higher latitudes, so it takes a lot less energy to move parcels from deep oceanic layers to the surface in high latitudes — and, not surprisingly, this is where most communication occurs between deep and surface waters. On the other hand, it seems quite logical that one needs to tap into the heat capacity of these deeper layers to create internal variability on long time scales. So one can rationalize the result that the centers of action for internal variability in the oceans migrate poleward from the tropics and subtropics to higher latitudes as one moves to lower frequencies. By the same physical argument, one expects minima in the response to external forcing in the subpolar oceans, since these are being held back by their strong coupling to the deeper layers.

We can all question these model results, of course. For example, could unresolved mesoscale eddies create more energetic multi-decadal variations in the wind driven gyres by cascading energy to larger scales? Also, temperature anomalies like those in the figure at the top do influence tropical rainfall patterns. In fact, they may do so more efficiently than more uniform temperature change; warming one hemisphere with respect to the other is an excellent way of pulling monsoonal circulations and oceanic ITCZs towards the warm hemisphere (the last few years have seen numerous studies of this response, relevant for ice ages and aerosol forcing as well as the response to high latitude internal variability; I'll try to return to this topic in a future post.) Could tropical cloud feedbacks, or the coupling to ENSO, amplify the effects of low latitude hydrological responses to high-latitude anomalies in these models?

In any case, it is good to have a list of what you have to question if , in particular, you want to argue that the warming in the past half-century has been dominated by internal variability. It is not enough to look at global or hemispheric means of surface temperature and note that the models are not that far from producing internal variability of the right magnitude — perhaps most existing models only do this once in a blue moon, but I can imagine increasing the variance at low frequencies by a factor of two, say, so that the required magnitude is achieved more frequently. But the spatial structure will be still be wrong. My intuition is that it will be harder to modify the structure than the amplitude of the variability.

If a model comes along with low frequency variability that is less polar concentrated and fits the century, or half-century, trend pattern better, that would be news. If it also has heat flowing into rather than out of the oceans during the growth of the warm phase of this mode, that would be even more dramatic news.

Can we analyze observations cleverly so as to separate forced from internal variations, with or without the use of models? If we had higher confidence in the evolution of the aerosol forcing over time it would be a lot easier. In light of the plausible structure of internal variability (and the relatively rich set of observations of the North Atlantic ocean) a focus on high Northern latitudes, asking if some of the observed trend in this region is internal, might be more productive that a focus on global means.

18 Noise, TOA Fluxes, and Climate Sensitivity

[Originally posted October 7, 2011]

Consider a simple energy balance model for the oceanic mixed layer (+ land + atmosphere) with temperature T and effective heat capacity c. The net downward energy flux at the top-of-atmosphere R is assumed to consist of a radiative relaxation, - βT, plus some noise,N. With imposed flux from the deep ocean to the mixed layer, S:

$$c\frac{dT}{dt} = R + S = -\beta T + N + S \tag{18.1}$$

The assumption is that there is no external radiative forcing due to volcanoes or increasing CO_2, etc. Can we use a simple one-box model like this to connect observations of interannual variability in R and T to climate sensitivity? This model is central to two papers by Spencer and Braswell (2008, 2011), hereafer (SB08, SB11).

This is a linear equation that one can break up into the sum of two terms $T \equiv T_N + T_S$, where

$$c\frac{dT_N}{dt} = -\beta T_N + N, \quad c\frac{dT_S}{dt} = -\beta T_S + S \tag{18.2}$$

The key assumption is that S and N, and, therefore, T_S and T_N are uncorrelated — or T_S and N for that matter. Since we are interested in estimating β from observations of R and T — a starting point might be regressing R against T, as in Forster and Gregory 2006. Defining

$$\beta^* \equiv -[RT]/[T^2] \tag{18.3}$$

where brackets are a time average, the difference between β^* and β is what we are interested in:

$$\beta^* = \beta - [TN]/[T^2] = \beta - [T_N N]/[T^2] = \beta(1 - [T_N^2]/[T^2]) = \beta[T_S^2]/[T^2]. \tag{18.4}$$

The equation for T_N has been used in setting $[T_N N]$ equal to $\beta[T_N^2]$. One could stop here, or rewrite the last expression as

$$\beta^* = \beta[S^2]/[S^2 + N^2]. \tag{18.5}$$

(used by Murphy and Forster 2010– in their critique of SB08, but there is actually no need to refer to S — and we don't need an evolution equation for T_S. SB11 use this same model, looking at observed phase lags between R and T to argue that noise-generated temperatures must be significant in the observations.

The equation for β^* holds if we filter all fields to emphasize certain frequency bands, so if one has some idea of the relative spectra of T_N and T_S, one could design the filter to reduce the effects of T_N.

The meaning of N and assumptions about its spectrum are the source of much of the confusion about this model, I think — at least it has been the source of my own confusion. In the simple model, N is the variability that you have at the TOA if T is fixed. You can try to estimate this fixed-T flux variance with a GCM. SB08 use 1.3 W/m2 for the standard deviation of monthly means of N, for a domain covering tropical oceans only, while a quick check of GFDL's AM2.1 model with fixed SSTs produces 1.0 W/m2 for the net radiation over the same domain, which does not raise any flags for me. (The model's global mean noise amplitude in monthly means is abut 0.6 W/m2.) In the GCM, this noise is essentially uncorrelated from month to month. In preparation for this post, I tried varying the parameters in the simple model over ranges that I thought were plausible, assuming that the decorrelation time for N is no longer than a month, and could not generate cases with large enough T_N to create significant ($> 10\%$) differences between β^* and β and phase lags in the right ballpark — if I push the parameters to create more noise in the temperatures (by reducing the heat capacity (depth) of the mixed layer, in particular) the TOA fluxes are too noisy. Returning to SB11, I noticed something that I missed the first time through, that they pass their "noise" through a 9-month top-hat smoother. If I do this and tune the noise variance, then things look more reasonable. But can "noise" with this spectrum be independent of T_S (ie ENSO)? I personally can't imagine how a model with fixed SSTs can produce TOA flux variations with this long a decorrelation time.

I think a more plausible picture is something like the following. The central and eastern tropical Pacific SSTs warm due to heat redistribution from below and relatively quickly warm the entire tropical troposphere; this stabilized atmosphere reduces convective cloudiness over the tropical Indian and Atlantic Oceans, reducing the reflection of the incident shortwave in particular, producing warming of these remote oceans that takes several months to build; the global or tropical mean temperature has a component that follows this remote oceanic response. So temperatures have a component forced by TOA flux anomalies, but these anomalies would not exist without the Pacific source of variability. This is a caricature of the tropical atmospheric bridge described more fully by Klein et al 1999 in particular.

A simple change in the box model that might capture a bit of this would be to set $N = N_0 + \alpha T_S$, where N_0 is real atmospheric noise with appropriately short decorrelation time, and α is a constant that relates the remote change in TOA flux to ENSO. (Ignoring the time it takes to set up this flux response may be an oversimplification.) I would also give T_S the spectrum of typical ENSO indices. This model generates phase lags without noise (N_0), but adding some noise might still be useful.

Changes in tropical circulation associated with ENSO warmings are quite different from the circulation responses we expect from increases in CO_2, and cloud feedbacks in particular are presumably sensitive to these circulation changes. From the perspective of a climate modeler, one thing that I would look for, as discussed in Essays 12 and 15, is if this co-variability of TOA fluxes and surface temperatures provides a metric that distinguishes between GCMs with different climate sensitivities. Actually, rather than equilibrium or transient climate sensitivity, I would look instead directly at the strength of the radiative restoring in transient warming runs — the canonical 1%/year CO_2 growth simulations being the simplest. The strength of radiative restoring changes in models as the system equilibrates (Essay 5), and equilibrium sensitivities are typically estimated by extrapolation in any case — while the transient climate response depends on ocean heat uptake processes that might play little role in interannual variability). If GCM simulations of this co-variability are not somehow correlated across an ensemble of models with the radiative restoring that occurs when CO_2 increases, this would itself be interesting.

19 Radiative-Convective Equilibrium

[Originally posted October 26, 2011]

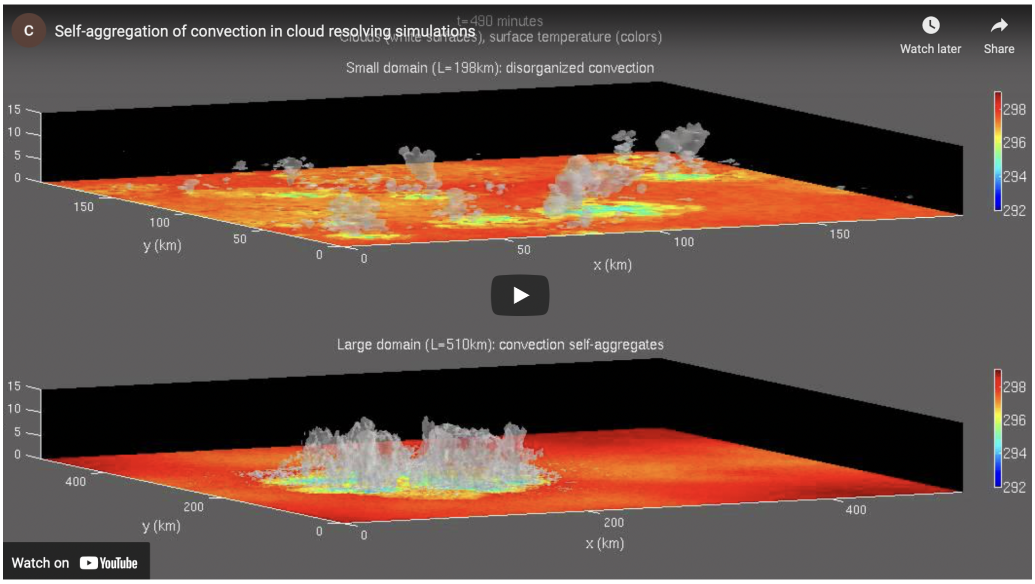


Figure 19.1:

Click here for animation.

The starting point for most of my thinking regarding climate sensitivity is the simple 1-dimensional radiative-convective model introduced in Manabe and Wetherald 1967. See also Manabe and Strickler 1964. For an early review of this kind of modeling, see Ramanathan and Coakley 1978. Sadly, Dick Wetherald passed shortly befroe this was written; although it is a very small gesture, I would like to dedicate this post to his memory.

This model solves for a single vertical temperature profile as a function of pressure p (or height z) which one can think of as representing the globally average temperature profile. There is no explicit atmospheric circulation, although it is present implicitly. The atmosphere is in hydrostatic balance:

$\partial p/\partial z = -\rho g$. The model consists conceptually of two parts. There is a radiative transfer module that generates the net shortwave and longwave radiative fluxes, the sum of which I will call $\mathcal{R}$. As input to this module one needs the incident solar flux, surface albedo, and the vertical profiles of temperature and whatever radiatively active constituents are assumed present. Secondly, there is a convective flux $\mathcal{D}$ that redistributes energy in the vertical:

$$\rho c_p \frac{\partial T}{\partial t} = -\frac{\partial \mathcal{R}}{\partial z} - \frac{\partial \mathcal{D}}{\partial z} \tag{19.1}$$

The surface temperature is determined by the net surface radiation and the convective flux at the surface:

$$C_S \frac{\partial T_S}{\partial t} = -\mathcal{R}(0) - \mathcal{D}(0) \tag{19.2}$$

Here c_p is the heat capacity at fixed pressure, ρ is the density, and $\mathcal{R}$ and $\mathcal{D}$ are positive upwards. I have also given the surface some heat capacity — this value is not important for the steady state.

If you integrate such a model to equilibrium without any convective redistribution of energy — ie if you compute pure radiative equilibrium — the result will be strongly gravitationally unstable near the surface. A minimal model of convection might redistribute energy vertically whenever the lapse rate, $-\partial T/\partial z$, increases beyond the dry adiabatic value for an ideal gas, $g/c_p \approx 9.8K/km$ so as to bring the lapse rate back to this critical value. The mixing is assumed to be very strong once the critical lapse rate is reached — the assumption is that the time scales of the convective mixing are much smaller than those of the radiative relaxation towards equilibrium. You can formulate a mixing process that achieves this if you diffuse c_pT+gz, the dry static energy, and turn on the diffusion only when the lapse rate exceeds this critical value. One also needs to model the surface convective flux. The simplest possible model is just to assume that the surface flux is zero except when it is needed to keep the surface from getting warmer than the surface air $T(0)$ — ie, ignoring the air-surface temperature difference.

MW don't actually adjust to the dry adiabatic value. If one does that, the tropopause is too low and the tropospheric lapse rate too large. The observed globally averaged lapse rate is about $6.5K/km$. MW simply use this observed value for the critical lapse rate, which results in a reasonable tropopause height. Theories for the observed tropospheric lapse are not easily incorporated in this globally averaged framework, since different mechanisms stabilizing the troposphere are at work in the tropics and in higher latitudes. (In low latitudes, it is more natural to talk about a moist rather than the dry adiabatic lapse rate; in higher latitudes, the large-scale quasi-horizontal turbulence that produces highs and lows and weather competes with smaller scale moist convection in transporting energy upwards.)

In addition, MW do not bother with an explicit diffusive model for the convective transport. Instead they use a simple convective adjustment — while integrating towards equilibrium with a vertically finite-differenced model, check at every time step to see if the flow is unstable according to the prescribed critical lapse rate — if any two layers are unstable set the lapse rate between these two layers equal to the critical value while conserving the mean energy (here this is just the mean temperature) of the two layers. Do this also at the surface to prevent the first atmospheric layer immediately above the surface from being colder than the surface.

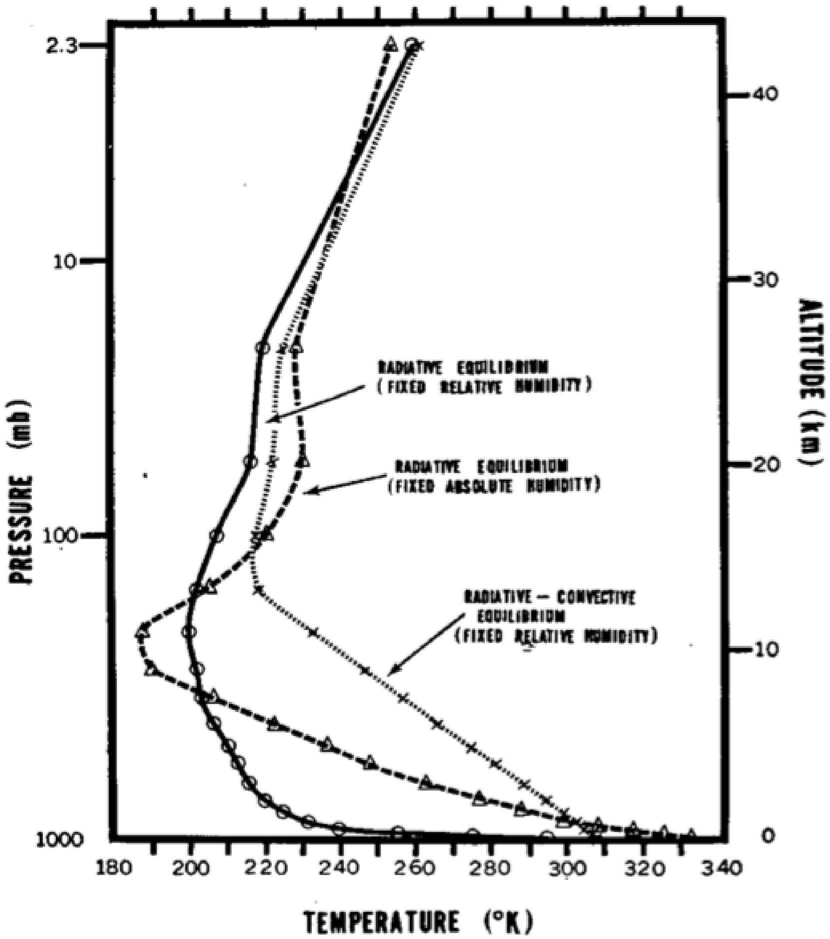


Figure 19.2: A figure from Manabe and Wetherald 1967

The tropopause height is part of the solution. The result for realistic settings is just a troposphere at the critical lapse rate merging continuously at the tropopause into a stratosphere in radiative equilibrium. If you know that this is what the equilibrium looks like, you can get the equilibrium solution by a simpler iteration. For a given surface temperature and tropopause height, the tropospheric temperatures are known. Given these temperatures you can compute radiative equilibrium above this tropopause. The solution will have two problems: the temperature will not be continuous at the tropopause, and the energy flux at the top of the atmosphere will not be zero. These two constraints can then be used to determined the two unknowns — the surface temperature and the tropopause height.

You can do more elaborate things with the surface fluxes and try to simulate the effective air-surface temperature difference, especially if you want to divide the surface convective flux into its two components, evaporation and sensible heat, but this extension doesn't change the model's climate sensitivity appreciably.

MW compare the assumption of fixing the relative humidity distribution in the troposphere to that of fixing specific humidities, providing the first modern estimates of the difference this makes for climate sensitivity. Stratospheric water is specified as is the ozone distribution. Clouds must also be prescribed in this model. Increasing the CO_2 the surface and troposphere warm by the same amount, by construction, while the stratosphere cools and the tropopause rises, as described in MW. Is this very strong coupling of the troposphere to the surface realistic? I think it is a very good place to start, but my purpose in this post is not to convince you of that but just to convey what this radiative-convective model is.

The strong coupling requires one to think about the energy balance of the surface + troposphere rather than the surface in isolation. Suppose one puts a layer into the troposphere that absorbs some of the solar radiation without increasing the reflection. From a surface energy balance perspective one might guess that this would cool the surface, since less solar radiation would penetrate to the ground. But from the perspective of a strongly coupled surface-troposphere system, whether one absorbs at the surface or in the interior of the troposphere is irrelevant for the temperature response to first order — in fact this absorption would cause warming to the extent that it prevents the scattering to space that would otherwise occur (you maximize this effect by putting the absorber over ice or a low cloud deck.) It is interesting to ask how strong the absorption in the troposphere must be to decrease the convective mixing to the point that the surface decouples from the troposphere. We might call this the "nuclear winter" problem.

In the past one or two decades, there has been an increasing amount of work on radiative-convective models with explicit moist convection. Take your numerical model of the atmosphere and place it over a flat homogeneous surface, ignore rotation, and assume that the geometry is re-entrant in both horizontal dimensions. There are no walls and every point in the horizontal is physically identical to every other point. Assume that the surface is saturated — ie ocean. Turn on the radiative transfer and start destabilizing the atmosphere, evaporating water and generating cumulus convection. Its the same idea as the single column model, but now the model is determining its own clouds and water vapor distribution as well as temperature profile. (Typically one still fixes ozone and stratospheric water). The upper part of the animation above, kindly provided by Caroline Muller, has horizontal resolution of 2km and a square 200 x 200 km domain. This is a statistically steady state achieved after a couple of months of integration. See Tompkins and Craig 1998 to read more about this kind of simulation. Romps 2011 is a recent attempt to push to much higher horizontal resolution, to better resolve the key patterns of entrainment and detrainment into and out of the turbulent convective plumes. (It is important to keep in mind that We cannot test these models in this homogeneous configuration — you naturally have to simulate the conditions in particular regions in which there have been field programs that provide appropriate data.)

The temperature profiles these models produce in radiative-convective equilibrium are qualitatively similar to those generated by single column convective adjustment models, with the moist adiabat determining the critical lapse rate. The surface and troposphere are very strongly coupled in these simulations. I'll discuss the changes in water vapor and clouds that they simulate in response to increase in CO_2 in future posts.

The lower panel in the animation at the top of the page is strikingly different from the upper panel, yet it is generated by simply increasing the size of the domain to 512 x 512 km. The convection now aggregates into a small fraction of the domain. See Bretherton et al 2005 for a discussion of this behavior. Caroline and I are currently re-examining theories of this self-aggregation in homogeneous models. The model has hysteresis for some parameter settings, so its climate is not always unique. I find this sort of thing challenging but frustrating as well. We saw something like this in an early low resolution 2-dimensional (x-z) study (Held et al 1993), but I was hoping that the 3D case would be free of this kind of complexity, so that we could more easily use it as a stepping stone towards understanding more realistic models. Is self-aggregation in the statistically-steady homogeneously-forced non-rotating model a curiosity, or is it telling us something important?

20 The Moist Adiabat and Tropical Warming

[Originally posted Dcember 7, 2011]

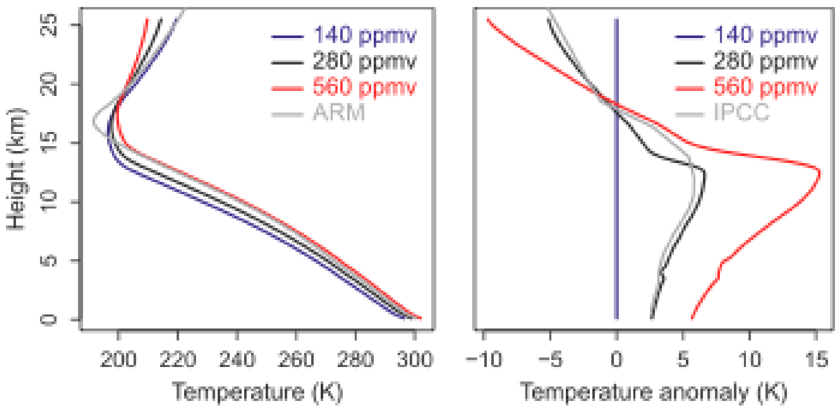


Figure 20.1: Results from a high resolution model of horizontally homogeneous radiative-convective equilibrium, Romps 2011. Left: equilibrium temperature profiles for 3 values of CO_2 compared to an observed tropical profile. Right: the temperature differences compared to the response to doubling CO_2 in an ensemble of CMIP3 global climate models.

As a moist parcel of air ascends it cools as it expands and does work against the rest of the atmosphere. If this were the only thing going on, the temperature of the parcel would decrease at 9.8K/km. But once the water vapor in the parcel reaches saturation some of this vapor condenses and releases its latent heat, compensating for some of the cooling (you get about 45K of warming from latent heat release when a typical parcel rises from the tropical surface to the upper troposphere). A warmer parcel contains more water vapor when it becomes saturated, so it condenses more vapor as it rises, and temperature decreases with height more slowly. That is, the moist adiabatic lapse rate, $-\partial T/\partial z$, decreases with warming.

To say something about the warming of the tropical atmosphere, rather than that of a moist adiabat, we need to argue that the tropical troposphere is close to a moist adiabat and remains close as it warms. The upper troposphere will then warm more than the lower troposphere. This is precisely what happens in our global climate models. The consistency or inconsistency of this prediction with observations, particularly the Microwave Sounding Unit (MSU) temperatures, is a long-standing and important issue A failure of the upper troposphere to warm as much as anticpated by this simple argument would signal a destabilization of the tropics — rising parcels would experience a larger density difference with their environment, creating more intense vertical accelerations — affecting all tropical phenomena involving deep convection. I like to refer to warming following the moist adiabat as the most "conservative" possible — having the least impact on tropical meteorology.

One sometimes sees the argument that a consequence of smaller upper tropospheric warming in the tropics would be lower climate sensitivity, since a large fraction of water vapor feedback originates in this region, and the large vapor increase could not occur without the temperature increase. But this is not the case, because of the cancellation between negative lapse rate and positive water vapor feedbacks produced by upper tropospheric warming. In fact, the negative lapse rate feedback is generally the larger of the two, so a weaker upper level tropical warming would probably increase climate sensitivity a bit, holding everything else fixed. [The water vapor feedback that I am referring to here is the part associated with the lapse rate change, if relative humidity is fixed, after subtracting off the part due to uniform warming of the troposphere]

The tropical atmosphere, and models of moist radiative-convective equilibrium, are dominated by concentrated saturated updrafts taking up a small fraction of the total area, with the rest of the flow experiencing very slow compensating subsidence. The behavior of such a skewed flow field can be counterintuitive. The picture that most of us have, I think, is that within the convective updrafts themselves the temperature profile takes its moist adiabatic value; this profile is then communicated efficiently to the rest of the tropics, since the atmosphere is unable to maintain substantial horizontal temperature gradients within the tropics. Horizontal gradients in pressure and temperature, above the boundary layer, are flattened by wave propagation rather than by mixing, a fundamentally different process than the homogenization of entropy in a dry convecting layer.

I remain somewhat confused as to how best to translate this picture into a scaling argument for how hard one has to push the tropical atmosphere to create a given departure from the moist adiabat. Arguments of the type summarized in Emanuel, Neelin, and Bretherton, 1994 suggest that attempts to alter the free tropospheric temperature profile will modify temperatures by a rather indirect path — heating perturbations will modify the circulation in a way that then modifies the temperature and humidity of the air near the surface, which finally puts you on a different moist adiabat.

It doesn't take much of a departure form an adiabat to be dynamically significant. A 3K temperature difference δT between parcel and environment averaged over a height $H = 10km$ naively produces an acceleration of $g\delta T/T \approx 0.1ms^{-2}$ and velocities at the top of the convective layer of

$$\sqrt{2gH\delta T/T} \approx 45ms^{-1} \tag{20.1}$$

which is larger than vertical motions observed in tropical convection. But it is not that easy to relate vertical motions quantitatively to departures from an adiabat in the tropics. There are subtleties in the definition of the moist adiabat itself associated with what happens to the condensate — temperatures are slightly different if the condensate is retained by the parcel, in which case its heat capacity must also be taken into account, or if the condensate falls out immediately, in which case we refer to the "pseudo-adiabatic" lapse rate. Real parcels are somewhere between these two extremes. You also needs to worry about the latent heat of fusion when ice forms, the presence of supercooled water making it tricky to predict when this transition to ice occurs. In addition, when computing the density difference between a rising parcel and its environment, and the associated vertical accelerations, you must account for the "condensate loading" — the pressures associated with the suspension of the condensate within the rising parcel. Finally, if a parcel entrains some dry environmental air as it rises, it has less latent heat to release per unit mass, and its temperatures will fall faster with height than the temperature profile generated by an undilute parcel.

Several of these effects can be of the order of a degree or two — they are big enough to matter when trying to estimate the magnitude of the departure of the tropical atmosphere from a moist adiabat — the CAPE (Convective Available Potential Energy) of the tropics (see, for example, Xu and Emanuel 1989 and Williams and Renno 1993. But none of them are large enough to alter the expectation that the tropical atmosphere will roughly follow a moist adiabat as it warms. One of these effects would have to change by an O(1) amount (doubling or halving its amplitude) in response to a 2K warming, say, to have a substantial effect on the sensitivity of lapse rates to warming, but why should that happen?

This conclusion is confirmed by high resolution models of horizontally homogeneous radiative-convective equilibrium, every one of which, to my knowledge, predicts a warming profile that is more or less moist adiabatic. The figure at the top is from Romps 2011, mentioned in the last post as well, which has 200m horizontal resolution. The figure on the left shows the model's equilibrated temperature profiles at three values of CO2 along with an observed tropical profile, while the panel on the right shows the changes in temperature in this model along with an average over CMIP3 global models (with grid sizes roughly 100x100 times larger). The profile of temperature change is essentially identical. There is actually a rather large fractional increase in CAPE and increase in the magnitude of vertical motions as the climate warms in these simulations, but this increase is nowhere near large enough to compensate for the upper level maximum in warming.

(The fact that the overall amplitude of the warming for doubled CO2 is nearly identical in the ensemble mean of GCMs and in this cloud resolving model is a coincidence — it is the vertical profile of the temperature change that I am focusing on here. The variety of results on sensitivity with cloud resolving models of radiative-convective equilibrium is as large as that obtained with GCMs, due primarily to differing cloud feedbacks associated with differences in organization of convection. These high resolution simulations are not necessarily more relevant to nature than GCMs, due to the idealized geometry and small domain, absence of rotation etc. The value of these dynamic radiative convective equilibrium models, even in these small idealized domains, is in testing our undersatanding of moist convection.)

The change in CO_2 itself has very little to do with this moist adiabatic response; you get essentially the same temperature response if you just just prescribe and then warm the surface temperature. Fig. 20.2, for example, is an early attempt at a dynamic radiative-convective model, from Held et al 1993, for a 5K surface warming with fixed CO_2 (the solid lines are moist adiabats): A dramatic change in convective organization can change the relevant moist adiabat constraining tropical temperatures. The self-aggregation transition described in the previous post, is a good example. The aggregated state is warmer by several degrees averaged over the troposphere than the state with more homogeneous convection, because the near surface relative humidity is higher in the region in which the convection is occurring. (Thanks to Caroline Muller for confirming this for me.)

As one moves upwards and convection peters out, there is presumably some potential to change local temperatures with local perturbations in ozone or aerosols, perhaps above 12 km or so. But below that, if you are trying to change the relationship between surface and tropospheric warm-

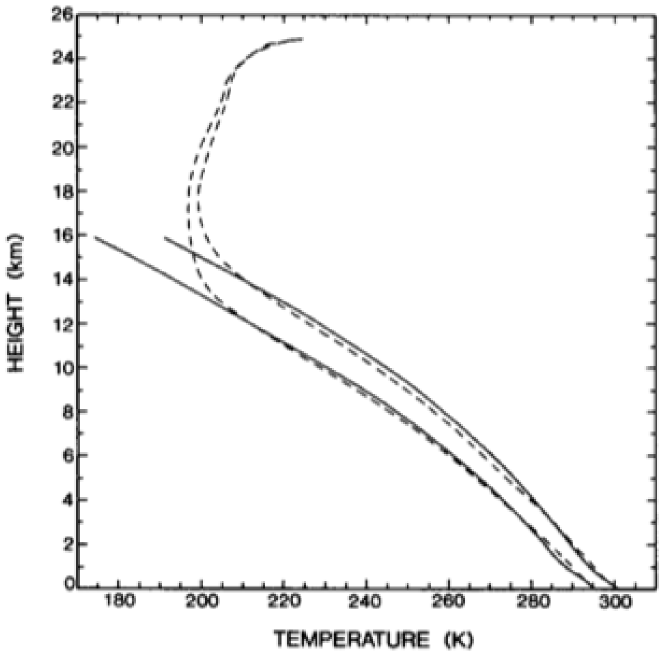


Figure 20.2:

ing, it seems that one is better off trying to change the relevant moist adiabat, by changing low level humidities or temperatures in the convecting regions, rather than creating huge departures from a moist adiabatic profile.

In addition to the analyses of MSU temperatures (which I won't try to summarize here) , there is other relevlant observational work that we need to focus on. One is the study of Allen and Sherwood 2008, using thermal wind balance to relate trends in the vertical gradient of the zonal winds to trends in the north-south temperature gradient The thermal wind equation is very accurate for zonal mean winds throughout the atmosphere, a simple consequence of the assumptions that the mean state of the atmosphere is in hydrostatic balance and that winds are in geostrophic balance. One can use an even more accurate relation, gradient wind balance, but given the large uncertainties in atmospheric warming trends, thermal wind balance is certainly accurate enough. It would be nice to see more attention on this use of wind trends, since these are totally independent of the temperature measurements, satellite or radiosonde.

Another study deserving attention is Johnson and Xie 2010 which argues that one can look at the distribution of deep convection in the tropics and rule out a trend towards overall destabilization. The temperature (and associated water vapor content) of near surface air has to reach a certain threshold for this air to rise close to the tropopause — currently 26-28C. If the atmosphere follows a moist adiabat as it warms, this critical temperature will increase along with the surface warming. If the upper troposphere does not keep up, this critical temperature would not increase as fast as the surface temperature itself, favoring more widespread convection — which, according to the paper, is not observed.

21 Temperature Trends: MSU vs. an Atmospheric Model

[Originally posted January 1, 2012]

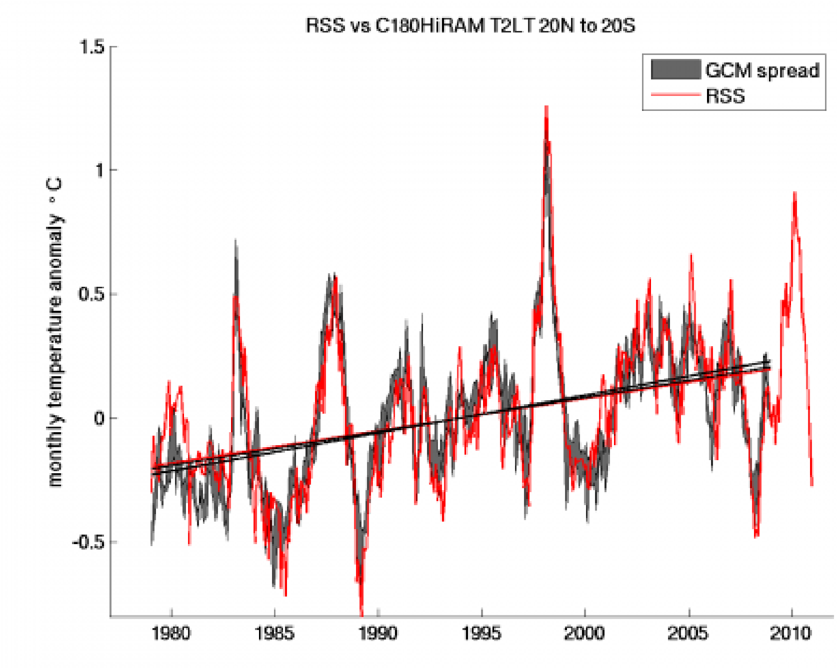


Figure 21.1: Lower tropospheric MSU monthly mean anomalies, averaged over 20S to 20N, as estimated by Remote Sensing Systems – RSS (red) and the corresponding result from three realizations of the GFDL HiRAMC180 model (black) using HadISST1 ocean temperatures and sea ice coverage. Linear trends also shown. Details below.

Motivated by Essay 20 and Fu et al 2011 I decided to look in a bit more detail at the vertical structure of the tropical temperature trends in a model that I have been studying and how they compare to the trends in the MSU/AMSU data. The model is an atmosphere/land model using as boundary condition the time-evolving sea surface temperatures and sea ice coverage from HadISST1. It is identical to the model that generates the tropical cyclones discussed in Essay 2. It has the relatively high horizontal resolution, for global climate models, of about 50km. Three realizations of this model, starting with different initial conditions, for the period covering 1979-2008, have been provided to the CMIP5 database, and it is these three runs that I will use in this discussion. The model also has prescribed time-evolving well-mixed greenhouse gases, aerosols (including stratospheric volcanic aerosols), solar cycle, and ozone. The atmospheric and land states are otherwise predicted.

The MSU data, as gridded monthly mean anomalies, were downloaded from RSS. The weights for the channels referred to here are included in a figure at the bottom of this post — thanks to Qiang Fu and Celeste Johanson for help in this regard. All of the model results are monthly mean anomalies from the model's seasonal cycle defined as the time mean for each month over the 30 year period Jan 1979- Dec 2008. Observations are plotted as anomalies from a time average over the same period. And all model and observed linear trends are computed over the same time interval as well (unless otherwise stated). I'll only discuss averages over the deep tropics from 20S to 20N.

Analyzing an atmosphere/land model running over prescribed oceanic boundary conditions has advantages and disadvantages as compared to analyzing a model fully coupled with the ocean. The advantage is that one avoids conflating disagreements between model and observations regarding the variation in sea surface temperature (SST), on the one hand, with problems that the atmospheric model may have in coupling SST variations to the troposphere and land surface, on the other. And one can compare in much more detail the time evolution of quantities of interest — even if one's coupled model is perfect, its El-Ninos will resemble reality only in their statistics.

The disadvantage is that one might be doing some damage to the atmosphere by disallowing two-way interactions with the oceans. The significance of this distortion is very much problem specific and can be subtle. For example, tropical cyclone intensity is presumably affected by running over prescribed SSTs, by not allowing the oceanic mixing generated by the storm to affect its intensity. If tropical cyclone intensity, in turn, affects the tropical lapse rate trends this would be a problem. I don't, at present, see this or other related possibilities as significant for this lapse rate issue, but its something to be alert for.

Let's start with the lower tropospheric channel referred to as T2LT or TLT. The red line in the figure at the top is the RSS MSU time series, while the shading spans the results from the 3 model realizations. (The smallness of this spread shows how tightly the tropical lower troposphere is coupled to the ocean surface in the model. This spread would be much larger in a fully coupled model). Trend lines are shown for both the observations and the three model runs (it is hard to see the 3 distinct model trend lines because of overlap). I get 0.130 C/decade for the RSS trend and 0.148 for the mean of the 3 model runs — with 0.154, 0.137, and 0.152 for the individual runs). If I drop the first two years, 1979 and 1980, the mean model trend drops to 0.143 and the RSS T2LT rises to 0.149. (It might be a consequence of how I plotted this, but this early period does seems to be a major source of the discrepancy. You can think of this as cherry-picking or as a very crude way of judging whether this difference is plausibly significant.)

Moving on to the deeper tropospheric average provided by T2 (also referred to as TMT), we get a very similar looking plot: The model trends

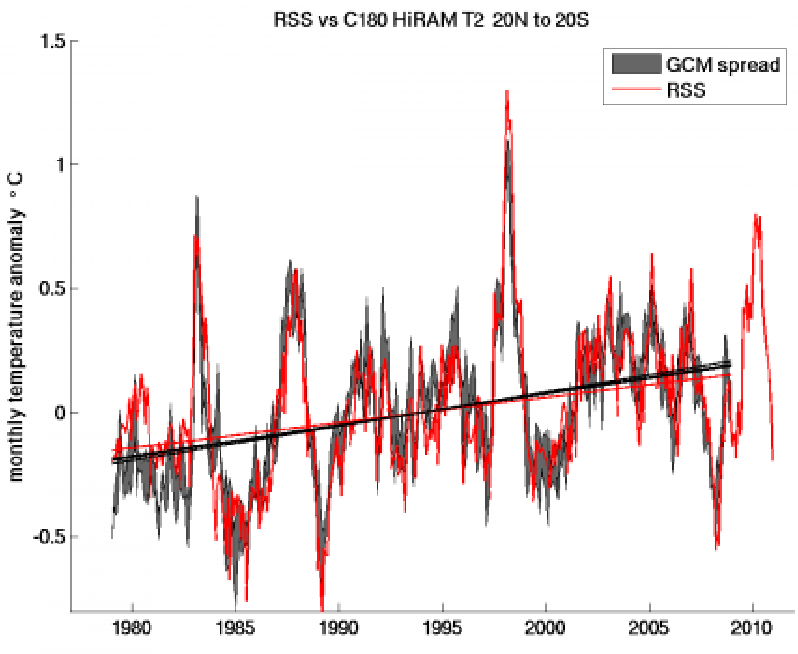


Figure 21.2:

are now (0.138, 0.125, 0.129) with a mean of 0.131, with the RSS trend over this period is 0.102. These trend are smaller than the T2LT trends, in both the model and the observations, despite the fact that T2 weights the lower troposphere less strongly that T2LT. The model trends actually increase with height through the troposphere. The problem, long appreciated, is that T2 has significant weight in the stratosphere, where there is a cooling trend in both model and observations as indicated here by the T4 time series: The warming due to absorption by El Chichon and Pinatubo aerosols is

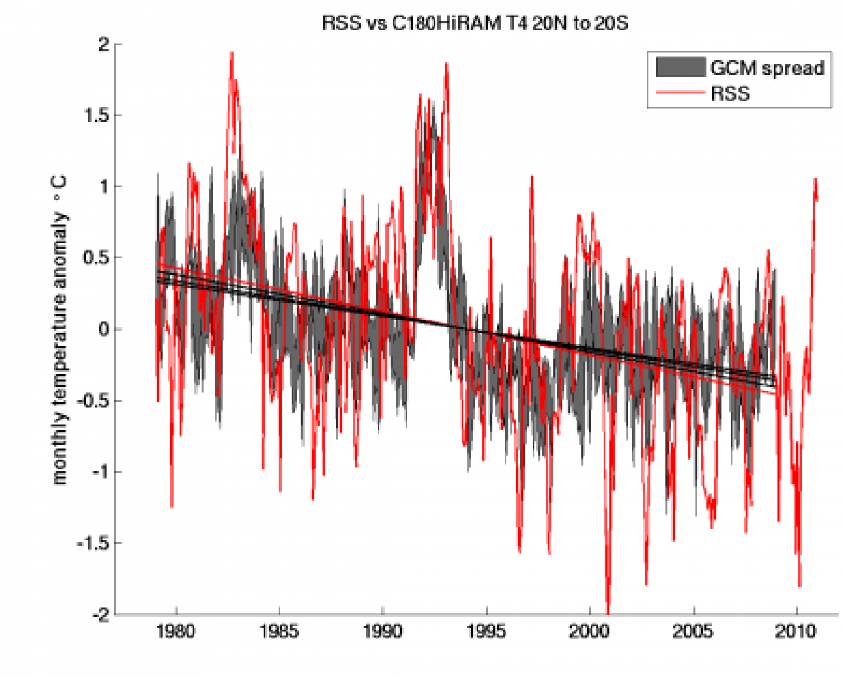


Figure 21.3:

superposed on an uneven cooling trend. The El Chichon signal is relatively weak in the model, contributing to the underestimate of the cooling trend. (The model is also missing substantial internal variability — it does not simulate a realistic Quasi-Biennial Oscillation, but his does not appear to be the dominant signal in this missing variability). Here I follow Fu et al 2011 and use T24 = 1.1*T2- 0.1*T2LT (oops — I meant T24 = 1.1*T2- 0.1*T4; June 8, 2012) to reduce the influence of the stratosphere on T2. A plot of T24 would look a lot like the that for T2 above, but the mean model trend is increased to 0.168, while the RSS T24 trend is 0.143. The model-0bs difference here is smaller than for T2 itself because the model's cooling trend in T4 is smaller than that observed.

The actual model trends as a function of height are shown in Fig. 21.4 below, along with the trends using the T2LT and T24 weighting functions. To try to capture the model's upper tropospheric warming better, I have defined T2UT = 2*T24 – T2LT to get something that follows the upper troposphere a little more closely (see the weights at the bottom of the post — you want to avoid negative weights while keeping the integral of the weights unchanged). (I have arbitrarily plotted the satellite channel trends at the pressure levels at which model versions agree with the model trend: T2LT = 700mb, T24 = 550mb, T2UT = 450mb.) The red dots are the RSS values. Also shown at 1000mb in magenta is the trend in SST and, by the three black dots, the land+ocean mean surface trends in the 3 realizations — all over the 20N-20S region. There is substantial spread in the land

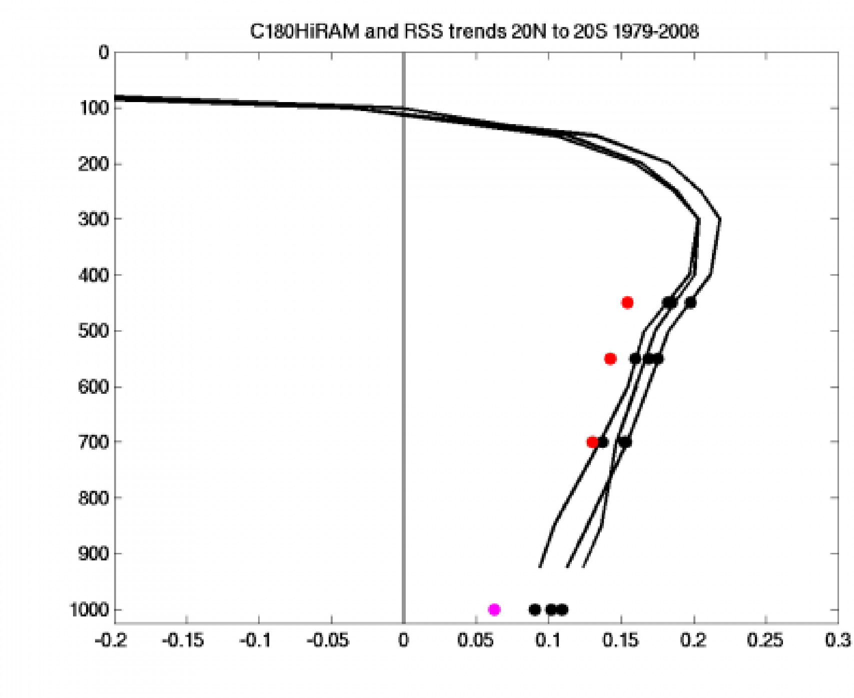


Figure 21.4:

warming, associated (I think) mostly with rainfall variability in semi-arid regions — I doubt that the effects of this variability propagate upwards beyond 700mb or so.

I also generated the same figure after dropping the first two years from the analysis, with the result in Fig. 21.5: The difference between these two plots is not small. A bias of the sort seen in the first plot, with the tropics evidently being destabilized as compared to the model, would have substantial consequences for tropical meteorology if extrapolated into the future.

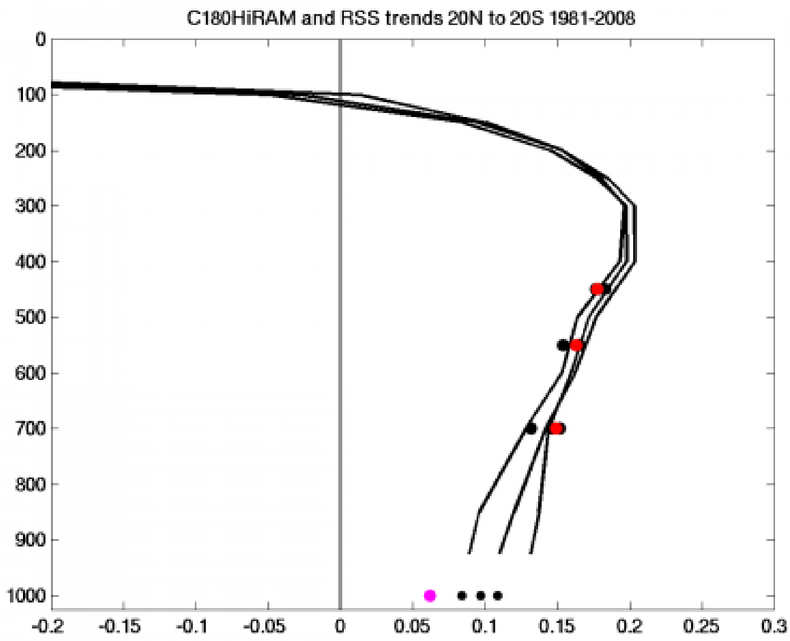


Figure 21.5:

Let ξ be the ratio of the trend in T2UT to the trend in T2LT (the ratio of T24 to T2LT, discussed in Fu et al is just $1 + (\xi - 1)/2$). The three model realizations give $\xi = (1.29, 1.33, 1.22)$ compared to 1.19 for RSS. If, once again, we repeat the calculation omitting the first two years, we get $\xi = (1.25, 1.34, 1.18)$ for the model and 1.19 (once again) for RSS. There is a hint that this ratio is more robust to the period considered than the trends themselves.

This is just one model and one observational analysis. (See Thorne et al 2011 for a recent discussion of differences between alternative analysis of the MSU data, and inconsistencies between radiosondes and MSU.) Accepting this comparison at face value, it is still not clear to me if there is a significant model bias or not, when the SSTs are specified. The differences seem subtle, but small differences in lapse rate can have important effects on tropical meteorology.

I would like to encourage more analysis of these prescribed SST ("AMIP") simulations in this context. Most of the recent model-data comparisons of tropospheric lapse rate trends focus on coupled models. Especially in the tropics, biases in forcing or climate sensitivity make themselves felt to a large extent through the SSTs. Superposing the bias due to SST differences on any biases due to the internal atmospheric dynamics controlling tropical lapse rates can be confusing. Normalizing tropospheric trends by surface ocean trends can help in this regard, but this assumes that the tropical mean SST is the only thing that matters, which need not be the case.

It is also nice to be able to focus on specific time periods in a way that would not be possible in free-running coupled models generating their own ENSOs, the detailed time histories providing potential insights into the data sets as well as the models. Are the early years in this record (1979-1980 roughly) also the source of model-data disagreement when other AMIP models are examined? Can we determine whether these differences are due to problems in the MSU data, the SST input into the models, or model biases?

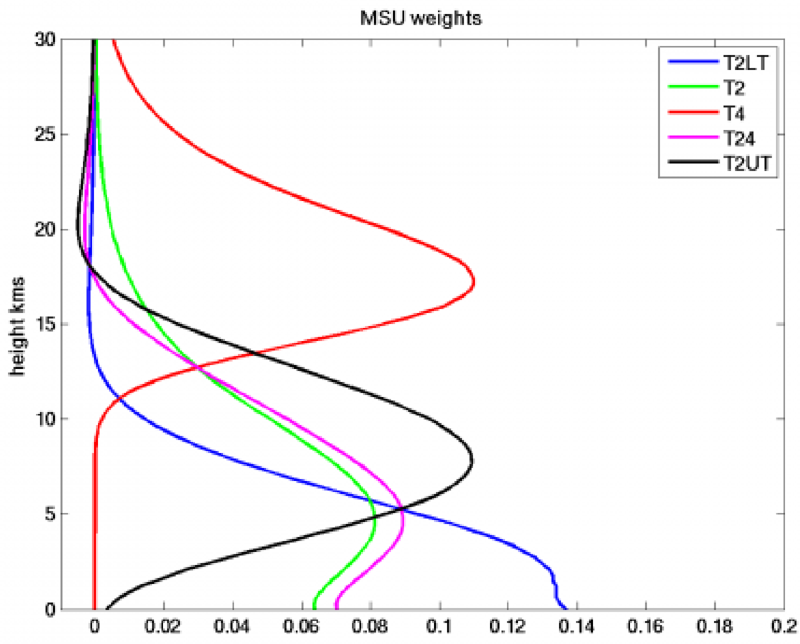


Figure 21.6:

22 Ultra-fast Responses

[Originally posted January 21 2012]

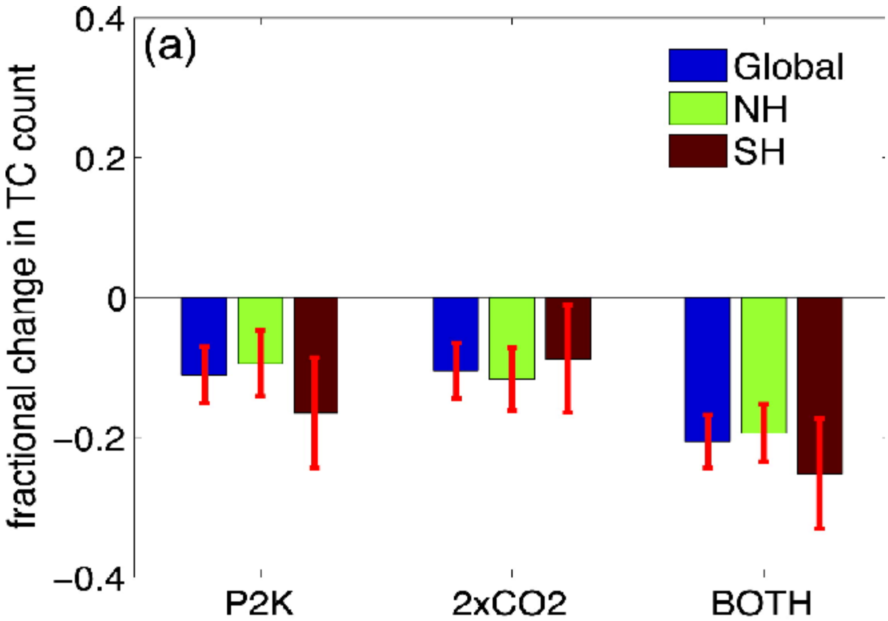


Figure 22.1: From Held and Zhao 2011, a simulation with an atmospheric model of the change in the number of tropical cyclones that form over each hemisphere and over the globe when sea surface temperatures (SSTs) are raised uniformly by 2C (labelled P2K), when the CO2 is doubled with fixed SSTs, and when SSTs and CO2 are increased together.

Suppose that we have a model of the climatic response to gradually increasing CO_2, and we examine the globally-averaged incoming top-of-atmosphere flux, N, as a function of time (using a large ensemble of runs of the model to average out internal variability). Letting δ refer to the difference between two climate states, for example the difference between the climates of 2100 and 2000 in a particular model, we end up looking at an expression like

$$\delta N \approx \frac{\partial N}{\partial C}\delta C + \frac{\partial N}{\partial T}\delta T + \frac{\partial N}{\partial X}\delta X \tag{22.1}$$

where T is the global mean surface temperature and X refers to all of the other things on which N depends. Here C is the CO_2 concentration, or, to the extend the useful range of this linearization, $log(CO_2)$. The forcing F can be defined as

$$F \equiv \frac{\partial N}{\partial C}\delta C. \tag{22.2}$$

We typically go a step further and write

$$\delta X = \frac{\delta X}{\delta T}\delta T \tag{22.3}$$

so that we can think of this last term as a feedback, modifying the radiative restoring strength,

$$\beta = -\frac{\partial N}{\partial T} - \frac{\partial N}{\partial X}\frac{\delta X}{\delta T} \tag{22.4}$$

i.e, so that

$$\delta N = F - \beta\delta T \tag{22.5}$$

While this is a formal manipulation that you can always perform if you want to, it is obviously more useful when δX is actually more or less proportional to δT. Ideally, there is a causal chain: $\delta C => \delta T => \delta X$. But what if the change in X due to an increase in CO_2 results from some other causal chain that doesn't pass through the warming of the surface (the warming of the strongly coupled surface-troposphere system)?

The classic example is the cooling of the stratosphere due to increasing CO_2. This cooling has no direct connection to the surface/tropospheric warming. If there were a strong negative cloud feedback, say, that prevented the surface/troposphere from warming, the stratospheric cooling would be hardly affected. But this stratospheric cooling does has a substantial effect on N, the energy balance at the top of the atmosphere — let's call it

$$(\partial N/\partial S)\delta S \tag{22.6}$$

where S is a measure of the stratospheric temperature response. The standard procedure is to include this term in the forcing, so that we retain an expression of the form

$$\delta N = \tilde{F} - \beta\delta T, \tag{22.7}$$

with

$$\tilde{F} = (\frac{\partial N}{\partial C} + \frac{\partial N}{\partial S}\frac{\delta S}{\delta C})\delta C \tag{22.8}$$

This has the advantage of maintaining the simple forcing-feedback picture, but at the expense of a more complicated expression for the forcing.

An alternative way of justifying this redefinition is to include, as part of the forcing, all responses that effect the energy budget very quickly — before the surface temperature responds substantially. This is what I referred to as the ultra-fast response in post 11 (ultra-fast = atmosphere in isolation – a month or so at most; fast = ocean mixed layer; slow = response of bulk of ocean; ultra-slow = even slower). In practice, one can try to estimate the forcing after the ultra-fast adjustment has taken place in a couple of ways. Using a full coupled atmosphere-ocean model, one can increase the CO_2 instantaneously, at $t = 0$, and watch how the ensemble mean N responds, extrapolating back to t=0 to estimate the forcing. The point here is not to look in detail at the ultra-fast response in the first month or so, but to extrapolate back using the "fast" relaxation characterizing the warming of the ocean surface — ie, plotting N vs T and extrapolating back to $T \approx 0$ (as in Essay 5). An alternative procedure is just to increase the CO_2 in an atmosphere/land model with fixed ocean temperatures and sea ice as boundary conditions and examine N in the equilibrated state. The first alternative seems more secure in that it is not subject to possible distortions due to decoupling atmosphere from ocean, but it may suffer from some fuzziness in the extrapolation if the distinction between ultra-fast and fast adjustments is not as sharp as we might like.

For the case of the stratospheric temperature adjustment to CO_2, you can avoid addressing this issue head on because, to first approximation, you can look at the energy balance at the tropopause rather than the top of the atmosphere. But it has become clear from the analysis of GCMs in the past few years that there is a potential for ultra-fast responses to CO_2 in the troposphere as well, particularly in the cloud field — see Gregory and Webb 2008, Colman and McAvaney 2011, and Andrews et al 2011. The result is that a part of the cloud change in a typical climate change scenario scales with the warming of the surface and another part does not, effectively scaling instead with the CO_2 itself. Letting X stand for the cloud field, we have formally

$$\delta X = (\partial X/\partial C)\delta C + (\partial X/\partial T)\delta T. \tag{22.9}$$

This splits the naive "cloud feedback" in the original analysis into two terms, moving one of them, the ultra-fast part that is realized before substantial temperature change occurs, into the forcing — leaving a remainder that scales with the surface warming and can usefully be thought of as a feedback.

The ultra-fast cloud response seems to be mostly confined to solar reflection from low clouds, at least in the Colman-McAvaney paper — which is also what I see in a quick look at some aqua-planet simulations with one of our models. What is the mechanism? I'm not sure –it's not clear to me whether land-ocean contrasts are the key, or whether open-ocean processes that can be studied with aqua-planet simulations capture the main mechanisms. A CO_2 change causes immediate changes in infrared fluxes, and these changes in fluxes can effect clouds in the absence of significant temperature changes — by changing the radiative cooling from cloud tops or the subsidence of air in the tropics (the adiabatic warming associated with this subsidence closely balances the radiative cooling) — both of which can effect low cloud amounts. I'll leave further discussion of the dynamics of the ultra-fast cloud response for another time (maybe I'll understand it better then).

Models' ultra-fast responses in precipitation have been discussed fairly extensively, partly with geoengineering in mind. Ming Zhao and I have recently looked at the ultra-fast response of tropical cyclones to CO_2, by fixing SSTs and increasing CO_2 in the model discussed in Essay 2. For doubling of CO_2 with fixed SSTs, we get about a 10% reduction in the number of tropical cyclones averaged over the globe, as shown in the plot at the top. These results may very well be model dependent. Almost all models show a reduction in global mean tropical cyclone numbers in global warming simulations, in which CO_2 and SSTs are increasing together. In our results, only about half of this global mean reduction is related to the warming.

Is any of this important? Between the fast times scales on which the oceanic mixed layer equilibrates and the slow time scales on which the deep ocean heat uptake saturates, the climate response tends to be proportional to the forcing itself, with a proportionality constant that depends both on the strength of the radiative restoring and on the efficiency of the heat uptake by the oceans. That is, in $N = F - \beta T$, one can set $N \approx \gamma T$ so that $T \approx F/(\beta + \gamma)$. See Essay 3, where I refer to this frequency band as the "intermediate regime" and argue (following many others) that it is relevant both for 20th century simulations and for 21st century projections. But if δT is proportional to δF and, therefore, to δC (assuming the forcing is dominated by CO_2), this extraction of part of the cloud response that scales with δC rather than δT is irrelevant. Testing this decomposition with observational trends also becomes very difficult.

Geoengineering is the place where this distinction is most clearly relevant. If one is successful, through solar management, say, in preventing warming, one still has to deal with consequences of the CO_2 increase that are not dependent on the warming.

Also, consider the standard idealized scenario where CO_2 increases up to some point and then is held fixed at that level thereafter. During the increasing phase, the ultra-fast responses are mixed in with the temperature-dependent responses, but after equilibration of CO2, we would see the effects of the remaining warming without the overlay of the ultra-fast response to CO_2. With respect to cloud feedback, this makes it harder to connect transient and equilibrium responses quantitatively .

I have already discussed in (post 5) an unrelated problem in connecting transient and equilibrium sensitivities — the potential for change in the strength of radiative restoring due to change in spatial structure of the warming as the system equilibrates. To the extent that we don't understand these things, it is that much more difficult to use evidence that bears on equilibrium sensitivity (primarily paleo) to constrain the transient climate response, and vice-versa.

23 Cumulative Emissions

[Originally posted February 13, 2012, 2012]

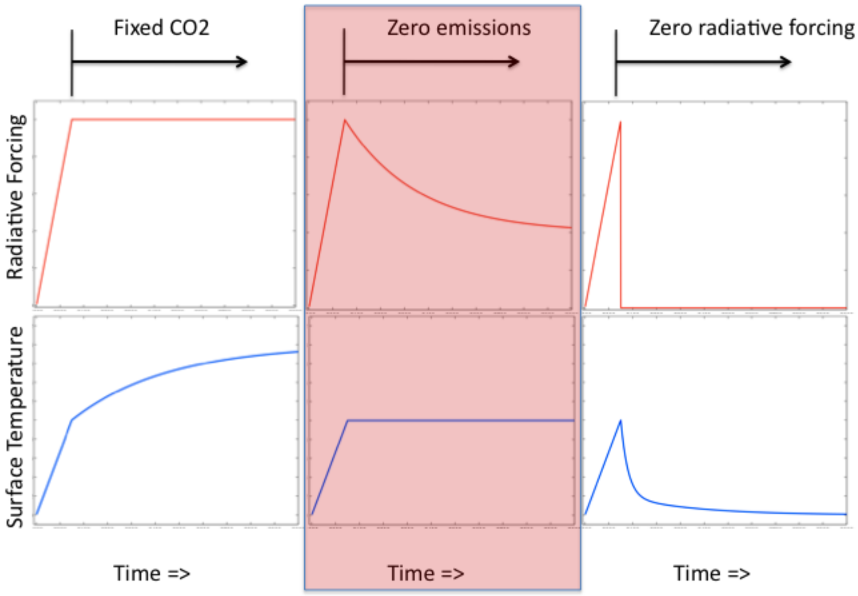


Figure 23.1: Schematic of three different idealized global warming scenarios. The time period is roughly 1,000 years and each scenario starts with the CO2 increase and warming from the anthropogenic pulse of emission in the 20th and 21st centuries. On the left, emissions are slowed so that CO_2 is maintained at the level reached at the end of this pulse. In the center, emissions are eliminated at the end of the pulse, resulting in slow decay of CO_2. On the right, CO_2 levels are abruptly returned to pre-industrial levels —perfect geoengineering — a scenario useful for isolating the recalcitrant component of warming discussed in Essay 8.

If we stop emitting CO_2 at some future time t how would surface temperature evolve over the ensuing decades and centuries — ignoring all other forcing agents? This question (or closely related questions) has been looked at using a number of models of different kinds, including Allen et al 2009, Matthews et al 2009, Solomon et al 2009, and Frolicher and Joos 2010. These models agree on a simple qualitative result: global mean surface temperatures stay roughly level for as long as a millennium, at the value achieved at the time t at which emissions are discontinued, as illustrated schematically in the middle panels above.

This simple result emerges from a cancellation between the climate response to CO_2 perturbations and the CO2 response to emissions. If the CO_2 in the atmosphere remained at the level attained at time T, then the surface would continue to warm as the deep ocean equilibrated and the heat uptake by the ocean relaxed to zero. This increase from a transient response with substantial heat uptake to a response with an equilibrated deep ocean is the fixed-concentration commitment.

However, when emissions are eliminated, the CO_2 in the atmosphere does not stay fixed, rather it decays slowly. This decay does not take the system back to pre-industrial CO_2 levels, since full equilibration requires transfer of carbon to sediments and crustal rocks, which requires far more than a millennium. We can imagine the airborne fraction of the emitted carbon as evolving in time, from a larger value before T, perhaps comparable to that observed in recent decades (about 45%), and then asymptoting, after roughly 1000 years, to a smaller non-zero value.

The papers listed above suggest that the reduction of airborne fraction from the current value to this equilibrated value more or less compensates for the additional warming that would be experienced with fixed CO_2. Additionally, the time scale of the adjustment of this airborne fraction and of the relaxation of the ocean heat uptake to zero are roughly the same — they are both controlled in large part by the physical mixing of shallow oceanic waters into the deeper oceans. This similarity in the slow adjustment time scale, and the coincidence of the rough cancellation of the fixed concentration warming commitment with the reduction in airborne fraction, combine to make plausible the relatively flat surface temperature response.

One could make a long list of things that could upset this picture, dramatic changes in land surface carbon uptake/release being an excellent example. In any case, it will be interesting to see what emerges from new generations of Earth System Models when applied to this idealized scenario.

It is worth keeping in mind that sea level, for example, will respond very differently in this zero-emission scenario — the component due to thermal expansion continues to rise on these time scales, in all of these models, as the surface warming penetrates further into the ocean. It is also worth keeping in mind that the temperature response to short-lived forcing agents, such as methane, would look more like the right panel in the figure, with the temperature response peaking at the time at which emissions are curtailed.

To the extent that the response of climate to emissions is linear, as it presumably is for small enough emissions, we could write an expression for the response of any climate index $\mathcal{C}$ to CO_2 emissions, $E(t)$:

$$\mathcal{C}(t) = \int_0^t G(t, t')E(t')dt' \tag{23.1}$$

where $G(t, t)dt'$ is the climate response at time t for unit emissions between the times t' and $t' + dt'$. ($t = 0$ is a time before which anthropogenic emissions are negligible.) If the problem can also be assumed stationary in time, then $G(t, t')$ is a function only of the time elapsed between forcing and response, $\tau = t - t'$. The claim is not that this linear perspective is the final story, of course, but only that it may be a useful point of reference. $G(\tau)$ combines the response of climate to CO_2 and the response of CO_2 to emissions.

What might $G(\tau)$ look like? Following the discussion above, for the global mean surface temperature we can imagine it looking as simple as

$$G(\tau) \approx A(1 - e^{-\tau/\tau_0}) \tag{23.2}$$

The fast relaxation time τ_0 is the time required for both the temperature of the ocean surface layer and its CO_2 concentration to equilibrate, treating the deeper ocean layers as infinite reservoirs of heat and carbon. A single relaxation time might not be adequate, but as long as the relaxation takes place fast enough, it would have little effect on the big picture except to smooth out the response to the high frequency component of the emissions. Might this $E \rightarrow T$ response function actually be more robust than the physical climate $CO_2 \rightarrow T$ or carbon cycle $E \rightarrow CO_2$ responses separately? If your model does not represent the time scale of the equilibration of the deep ocean adequately, this might, as mentioned above, have compensating effects on the shapes of the $CO_2 \rightarrow T$ and $E \rightarrow CO_2$ response functions, leaving the $E \rightarrow T$ response with the same relatively flat shape.

Ignoring the relaxation time τ_0, the implication of this simple form for the response is that the global mean surface temperature at time t can be thought of as simply the linear response to the sum total of past emissions

$$T(t) \approx A \int_0^t E(t')dt' \tag{23.3}$$

The implications of this result can be looked at as a glass half empty or half full. From a pessimistic perspective it says that, in the absence of geo-engineering, we are committed for the next millennium to the warming that we have already created by past emissions. More optimistically, we could say that future surface temperature increases are due to future emissions — that is, it is not the climate system that has committed us to additional warming, but rather the inertia in the society and infrastructure producing the emissions themselves.

To the extent that this picture holds, we can also say that the emissions trajectory over time is not particularly important for where we end up — the climate in the year 2100, say, would depend on the total emissions between now and 2100 and not on how these emissions were distributed over the 21st century.

The ease of communicating this result is also worth emphasizing. Only one number is needed — A — the warming per unit cumulative emissions. Typical central estimates for A in the papers listed above are in the range $1.5 - 2C$ per trillion tons of carbon emitted. Clearly, it is pretty important to know whether this simple picture is useful or misleading.

One final point. For the idealized scenario pictured in the central panel at the top of the post, the warming never approaches the value consistent with the equilibrium response to the maximum CO_2 at the end of the anthropogenic pulse. To relate the sustained temperature response to the maximum CO_2 we need to use the transient climate response (TCR). (See Essays 4-6 for discussion of the TCR). I think this is another good reason to place more emphasis on the TCR in discussions of climate sensitivity.

24 Arbitrariness in Feedback Analyses

[Originally posted February 28]

This post is concerned with arbitrariness in the terminology we use when discussing climate feedbacks. The choice of terminology has no affect on the underlying physics, but it can, I think, affect the picture we keep in our minds as to what is going on, and can potentially affect the confidence we have in this picture.

In feedback analyses of a climate response to some radiative forcing, we start with a reference response, the response "in the absence of feedbacks", and then we look at how this reference response is modified by feedbacks. An electrical circuit analogy often comes to mind, with the reference response analogous to the unambiguous input into a circuit. But the choice of reference response in our problem is ultimately arbitrary. The following is closely based on the introductory section of Held and Shell 2012.

My starting point is the same as that for several other posts: an expression for the net incoming flux of energy at the top of the atmosphere. Here I am just going to think of the incoming energy flux as a function of three numbers, $N(f, A, B)$, where f is the forcing agent and where A and B are two other things that N depends on. Perturbing our forcing agent we define the radiative forcing as $F = (\partial N/\partial f)\delta f$. I'll assume that F is positive. To establish a new equilibrium we need $\delta N = 0$ or

$$F = -(\partial N/\partial A)\delta A - (\partial N/\partial B)\delta B. \tag{24.1}$$

We could stop here, treating A and B on an equal footing. But suppose that we are mostly interested in δA. (In fact, let's suppose that A is the global mean surface temperature.) We no longer treat A and B in the same way but instead write the surface temperature response as

$$\delta A = -F/(\lambda_A + \lambda_B) \tag{24.2}$$

where

$$\lambda_A = \partial N/\partial A = \partial N/\partial T_{surf} < 0 \tag{24.3}$$

and

$$\lambda_B = (\partial N/\partial B)\delta B/\delta A \tag{24.4}$$

is a measure of B-feedback. (In using this terminology, we are presuming that δB can be thought of as proportional to δA — see Essay 22). The reference response in the absence of B-feedback is just

$$\delta A|_B \equiv -F/\lambda_A. \tag{24.5}$$

We can then write

$$\delta A/\delta A|_B = 1/(1 - \mu_B) \tag{24.6}$$

where

$$\mu_B \equiv -\lambda_B/\lambda_A \tag{24.7}$$

is a non-dimensional measure of the B-feedback.

Now I am going to make a choice for B that may seem a little odd — I'll choose B to be the tropospheric temperature. Most infrared photons escaping to space are emitted from the troposphere rather than the surface. If the troposphere does not warm, then to regain energy balance the surface has to warm by an order of magnitude more than if the tropospheric warming were comparable to that of the surface. If the surface and tropospheric responses are, in fact, comparable, this would be described as a large negative tropospheric feedback drastically reducing the magnitude of the reference response (the response at fixed tropospheric temperature).

This is clearly not the traditional formulation! The standard choice would be to set B equal to the surface temperature minus the tropospheric temperature, so that B-feedback would become lapse rate feedback. The reference response is now the response you get in the absence of lapse-rate feedback and is now much smaller — and the feedback relatively modest.

But what is wrong with this tropospheric feedback picture? It doesn't change the final answer — it just makes for a different decomposition between reference response and feedback. The main problem is that the no feedback limit, in this tropospheric feedback perspective, is not physically plausible. Warming the surface without warming the troposphere would destabilize the atmosphere, and atmospheric circulations would develop to transfer energy from the surface to the troposphere to fight off this destabilization. Atmospheric models of all kinds as well as observations (especially on interannual time scales) are consistent with this picture. By choosing an unphysical reference response, we end up with a framework in which the total response is a small difference between two large terms. It's not that it's wrong; it's just not natural.

The picture becomes even more problematic if we add another effect to the mix, for example a positive surface albedo feedback that we would typically think of as modest. For a given size of the radiative effect of the albedo change per unit surface warming, λ_{alb}, in $W/m2)/K$, the non-dimensional measure of the strength of the feedback,

$$\mu_{alb} = -\lambda_{alb}/\lambda_{surf} \tag{24.8}$$

will be very large because the reference response, $\propto 1/\lambda_{surf}$, is very large if one uses the tropospheric feedback framework. One is effectively evaluating the importance of albedo feedback by estimating how much it would increase temperatures while thinking that temperature perturbations are only damped by those infrared photons emitted by the surface. But this albedo feedback would never, in reality, operate in the absence of the strong negative "tropospheric feedback." So one gets a very skewed picture of the underlying dynamics, despite the fact that this is simply making a particular, unconventional, choice of reference response.

Those of you who have glanced at the paper linked to above will realize that by introducing the idea of the arbitrariness of the reference response, what I am really trying to do here is soften you up to the idea of redefining how we talk about water vapor feedback — by using a fixed tropospheric relative humidity, rather than fixed specific humidity, as the reference response. More about this in another post.

25 Relative Humidity Feedback

[Originally posted March 1, 2012]

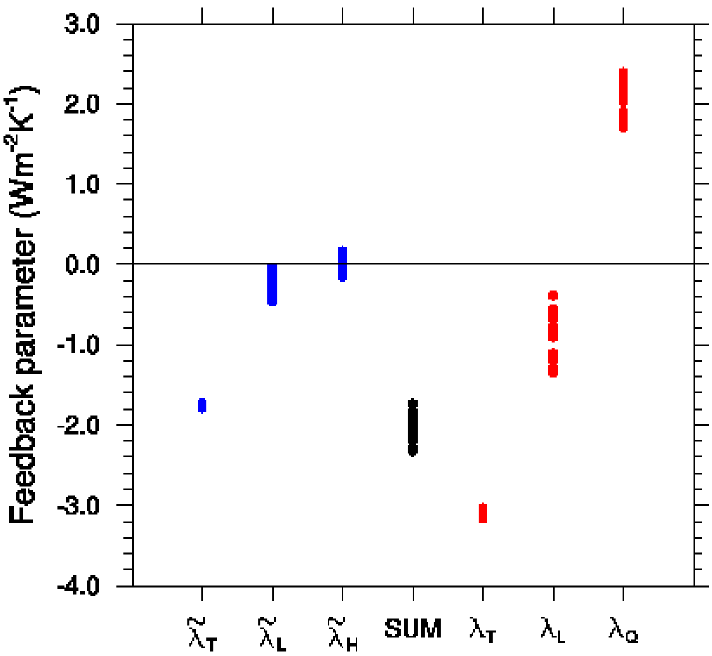


Figure 25.1: Fedbacks in AR4 models, from Held and Shell 2012. The three red columns on the right provide the traditional perspective: "Planck feedback"– the response to uniform warming of surface and troposphere with fixed specific humidity (λ_T), lapse rate feedback at fixed specific humidity (λ_L), and water vapor feedback (λ_Q). The three blue columns on the left provide an alternative perspective — with fixed relative humidity uniform warming feedback ($\tilde{\lambda}_T$), fixed relative humidity lapse rate feedback, ($\tilde{\lambda}_L$), and relative humidity feedback ($\tilde{\lambda}_H$). The sum of the three terms, shown in the middle black column, is the same from either perspective. Surface albedo and cloud feedbacks are omitted. Each model is a dot.

This is the continuation of post 24, describing how we can try to simplify the analysis of climate feedbacks by taking advantage of the arbitrariness in the definition of our reference point, or equivalently, in the choice of variables that we use to describe the climate response. There is nothing fundamentally new here — it is just making explicit the way that many people in the field actually think, myself included. And if you don't like this reformulation, that's fine — it's just an alternative language that you're free to adopt or reject.

The simplest way to think about this reformulation it that it describes climate change in terms of changes in temperature and relative humidity rather than temperature and specific humidity (or water vapor concentration or vapor pressure). Consistently, the reference response is computed by assuming that the surface and troposphere warm uniformly while relative humidity within the troposphere remains unchanged.

The key is the claim that fixing the relative humidity is a much more natural starting point than fixing specific humidity. I am open to new observations or models that point in a different direction, but I don't see anything on the horizon that looks like it will modify my personal expectation in this regard. I will try to explain why I feel this way in forthcoming posts. But here are a general comment to think about in the meantime:

We want to use a reference response that is physically meaningful in itself — ie, that doesn't require "feedbacks" to be present to ensure that it remains physically meaningful as climate changes. But specific humidity can't remain fixed as we cool the climate — the atmosphere would become supersaturated in a lot of places. And this would happen pretty quickly; the amount of cooling at the peak of the last glacial would be more than enough. Why should fixing specific humidity be a useful starting point as we warm but not as we cool the atmosphere? We would have to argue that there is something special about the position of the present climate in the space of climates with different temperatures.

Using the same notation as in the previous post, and ignoring clouds and surface albedos, in the traditional formulation we have

$$\delta N = F + (\lambda_T + \lambda_L + \lambda_Q)\delta T_{surf} \tag{25.1}$$

where the three terms on the right account respectively for the effect on the incoming top-of-atmosphere flux N of a uniform increase in temperature of the surface and the troposphere, the effect of differences between the tropospheric and surface temperature responses, and the effects of the increase in water vapor. In equilibrium,

$$\delta T_{surf} = \delta T|_Q/(1 - \mu_L - \mu_Q) \tag{25.2}$$

where the reference response with fixed specific humidity is $\delta T|_Q \equiv -F/\lambda_T$ and where $\mu_L = -\lambda_L/\lambda_T$ and $\mu_Q = -\lambda_Q/\lambda_T$. Estimates from the AR4 models are shown in red in the figure above.

We now divide λ_Q into three terms: the effect on the flux of the increase in vapor needed to maintain fixed relative humidity, assuming that the troposphere warms by the same amount as the surface (λ_{QT}); the effect of the additional vapor needed to maintain relative humidity given that the tropospheric and surface warming differ (λ_{QL}); and the effect of changes in relative humidity ($\tilde{\lambda}_H$). We can then define the effect of warming the troposphere equal to that of the surface, with fixed relative humidity, as $\tilde{\lambda}_T = \lambda_T + \lambda_{QT}$, and the effect of the lapse rate change with fixed relative humidity $\tilde{\lambda}_L = \lambda_L + \lambda_{QL}$:

$$\tilde{\lambda}_T + \tilde{\lambda}_L + \tilde{\lambda}_H = \lambda_T + \lambda_L + \lambda_Q \tag{25.3}$$

The decomposition on the left side is shown for the same AR4 models in the blue columns in the figure. Corresponding to this reformulation, we can also define a new reference response, $\delta T|_H \equiv -F/\tilde{\lambda}_T$ and non-dimensional feedback strengths, $\tilde{\mu}_L = -\tilde{\lambda}_L/\tilde{\lambda}_T$ and $\tilde{\mu}_H = -\tilde{\lambda}_H/\tilde{\lambda}_T$.

$$\delta T_{surf} = \delta T|_H/(1 - \tilde{\mu}_L - \tilde{\mu}_H) \tag{25.4}$$

So you can describe these model responses as starting with the fixed relative humidity-no lapse rate change reference (about 1.75 (W/m2)/K) with a bit of negative fixed relative humidity-lapse rate feedback (about 0.25 (W/m2)/K) and very small relative humidity feedback, leading to the 2 (W/m2)/K total in the absence of any surface albedo or cloud feedbacks. I think we can agree that this is a simpler picture of the model responses, avoiding the cancellation between the large positive water vapor and negative lapse rate feedbacks.

A key point is that the scatter among the models in the individual terms is now considerably smaller. The tendency for water and lapse rate feedbacks to be negatively correlated across models has been noted since these feedback analyses were first performed across multiple models (Zhang et al 1994) and has been discussed recently by Ingram 2010. At least from this perspective of the model responses, avoiding the negative correlation seems like a very helpful simplification.

Another interesting point is that the fixed relative humidity lapse rate feedback is negative, albeit small. This is the basis for my comment in Essay 20 regarding why I thought that negative lapse rate feedback wins out over increased water vapor feedback when a model's tropical upper tropospheric warming is increased.

It is also interesting to add other sources of feedbacks, like clouds, into the mix. The cloud feedback as measured by λ_{cloud} is unchanged by anything said here. But the non-dimensional measure is changed, from $\mu_{cloud} = -\lambda_{cloud}/\lambda_T$ to $\tilde{\mu}_{cloud} = -\lambda_{cloud}/\tilde{\lambda}_T$. In the traditional perspective, cloud feedback is effectively thought of as independent of water vapor feedback. But if cloud feedback is negative, say, then the resulting reduction in the temperature response will reduce the water vapor in the atmosphere, assuming fixed relative humidity, which makes the effect on temperature of this negative feedback stronger. I think this way of looking at things gives us a better picture of the net effect of cloud feedbacks.

It is also worth thinking about this reformulation from the perspective of the issue of skewness in our uncertainty in climate sensitivity (ie Roe and Baker 2007). If we have a distribution of values of μ that is symmetric about its mean, then the distribution of $1/(1-\mu)$ will be skewed with a long tail towards higher values. But in the reformulation using a fixed relative humidity refrence, we have increased the value of the reference response, the numerator in $\delta T = \delta T|_{ref}/(1-\mu)$, and increased the denominator by the same factor, so we have decreased the total μ. How does this square with the skewness argument, since I have repeatedly stressed that we're not changing anything about the final result, just our interpretation of it? I'll leave this for the reader to think about.

26 Relative Humidity in Cloud Resolving Models

[Originally posted April 9 2012]

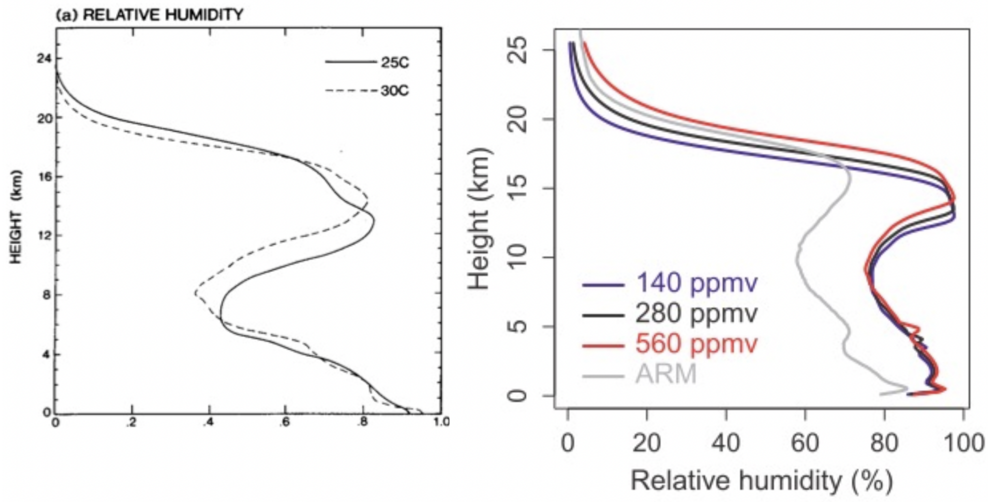


Figure 26.1: Time and spatially averaged relative humidity profiles from radiative-convective equilibrium simulations with cloud-resolving models. The figure on the left is from Held et al 1993 and shows results from two simulations differing by $5°C$ in the prescribed surface temperature. That on the right is from Romps 2011 and shows the result of changing the CO2 and adjusting surface temperatures to keep the net flux at the top of the atmosphere unchanged. (Also shown on the right is the observed profile at a tropical western Pacific ARM site.)

Regarding water vapor or, equivalently, relative humidity feedback, we can think of theory/modeling as providing a "prior" which is then modified by observations (trends, interannual variability, Pinatubo response). My personal "prior" is that relative humidity feedback is weak. or, conversely, that the strength of water vapor feedback in our global models is about right.

In justifying this prior, I like to start with the rather trivial argument, already mentioned in the last post, that the amount of water vapor in the atmosphere cannot possibly stay unchanged as the climate cools since many regions will become supersaturated, including the upper tropical troposphere where most of the water vapor feedback is generated.. So to expect specific humidity to remain unchanged as the climate warms requires the present climate to be close to a distinguished point as a function of temperature – the point at which water vapor stops increasing as temperatures increase. Its not impossible that we do reside at such a point, but you're going to have work pretty hard to convince me of that — it doesn't strike me as a plausible starting point.

Of course, there is also the community's collective experience with global atmospheric models over the past several decades. Less familiarly, there is experience more recently with the kind of "cloud-resolving" models (CRMs) discussed in Posts 19-20. I am going to focus on the latter here. This will have the advantage of introducing what I consider to be the physical mechanism that could most plausibly alter the strength of water vapor feedback.

In global climate models in use for climate studies, which typically have horizontal resolutions of 50-200 kms, most of the turbulent motions transporting heat, momentum and water vertically in the tropics are not resolved by the grid and must be provided by a sub-grid closure scheme. Our ability to develop convincing closures for moist convective turbulence remains limited. In contrast, in CRMs the horizontal grid size might be 1-3 kms, which begins to resolve the most energetic motions responsible for vertical mixing in the tropics, the convective plumes that reach from the surface to the upper troposphere. There is still sensitivity to the treatment of subgrid motions at even smaller spatial scales, but there is little doubt that the explicitly resolved deep convective plumes provide more realistic simulations of tropical mixing than our attempts at subgrid closure in global models.

One goal of this work is to devise CRM computations in small domains that provide insights into climate sensitivity. A basic starting point is radiative–convective equilibrium in a doubly periodic and, therefore, horizontally homogeneous, box. Fix the surface temperature, let the radiative fluxes cool the troposphere, and see how the convection, clouds and relative humidity distributions develop as the system achieves it statistically steady state (its climate). Then increase the surface temperature and study how the model climate responds. (Or increase the CO2 and iterate the surface temperature so as not to perturb the net energy flux at the top-of-atmosphere.) I like to think of radiative-convective equilibrium as the Rayleigh-Benard Convection of climate theory. (Unfortunately, there is no good way of simulating it in the laboratory.)

The figure on the left at the top is from one of the first CRM studies of moist radiative-convective equilibrium, with a 2D model and with the relatively low resolution of 5km. It shows the equilibrated horizontally and time averaged relative humidity profile for a control simulation and for another simulation with an increase of 5K in the imposed surface temperature. On the upper right is the result of a more recent and much higher resolution 3D simulation. (Here the perturbation runs have both increased surface temperature and increased CO_2, so they include what in previous posts I have called the "ultra-fast" response to CO2, as well as the usual effects of the increase in surface temperature due to the CO_2 increase.)

The first thing to notice is that the changes in the relative humidity profile in these models as the climate warms are small. (Some aspects of these small changes, especially the upward displacement of the relative humidity profile in the upper troposphere, are robust features across simulations of this type.) The point of comparison is the reduction in relative humidity needed to maintain constant specific humidity, which would be about 6%/degree C warming near the surface to 12%/C or so in the upper troposphere. The temperature changes in these simulations are close to moist adiabatic, as shown in Post 20 for the Romps simulation. So the relative humidity feedback in these models is very weak, just as in global models. There are a lot of these cloud resolving models in the literature, in domains with different sizes and shapes, some in the "pure" radiative-convective configuration I am focusing on here, others with some specified large-scale flows superposed — they all look similar in this regard.

Something else you have probably noticed is that the relative humidities in these two models are quite different (the model on the left is drier throughout most of the troposphere). While I am only showing two models, this large spread is also representative of models of this type. The following picture is the one that I think of when looking at results such as these.

Air is carried up to the upper troposphere in deep convective plumes that extend to various heights – when the air emerges from these plumes it descends very slowly. This skewness of vertical motion, rapid ascent and slow descent, is consistent with the small areas covered by the convective plumes, and is a distinctive feature of the tropical atmosphere, as understood by Riehl and Malkus in the late 1950's . We can think of the air as saturated when it emerges from the convective plumes, and as it descends it warms and its relative humidity quickly starts dropping. If it makes it into the lower troposphere without incident its relative humidity would be a few percent or lower! But this decent might take 2 or 3 weeks, so descending parcels will typically mix with air that has recently been moistened by water detrained from convection at these lower levels. Near the surface the air is moistened by mixing with the water that is constantly evaporating from the oceans (these models typically assume a saturated surface), so we end up with a minimum of relative humidity in the interior of the troposphere. But the strength of this minimum depends on how far typical parcels descend before being moistened, and this depends on how the convection is organized.

Picture someone in the tropics with a big garden hose pointing upward, trying to moisten the air before it descends too far and its relative humidity drops to near zero. Standing in one place would be very ineffective –assuming that there is relatively little horizontal mixing going on — the hose would always be moistening the same small fraction of parcels, wasting water by moistening parcels that are already moist, and allowing the troposphere as a whole to dry. Moving the hose around or using many small hoses simultaneously so to moisten more air parcels, while using the same amount of water, would be much more effective.

Post 19 shows an example of how the organization of convection can change drastically in a CRM as a function of a model parameter. The mid-tropospheric minimum in time-averaged relative humidity in the aggregated state is 15% in the model described there, as constrasted with about 60% in the more disaggregated state! CRMs differ in their mean relative humidity profiles because they organize convection differently, due to model differences of various kinds, including domain size and shape, resolution, cloud-radiation interactions, surface flux formulations, etc — these dependencies are not well understood. Relative humidity changes are small when these models are warmed because this organization does not change significantly in response to the warming.

Could it be that convection aggregates more as the climate warms? If you are looking for a way to weaken water vapor feedback, this is one of the better places to look., but you'll need a lot of aggregation to compete with Clausius-Clapeyron. Google "convective organization and water vapor feedback" to get a feel for what people are thinking about. Are horizontally homogeneous models of radiative-convective equilibrium the appropriate theoretical tools for studying this? The problem is that they are missing a lot of the processes that generate flows that mix water vapor horizontally, mixing that likely reduces the sensitivity of humidity to convective organization. These flows are better represented in global models, despite limitations in their representations of moist convection. CRMs in small domains may distort the big picture

Despite these possibilities, integrations with CRMs to date, such as those shown at the top, have helped solidify my theoretically-based prior, which I think of as the most conservative possible: nothing much changes, including the convective organization.

27 Estimating TCR from Recent Warming

[Originally posted April 30, 2012]

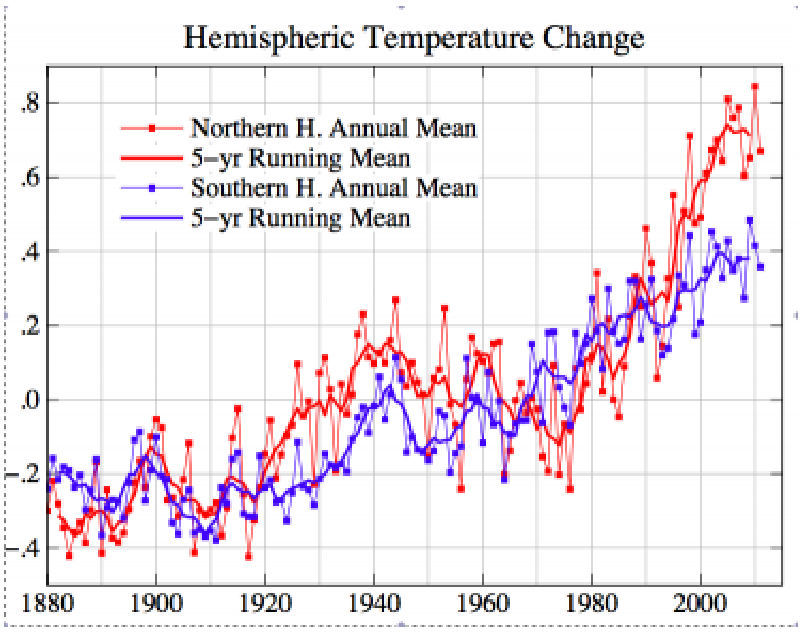


Figure 27.1: GISTEMP annual mean surface temperatures (degrees C)for the Northern and Southern Hemispheres

Here's an argument that suggests to me that the transient climate response (TCR) is unlikely to be larger than about 1.8C. This is roughly the median of the TCR's from the CMIP3 model archive, implying that this ensemble of models is, on average, overestimating TCR

Formally, we define the TCR of a model by increasing the CO_2 at the rate of 1%/year and looking at the global mean surface warming at the time of doubling. I have discussed the relevance of the TCR for attribution of 20th century warming and for warming scenarios over the next century in several earlier posts (3,4,6). Gregory and Forster 2008 – GF08 – is a good reference on this topic. The discussion below assumes that, for the time scales of relevance here, the forcing and response are more or less proportional with negligible time lag (i.e. $F = \beta T + N$ were F is the forcing and N the ocean heat uptake, but $N = \gamma T$, so $T = \xi F$ where $\xi = 1/(\beta + \gamma)$). TCR is then obtained by multiplying ξ by the forcing for CO_2 doubling. TCR is smaller than the equilibrium response to CO2 doubling (the climate sensitivity) because of the effects of heat uptake — but note also the complication discussed in post 5: the strength of the radiative restoring can change (it typically decreases in models) as the deep ocean equilibrates to a change in forcing. I won't discuss equilibrium sensitivity further here.

The figure at the top of the post shows the time series of surface temperature averaged over the two hemispheres, from GISTEMP. The Southern Hemisphere (SH) has warmed relatively steadily over the past century, while the Northern Hemisphere (NH) warmed more rapidly in the first part of the century and from 1970-2000, with the familiar cooling episode in between. I expect the response to the WMGG's to be roughly separable in space and time: $A(t)B(x, y)$. One might conceivably see a slow drift in the pattern of the response, and some changes in structure near the sea ice edge, but it is hard to see how multi-decadal swings in the spatial structure could emerge from the forced response to WMGGs. GCM simulations are consistent with this expectation.

So this structure could be due either to the response to other forcing agents, aerosols in particular, or to internal variability. The major source of internal variability on these time scales is thought to be the pole-to-pole overturning circulation in the Atlantic ocean. Variations in the strength of this circulation alter the temperature difference between the hemispheres. In models, the mean NH temperature is a lot more responsive than the SH to this variability, so a stronger than average overturning warms the NH more than it cools the SH, resulting in a global mean warming and providing a consistent picture for the relatively steady SH trend. On the other hand, aerosol forcing is predominately located in the Northern Hemisphere, also providing a natural explanation for the relative shape of these curves to the extent that the time variation of the forcing matches features in the observed NH temperature variations.

It is important to sort out whether the non-WMGG forced response or internal variability is dominant in this regard, or if they both contribute substantially. But here I want to see what this plot implies about the TCR, irrespective of which of these sources is dominant. To do this, I am going to focus on the latest period, since 1980 or so, in which the rate of NH warming has been unusually large compared to that in the SH. Focusing on this most recent 30 year period has advantages because it is the satellite era, so we have more observations of things like total solar irradiance that help us reduce the mechanisms that we need to consider.

GF08 discusses the estimate of TCR that one obtains by making the simple assumption that neither internal variability nor aerosols affect the trend over the period since 1980. The WMGG forcing from 1980 to 2010 is 1.1 W/m2 using standard expressions (see this NOAA site), and is fairly linear in time. With a warming of 0.5K in global mean temperature T, this would require a value of ξ (in $T = \xi F$) of about $0.45C/(Wm^{-2})$. GF08 remove volcano years before regressing T against F, and one could also remove ENSO as do Lean and Rind 2009 and Foster and Rahmstorf 2011, in order to reduce the scatter before estimating ξ, but this doesn't change the overall trend in temperature much and so doesn't change the central estimate of ξ. A value of $\xi = 0.45$, multiplied by the standard CO2-doubling forcing of $3.7W/m^2$, gives a value of about 1.8C for TCR.

GF08 use an estimate of the internal variability in 30-year trends (obtained from a GCM) to expand the uncertainty in this estimate beyond that coming from the regression itself; they assume that the system is equally likely to have been in a warming phase of multidecadal variability as a cooling phase over this period, so their uncertainty range remains centered around the TCR value of 1.8C.

But the rapid warming of the NH with respect to the SH over this 30 year period requires an explanation other than WMGGs. One possible explanation is that aerosol forcing has decreased over this period, enhancing NH warming. But if that is the case, the aerosol reduction is providing some of the global mean warming as well, so the total WMGG+aerosol forcing over this period would be enhanced, reducing the value of ξ. If instead internal variability is the culprit in the large recent differential warming of the hemispheres, we reach the same conclusion — this variability would have contributed not just to the differential warming but to the global mean warming (see, for example, Knight et al 2005 orZhang et al 2007), requiring us to lower our estimate of ξ as before.

By how much should we lower this estimate? You need to quantify and distinguish between the aerosol and internal variability sources to go much further. My personal best estimate is currently about 1.4C — I won't try to justify this further here, but it is close to the central estimate for TCR in the recent paper by Gillett et al 2012.

A TCR of 1.4K corresponds to a value of $\xi \approx 0.38K/Wm^{-2}$ and $1/\xi \approx 2.65Wm^{-2}/C$. Assuming a typical GCM heat uptake efficiency, $\gamma \approx 0.7W/m^2$ (I would really like to have a simple theory for this number), this gives a radiative restoring strength of $\beta \approx 1.95Wm^{-2}/C$. This is roughly the value you get from fixed relative humidity models with no cloud feedback (see post 25). You need some positive cloud feedback or greatly reduced heat uptake to get up to a TCR of 1.8C. With estimated current WMGG radiative forcing of about $2.8W/m^2$, and with a climate resistance of $0.38C/Wm^{-2}$, you still need aerosol forcing of about $-0.7W/m^2$ to get the century-long global warming down to 0.8C.

It's a simple story, based on a lot of assumptions. Analysis of GCMs with this argument in mind might help focus attention on aspects of model simulations that constrain TCR — or it might indicate weaknesses in the argument, allowing models to be consistent with the recent rate of warming in both hemispheres while simultaneously possessing a TCR larger than 1.8C.

28 The "Fruit Fly" of Climate Models

[Originally posted May 25, 2012]

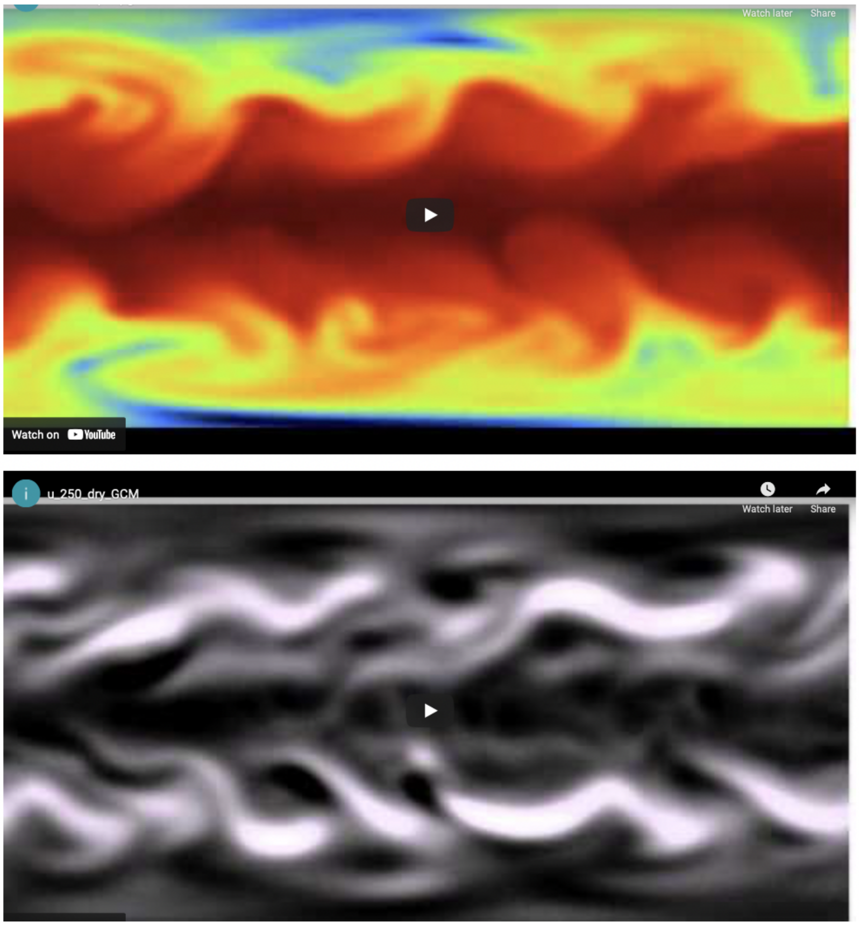


Figure 28.1: Snapshot of the near surface temperature (top) and upper tropospheric zonal winds (bottom) in an idealized dry atmospheric model.

The first 500 days of spinup from a state of rest are shown at one frame per day for the entire globe here and here.

As a change of pace from discussions of climate sensitivity, I'll describe an idealized atmospheric model that I think of as an important element in a model hierarchy essential to our thinking about atmospheric circulation and climate.

Many of my colleagues are probably tired of hearing me talk about the importance for climate theory of studying a hierarchy of climate models and especially tired of hearing me make the analogy with the hierarchy of model organisms (E. Coli, yeast, fruit fly, zebra fish, mouse, etc) that biologists utilize so effectively. As I have written in a little essay on this subject, biologists have the advantage — their hierarchy is provided by nature, and the conservative character of much of evolution provides confidence that a lot of what we learn from simpler life forms carries over to more complex organisms. Climate theory requires such a hierarchy as well — how else do you go about trying to understand a complex system that you cannot easily perform controlled experiment on? — but we need to construct it ourselves, and agree among ourselves on which idealized systems to study intensively. For a discussion of the atmospheric circulation from the perspective of the insights gained from working with a hierarchy of atmospheric models, see the excellent review by Schneider 2006.

The model I'll describe here is of a dry atmosphere, an ideal gas on a spherical rotating planet forced only by radiative fluxes — modeled as a simple relaxation of temperature to a "radiative equilibrium" that is a function of latitude and pressure — and a frictional force that relaxes the flow near the surface to zero (in the reference frame rotating with the surface). The model equations are described in Held and Suarez 1994. You can get a feeling for how this and similar setups have been utilized, both for testing numerical methods and for exploring climate dynamics, by googling held suarez idealized gcm.

The model is designed to capture some of the complexity of midlatude jets and storms tracks on a rotating sphere. The climate that emerges (the statistics of the winds and temperatures) has a lot of features that are quite Earth-like. The animations at the top show the near surface temperature and the upper tropospheric zonal (east-west) component of the winds spinning up from a state of rest, using a vanilla spectral model with modest resolution — 20 vertical levels and T42 horizontal resolution — meaning that all fields are expressed as sums over the spherical harmonics Y_ℓ^m with total wavenumber $\ell \leq 42$. Here's a plot of the time-averaged zonally-averaged zonal winds (zonal = east-west) produced by this model (actually an average over the 2,000 days following the 500 day spinup shown in the animations). The contour interval is 5m/s. The zero contour meets the surface near 30 and 60 degrees latitude. I have starting calling this the

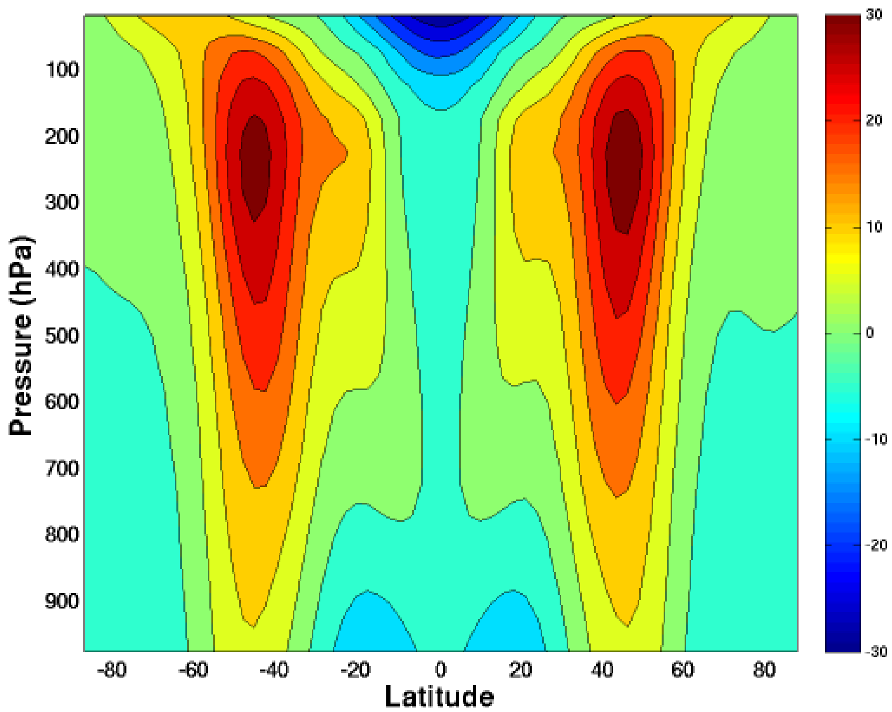


Figure 28.2: Snapshot of the near surface temperature (top) and upper tropospheric zonal winds (bottom) in an idealized dry atmospheric model.

fruit fly of climate models. I am not expecting this terminology to catch on, but fruit fly seems about right to me — the model is complex enough to be turbulent and chaotic, with a lot of space and times scales involved, and I think it repays close study, but it is missing many sources of complexity present in the Earth's atmosphere, and in more comprehensive models. In particular, there are no clouds or even water vapor .

Here's how the time and zonally averaged winds near the surface change as you increase or decrease the rotation rate by modest amounts from it's Earth-like value:

The circulation pattern shifts polewards as the rotation rate decreases. The location of the midlatitude westerlies marks the location of the storm tracks, and the transition from easterlies to westerlies in the subtropics marks the region of mean subsidence that generates the subtropical arid zones — in models with water vapor and precipitation. So understanding shifts like these can be important for a lot of reasons. We can think of this problem as a test of our ability to reason about this kind of thing, before

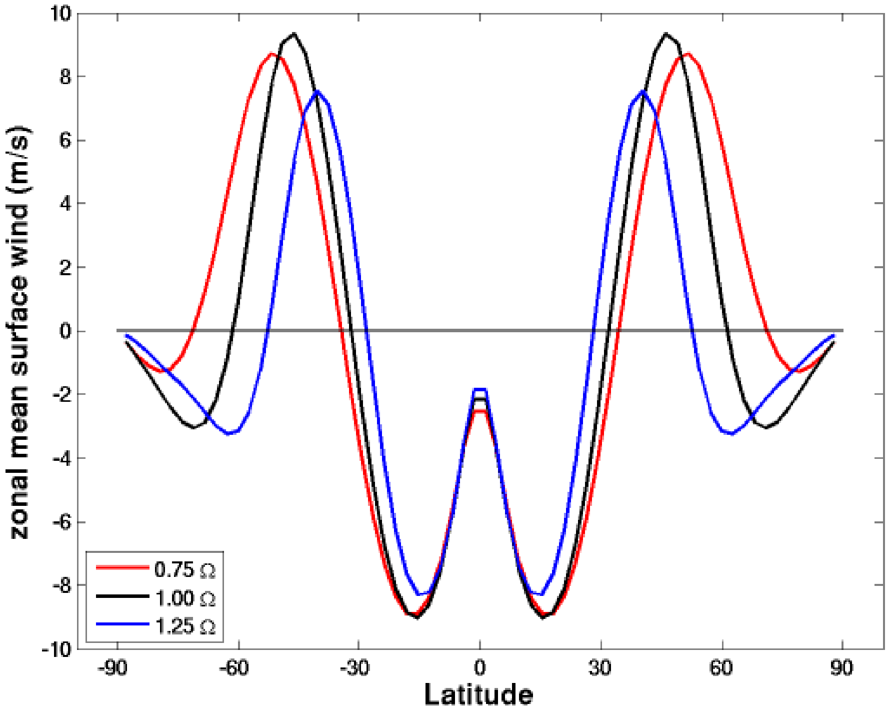


Figure 28.3: Snapshot of the near surface temperature (top) and upper tropospheric zonal winds (bottom) in an idealized dry atmospheric model.

tackling the question of circulation shifts in response to global warming.

Gang Chen, Walter Robinson and I looked at how this pattern shifts as the strength of the surface drag is increased or decreased with this same setup in Chen et al 2007. The circulation moves polewards as the drag per unit surface wind is reduced — ie, as one makes the surface smoother. We offer an explanation in the paper, but I don't think we understand it as well as the rotation dependence. The same dependence survives in more comprehensive GCMs — a rougher (sorry – smoother 5/30/12) land surface moves the westerlies and the storm track polewards and decreases polar surface pressures — and is a significant issue when trying to understand model biases and inter-model differences.

If you vary the rotation rate over a larger range in this model, more dramatic things happen. For high rotation rates, the circulation takes on a Jovian appearance with multiple jets in each hemisphere; at very low rotation rates, it looks more Venusian, with a Hadley cell extending from equator to pole and with the upper tropospheric flow resembling solid body rotation at an angular velocity larger than that of the surface (a "superrotating" state).

One way that this model helps me is when I am exposed to a new idea for, say, the time-averaged state of the atmosphere — perhaps for the equator-pole temperature gradient, or the globally averaged kinetic energy generation and dissipation. (For example, there are suggestions that simple variational principles explain these things.) The first thing I do is ask if there is anything in the formulation of the theory that precludes it from being applied to this relatively simple atmosphere-like model. (The fruit fly can't be used to study immunological responses only found in vertebrates, say.) If not, we can go ahead and test the theory. In my experience, it is often better to be less ambitious and develop and test theories for these turbulent chaotic flows directly, and only after proving them to be useful in idealized contexts make a case for their relevance to the real world.

29 Eddy Resolving Ocean Models

[Originally posted June 27, 2012]

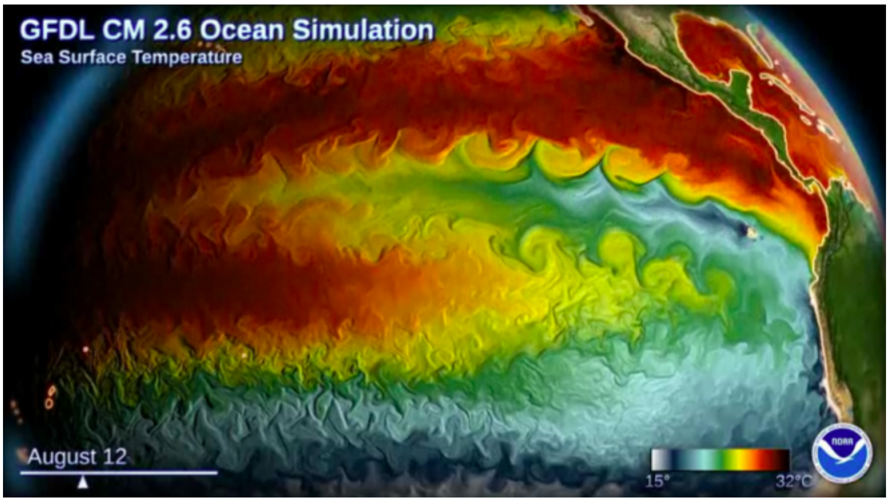


Figure 29.1: Snapshot of the sea surface temperature in a coupled climate model under development at GFDL, the ocean component having an average resolution of roughly 0.1 degree latitude and longitude. Click here for the animation. (Visualization created by Remik Ziemlinski; model developed by T. Delworth, A. Rosati, K. Dixon, W. Anderson using MOM4 as the oceanic code base.)

As models gradually move to finer spatial resolution we naturally expect to gradually improve our simulations of atmospheric and oceanic flows. But things get especially interesting when one passes thresholds at which new phenomena are simulated that were not present in anything like a realistic form at lower resolution. The animation illustrates what happens after one passes through an important oceanic threshold, allowing mesoscale eddies to form, filling the oceanic interior with what we refer to as geostrophic turbulence. At resolutions too coarse to simulate the formation of these eddies, flows in ocean models tend to be quite laminar except for some relatively large scale instabilities of intense currents of the kind seen in the snapshot north of the equator in the Eastern Pacific. (For a transition comparably fundamental in atmospheric models, one has to turn to the point at which global models begin to resolve the deep convective elements in the tropical atmosphere — see for example Post 19).

When one makes the transition to a mesoscale eddy-resolving ocean model, one is in a sense just catching up with standard-resolution atmospheric models — the eddy production process involved is essentially identical to the process, referred to as baroclinic instability, that generates midlatitude cyclones and anticyclones in the atmosphere. The difference is that the scale at which eddies are generated by this process is much smaller in the ocean than in the atmosphere.

[Theory tells us that a key scale is $\sqrt{g^*H}/f$ where H is the vertical scale of the flow, f is the Coriolis parameter (twice the angular velocity of the Earth multiplied by the sin of latitude), and g^* is the "reduced gravity", the gravitational acceleration multiplied by the factional change over the vertical scale H in the potential density (the density of a parcel when carried adiabatically from its ambient pressure to a reference pressure). The bottom line is that the reduced gravity is much smaller in the ocean than in the atmosphere.]

Just as for the high resolution atmospheric simulations animated in posts 1 and 2 , it is a challenge to confront these simulations with observations in the most informative way. One observational constraint that has been especially useful for a first look at the quality of the mesoscale eddy field is the estimate of kinetic energy in the surface flow provided by satellite altimetry. (The horizontal gradient of sea surface height provides an estimate of surface currents through the geostrophic relation.) Here's a comparison of this model with an observational estimate, described in Delworth et al 2011. Note that the color scale is logarithmic — $\log(cm^2/s^2)$. The geostrophic relation breaks down near the equator. Among many aspects of these eddy-resolving simulations that are worthy of close study — mesoscale eddies are known to interact with convection in key regions of deep- water formation; eddies and vortices forming around the Cape of Good Hope appear to be important for the saltiness of the Atlantic; and, perhaps most importantly, these eddies help set the strength and structure of the Antarctic Circumpolar Current and its sensitivity to changes in wind and thermal forcing — this being a a key region for heat and carbon uptake. Eddy heat transport across the circumpolar current could play a crucial role in regulating how fast the waters around Antarctica warm. Lower resolution ocean models include closure schemes for the fluxes associated with these mesoscale eddies, but these remain relatively crude (I can say this because I have spent

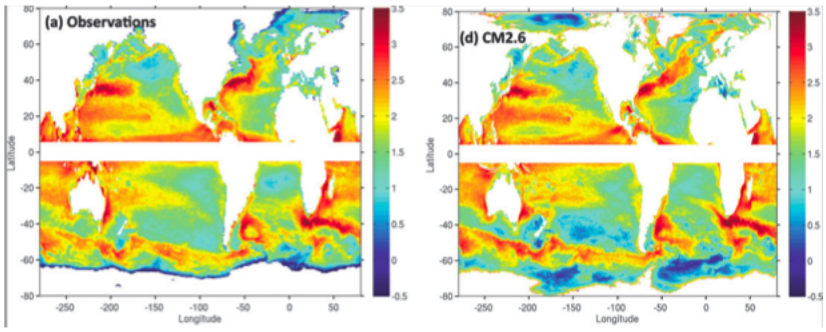


Figure 29.2:

some time trying to develop these theories in both atmospheric and oceanic contexts, as illustrated here.) There is little doubt that direct simulation is better than any existing closure schemes.

On the other hand, these oceanic eddies are not as dominant as they are in the atmosphere. This is at least in part because the basin geometry creates north-south currents that play a significant role in oceanic north-south heat transport, unlike the atmosphere where poleward heat flux is dominated by eddies. (The latitude band of the Drake passage is distinct in this regard, with no meridional coast along which boundary currents can form, making the dynamics in the Circumpolar Current more atmosphere-like. But we'll have to stay tuned to see how our overall perspective on the role of the oceans in climate change is altered by these eddy resolving ocean models — a problem that is being tackled at a number of modeling centers around the world.

Note: The calendar indicator in the lower left corner of the animation seems to be off.

30 Extremes

[originally posted August 4 2012]

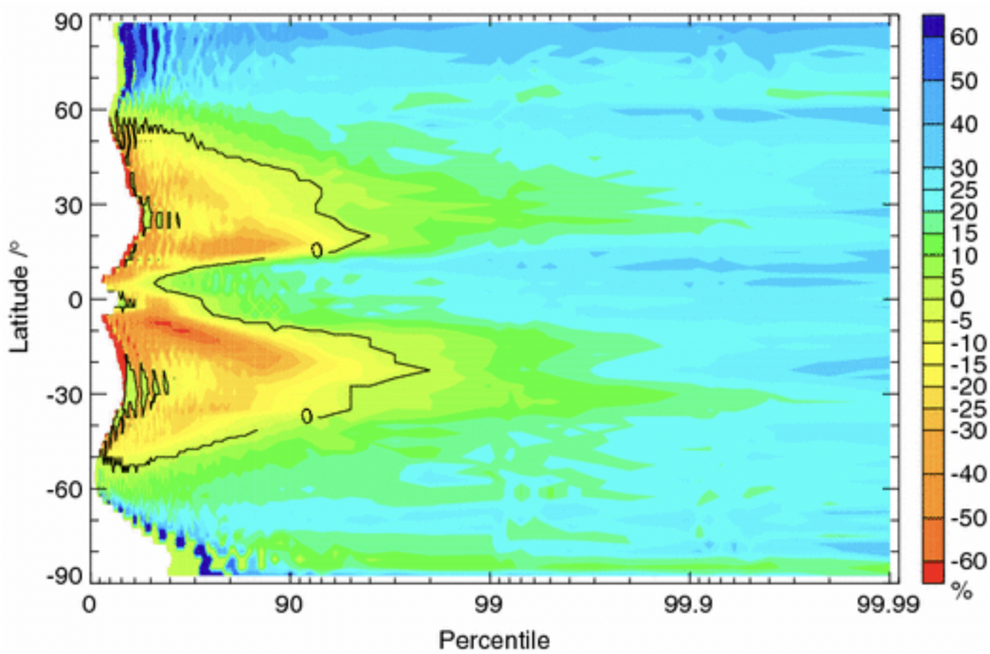


Figure 30.1: Percentage change in the precipitation falling on days within which the daily precipitation is above the pth percentile (p is horizontal axis) as a function of latitude and averaged over longtitude, over the 21st century in a GCM projection for a business-as-usual scenario, from Pall et al 2007.

When I think about global warming enhancing "extremes", I tend to distinguish in my own mind between different aspects of the problem as follows (there is nothing new here, but these distinctions are not always made very explicit):

- **Increases in the frequency of extreme high temperatures that**

result from an increase in the mean of the temperature distribution without change in the shape of the distribution or in temporal correlations

The assumption that the distribution about the mean and correlations in time do not change certainly seems like an appropriately conservative starting point. But if you look far out on the distribution, the effects on the frequency of occurrence of days above a fixed high temperature, or of consecutive occurrences of very hot days (heat waves), can be surprisingly large. Just assuming a normal distribution, or playing with the shape of the tails of the distribution, and asking simple questions of this sort can be illuminating. I'm often struck by the statement that "we don't care about the mean; we care about extremes" when these two things are so closely related (in the case of temperature). Uncertainty in the temperature response translates directly into uncertainty in changes in extreme temperatures in this fixed distribution limit. It would be nice if, in model projections, it was more commonplace to divide up the responses in extreme temperatures into a part due just to the increase in mean and a part due to everything else. It would make it easier to see if there was much that was robust across models in the "everything else" part. And it also emphasizes the importance of comparing the shape of the tails of the distributions in models and observations. Of course from this fixed-distribution perspective every statement about the increase in hot extremes is balanced by one about decreases in cold extremes.

The discussion of this topic is often confused by the fact that people are asking different questions. Suppose we consider days that exceed some fixed temperature T that is on the tail of the distribution of daily temperatures. If the mean temperature warms by δT, while holding the distribution about the mean fixed, this number could increase dramatically, depending on the shape of the distribution, even if δT is much smaller than the width of the distribution. In this case, the mean warming is contributing a small fraction of the temperature anomaly in these extreme warm events even though the probability of these events has increased a lot (see Otto et al 2012 for a discussion of the Russian heat wave along these lines). If we redefined our criterion for a very hot day by upping the criterion by the small amount δT we would go from a description of what is going on as one in which the "number of very hot days increases dramatically" to one in which "the number of very hot days does not change but they are on average

δT warmer":

$$P_{new}(T) >> P_{old}(T) = P_{new}(T + \delta T).$$

My gut reactions to these two descriptions of the same physical situation are rather different. The goal has to be to relate these changes to impacts (things we care about) to decide what our level of concern should be, rather than relying on these emotional reactions to the way we phrase things.

- **Increases in extreme precipitation that result from an increase in atmospheric moisture, this increase in turn resulting from the increase in saturation vapor pressure resulting from warming — without changes in the winds that are converging moisture into the region of interest during these extreme precipitation episodes**

  There is an important sense in which the increase in high precipitation events is more basic, and more robust, than the changes in the mean precipitation. Some expectations for the latter are discussed in Post 13-14 and include regions of increasing and regions of decreasing mean precipitation. Changes in extremely high precipitation events seem to be simpler — we expect them to increase nearly everywhere. It is precisely when one is strongly converging water into some region, creating a lot of precipitation, that the upper bound on the water vapor in the atmosphere comes into play most strongly. irrespective of what the time mean humidity is doing. If you think of the dominant term that is trying to increase water vapor mixing ratios q in regions of strong upward motion w as $-w\partial q/\partial z$, and assume that the atmosphere is saturated $q = q_s$ over some depth, then the rain rate would be determined by integrating

  $$w\partial q_s/\partial z = w(\partial q_s/\partial T)(\partial T/\partial z)$$

  over the layer within which condensation is preventing supersaturation. Since the saturation mixing ratio at a given pressure is just a function of temperature, and the temperature profile would be moist adiabatic, we have a straightforward null hypothesis connecting the warming and changes in these precipitation extremes, just as we do for temperature extremes.

  The figure at the top of the page, from Pall et al 2007 illustrates this nicely. Take each grid point in a GCM and create a histogram of daily precipitation. Look at the change in total precip above the p-percentile of precip values, for a particular scenario by the end of the 21st century. To create a smooth zeroth-order picture, sum the p-percentile precip at each point over longitude and then compute the fractional change in the precip amount — as a function of p and of latitude. I like this plot because of the way it distinguishes between the subtropics (where mean precip is decreasing) and subpolar latitudes (where the mean is increasing) — but it does have the disadvantage, if I am interpreting it correctly, that these averaged results are dominated by the high precip regions at that latitude. In subpolar latitudes, precip is increasing in both heavy and light precip events. In the subtropics there is an increase in very heavy precip events (above the 90-95th percentile of daily values) but a decrease when the rainfall values are light. It is the latter that is evidently causing the reduction in the mean, along with an increase in the frequency of dry days not evident in this plot. SREX (Ch. 3) has a summary of observations of trends in extreme precipitation and a lot of references.

- **Changes in the frequency or severity of storms or lower frequency climate anomalies, such as droughts, resulting from changes in atmospheric or oceanic circulations on large scales**.

  An example might be a poleward shift in the Atlantic storm track increasing the frequency of extreme wind and extreme surface wave events on the poleward flank, and decreasing these same extreme events on the equatorward flank of the storm track. These changes in extremes do not result from any subtle change in the underlying dynamics of the storms or waves — the robustness of the changes in extremes depends entirely on the robustness of the large-scale storm track shift.

  Another example is the constructive superposition of la Nina and global warming-induced drought over the southern tier of the continental US. Radiative forcing due to increased well-mixed greenhouse gases expands the subtropics and shifts the midlatitude storm tracks polewards in a variety of models of different levels of complexity. El Nino has the opposite effect, especially over and downstream of the Pacific, where it shifts the jet and storm track equatorwards. So the opposite phase of the ENSO cycle, la Nina events, tends to reduce precipitation especially in the southern tier of the continental US. See also this analysis by Bergman et al 2010 of the connections between Pacific ocean temperatures and medieval megadroughts. The la Nina response adds to the simulated effect of the greenhouse gases. See Lau et al 2008 for discussion related to this superposition. By the same token, some of the effects of El Nino events on North America due to the changes in atmospheric circulation might be ameliorated. Even if the meteorology turns out to be basically a linear superposition, impacts of various kinds — forest fires, agricultural, etc, — will remain a source of strong nonlinearities. It is the existence of these nonlinearities in impacts that makes this constructive interference for US drought between la Nina and warming important, even if the effects of warming on the ENSO variability itself turn out to be modest.

Finally, we have–

- **Changes in the intensity of storms.**

  There is a tempting hand-waving argument that storms will intensify because there would be more heat of condensation released in rising air, creating more buoyancy and stronger upward motion, but there are a variety of reasons why this is not a convincing argument. In any case, you have to distinguish between extratropical storms and tropical cyclones — these have such different dynamics that they present us with two very different sets of problems. My point here is just to emphasize that, as outlined above, there are reasons to expect changes in extremes that do not depend on these changes in storm intensity.

31 Relative Humidity in GCMs

[Originally posted September 1, 2012]

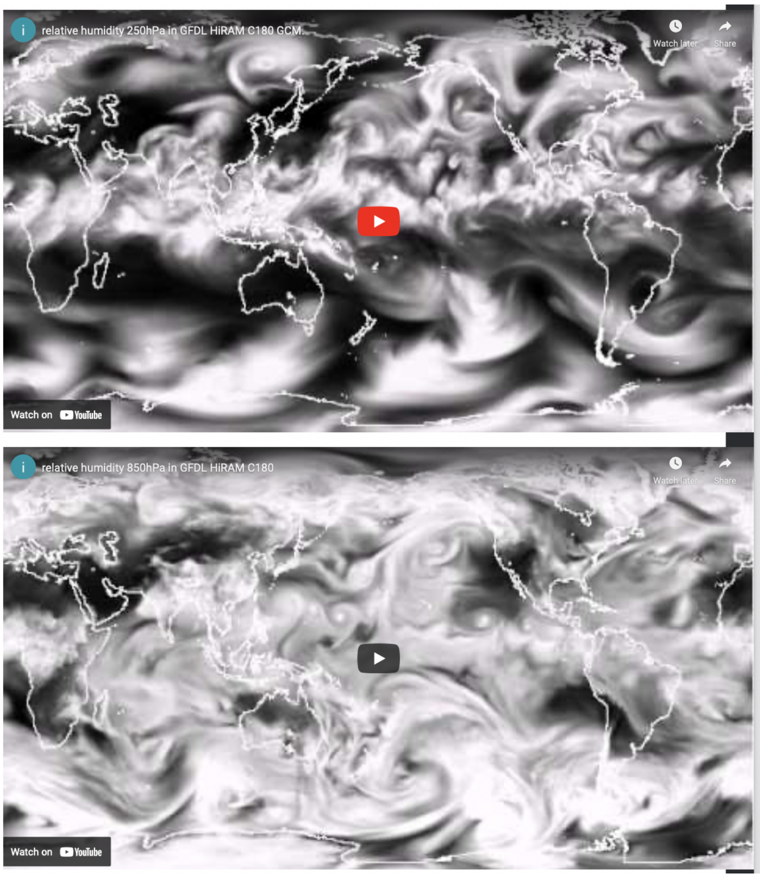


Figure 31.1: Snapshot of relative humidity in the upper (250hPa) and lower (850hPa) troposphere in an atmospheric model with 50km horizontal resolution.

See here and here for animation of evolution over one year.

In their 1-D radiative-convective paper of 1967, Manabe and Wetherald examined the consequences for climate sensitivity of the assumption that the tropospheric relative humidity (RH) remains fixed as the climate is warmed by increasing CO_2. In the first (albeit rather idealized) GCM simulation of the response of climate to an increase in CO_2, the same authors found, in 1975, that water vapor did increase throughout the model troposphere at roughly the rate needed to maintain fixed RH. The robustness of this result in the world's climate models in the intervening decades has been impressive to those of us working with these models, given the differences in model resolution and the underlying algorithms, a robustness in sharp contrast to the diversity of cloud feedbacks in these same models.

The animation above shows the evolution of RH on the 250hPa pressure surface in the upper troposphere and on the 850hPa surface in the lower troposphere in a GCM (this is the same 50km resolution model as discussed in several previous posts). The loop covers one year with each frame showing a daily mean (this makes the animation a bit jumpy unfortunately.) The brightest white is 100% relative humidity and darkest black 0%. Values less than 10% in the upper panel are common, in subsidence regions within the tropics or in stratospheric intrusions at higher latitudes. Air parcel trajectories cut though these pressure surfaces in complex ways and are difficult to visualize, these parcel trajectories being the key to understanding the resulting relative humidities.

These animations can be useful in discussions with those unfamiliar with GCMs, who might mistakenly think that RH is fixed by fiat in these models. The result that RH distributions remain more or less unchanged in warmer climates is an emergent property of these models. Is it possible to construct a GCM that keeps the amount of water vapor itself more or less unchanged as the climate warms, rather than roughly following the saturation vapor pressure? It would be nice to have such a model, which we could then analyze to see if it provides as convincing a simulation of other aspects of the atmospheric circulation as do our existing GCMs. But no one has constructed such a model to my knowledge.

GCMs do simulate modest changes in the distribution of RH in response to increasing CO_2. In fact, there is actually considerable similarity across models in the pattern of RH change that is simulated, primarily reflecting an upward stretching of the troposphere and poleward expansion of the subtropical dry zones. Fig. 31.2, from Sherwood et al 2010, shows the mean relative humidity response, per degree C global surface warming, in the CMIP3 models, using the idealized scenario of a 1%/year increase in CO2 and comparing the climate at the time of doubling to the control:

Shading means that 16 of the 18 models being averaged over agree on

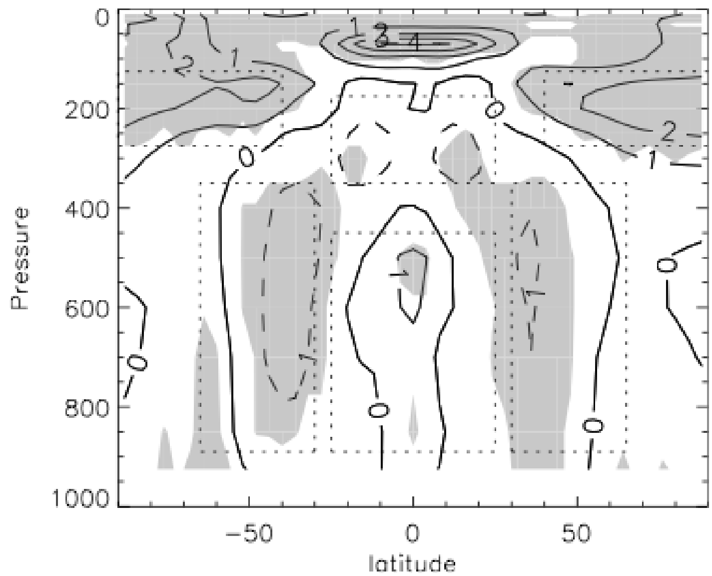


Figure 31.2:

the sign. Averaging different models together reduces the amplitudes of the changes seen in individual models, but highlights the robust part of these changes. You can look at the individual model results in the paper; individual models have larger amplitudes and more spatial structure in the pattern of the response,but they never approach the magnitude needed to compete with the temperature dependence of the saturation vapor pressure (more than 10%/C in the upper tropospheric regions of prime importance for water vapor feedback.)

Differences between the climatological RH in different models can be substantial, and the biases in these models compared to various observational estimates can be substantial as well (see, for example, the recent paper of Risi etal 2012 which also has quite a few references to other papers discussing these biases). Are these biases large enough to detract from our confidence in the robustness of the basic result that RH doesn't change that much with warming? The situation is similar to that described in Post 26 on high-resolution simulations of radiative-convective equilibrium in small domains — different models simulate very different RH distributions associated with differences in the way that the convection is organized, but each model when warmed hold its RH distribution nearly fixed because the convective organization is effectively unchanged.

One can try to shed light on this robustness in global models by turning to the fruit fly model that I discussed in Post 28 — a dry ideal gas atmosphere on a sphere forced by relaxing temperatures to a specified "radiative equilibrium" field and relaxing near surface winds to zero. Galewsky et al 2005 add a simple water-like passive tracer to this model — a tracer that does not interact with the flow or the temperatures. It just has a specified source at the surface ("evaporation") and a sink that exists only when the water vapor pressure rises above a saturation value that is a function of temperature, in which cases it just resets the vapor pressure to saturation, with the water vapor that disappears in this process thought of as "precipitation". The resulting time mean relative humidity is shown on the right. The left panel is just the mean simulation from the AR4 models lifted form the Sherwood et al paper.

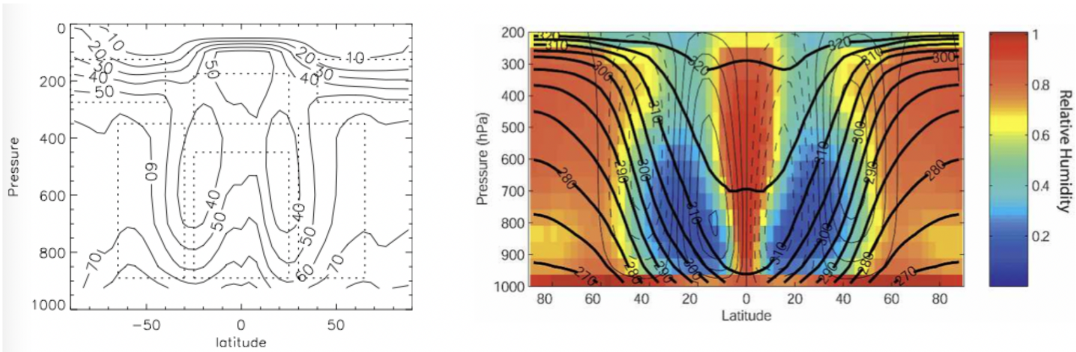


Figure 31.3:

In the figure on the right, the bold lines are the mean potential temperature, or isentropic, surfaces. Outside of the tropics, one can think of the air trajectories as tending to align along these surfaces. Also shown with lighter lines (harder to see) are the streamlines of the mean meridional circulation — the time and zonally averaged circulation in the latitude-height plane, indicating mean upward motion at the equator and downward motion in the subtropics.

The most obvious feature that this model captures qualitatively is the subtropical dry zones. Air parcels in these driest areas have either been carried down and warmed due to compression by the mean subtropical subsidence after losing most of their water in upward motion near the equator — or they have traveled down the midlatitude isentropic surfaces after having condensed most of their water during an earlier poleward and upward excursion. (The point of this paper was to think about how to quantify the relative importance of these two classes of trajectories.) Differences with the comprehensive models on the left are due in part to the absence of realistic boundary layer mixing spreading the evaporated water upwards, the absence of a seasonal cycle and monsoons that move subtropical dry zones and wash out the minima in the annual mean figure shown on the left, and the distortion of the vertical structure of the outflow from the tropical rising motion. (In the dry model, this outflow is spread over a broad layer of the troposphere, whereas in more realistic models with moist convection this outflow is confined more sharply to a layer near 200mb, causing the dry zone to be displaced upwards compared to the passive water model.) The bottom line is just that the atmospheric flow is what prevents this model atmosphere from becoming saturated everywhere – by wringing water out of parcels of rising/cooling air and then bringing these parcels back down so their relative humidity drops as they warm. The dry model with passive water may help in thinking about connections between the ensemble of particle trajectories and this dehydration.

32 Modeling Land Warming given Oceanic Warming

[Originally posted November 25, 2012]

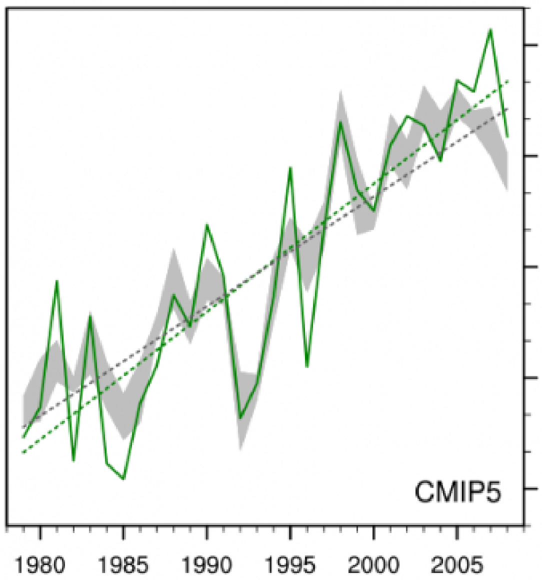


Figure 32.1: Anomalies in annual mean near surface air temperature over land (1979-2008), averaged over the Northern Hemisphere, from CRUTEM4 (green) and as simulated by an ensemble of atmosphere/land models in which oceanic boundary conditions are prescribed to follow observations.

As discussed in previous posts, it is interesting to take the atmosphere and land surface components of a climate model and run the resulting rediced model over sea surface temperatures (SSTs) and sea ice extents that, in turn, are prescribed to evolve according to observations. In Post 2 I discussed simulations of trend and variability in hurricane frequency in such a model, and Post 21 focused on the vertical structure of temperature trends in the tropical troposphere. A basic feature worth looking at in this kind of model is simply the land temperature – or, more precisely, the near-surface air temperature over land. How well do models simulate temperature variations and trends over land when SSTs and ice are specified? These simulations are referred to as AMIP simulations, and there are quite a few of these in the CMIP5 archive, covering the period 1979-2008.

The figure at the top summarizes the variation in the Northern Hemisphere mean surface air temperature over land in these CMIP5 AMIP runs. (The figures in this post were generated by my colleague Bruce Wyman.) We compute annual and hemispheric means from the monthly averages in the archive. We first average over all available realizations for each of 17 models. (We have left out two of our own models from this ensemble simply because we generated this figure to have something to compare our results with — adding a couple more models would have little impact on this figure.) The observations, in green, are taken from CRUTEM4. The model results are interpolated to the observational grid and the model results treated in the same way as the observations after that point (including discarding model results at grid points where monthly averaged data is missing.) Anomalies are computed, for each model and the observations, from the mean over the same 1979-2008 period. The shading in the figure indicates the middle half –the 25%-75% percentiles — of the resulting ensemble of values. (Sometimes it is important to focus on the model outliers and the full spread, but here we do the opposite and focus on the core of the model distribution.) The land warming trend in these models is about 15% smaller on average than the observed trend over this period. An example of a study that looks at land temperature trends in earlier AMIP simulations (but extending over the full 20th century) in this way is Scaife et al 2009 . I would like to see more work along these lines.

The figure below shows the same result for one of our models, the 50km resolution HiRAM model described in Posts 2 and 21. The shading means something different in this figure. We have three realizations of this model in the archive and the shading shows the spread across these three runs, so it gives you some feeling for the internal variability generated in this statistic by a model with prescribed ocean temperatures and sea ice. This atmospherically-generated internal variability is washed out by averaging over multiple realizations in the figure above. This particular model also underestimates the observed linear warming trend over this period by about 15%. (The grid in this model has the topology of a cube: the C180 in the figure indicates that there are 180×180 points on each face of the cube.)

I failed to mention that in these AMIP simulations, in addition to the observed variations in SST and sea ice, one also typically prescribes time-

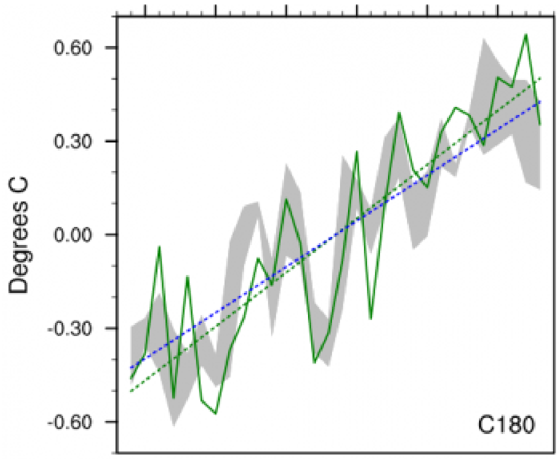


Figure 32.2:

varying "forcing agents"– well-mixed greenhouse gases, aerosols, ozone, solar cycle variations in incoming flux. In some AMIP models aerosol and ozone variations might be predicted, given emissions of precursors, but in the particular model that produces the results above these are all prescribed. (There are no interannual variations in land surface properties such as the type of vegetation in our model at all, and no urban heat island effects.) What happens if you keep all of these forcing agents fixed and vary only the lower boundary condition – the SST and sea ice. The figure below shows what you get from three realizations of this type in the same model. This tells you how much of the land temperature variation and trend is "forced" by the observed changes in ocean boundary conditions versus changes in the forcing agents themselves. In this model, the warming trend over Northern Hemisphere land is reduced by about 30% when holding these forcing agents fixed. Assuming that this is a linear superposition, 70% of the model trend is generated by the communication of the observed oceanic warming to the land.

You have to be a little careful in interpreting this decomposition. Part of the SST and sea ice variation is itself due to the changing forcing, of course. But there are still important things one can learn by comparing this kind of simulation with observed land warming. Suppose that all of of the land warming is just communicated from the ocean, with no direct dependence on forcing agents. Then one can use this fit to analyze what one might call the degree of redundancy of the land temperature record. I use the word redundancy with some reluctance, because it has the connotation of irrelevant whereas I actually mean just the opposite. Redundant climate

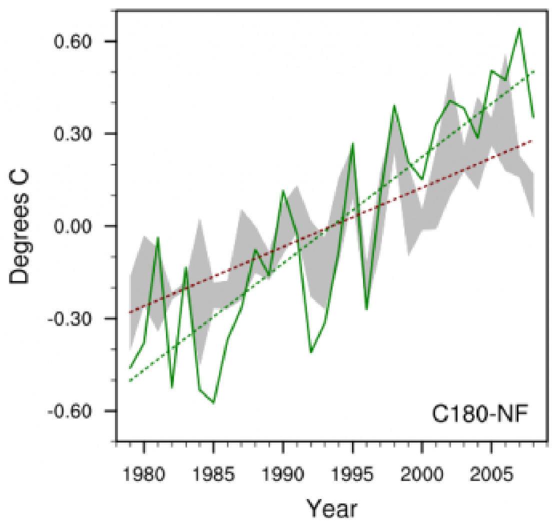


Figure 32.3:

records are precisely what we need!

On the other hand, to the extent that one can isolate the directly forced component, one can try to use it in attribution studies aimed at seeing whether or not a particular model has, say, the right mix of greenhouse gas and aerosol forcing. For this purpose the hemispheric mean doesn't give us too much to work with., but there is a lot more information than this in the spatial and seasonal structure of this directly forced component. In particular, one can increase the amplitude of this component by focusing on regions, such as Central Asia, where the oceanic influence is weaker. But it also helps to focus on those regions and times of year when internal (atmospherically-generated) variability is at a minimum (ie summer).

This kind of decomposition of land temperature trends has not received a lot of attention. There are more papers that use AMIP simulations to attribute trends in the atmospheric circulation in this way, such as Deser and Phillips 2009. It would be helpful if this kind of decomposition were available for multiple models in the CMIP5 archive. (An earlier paper that introduces the use of AMIP simulations for detection/attribution studies that I was not aware of until recently is Folland et al 1998. It would, in particular, be useful to know how robust the spatial and seasonal structure of the fixed "forcing" component is — the more robust this component, the more likely that one can subtract it cleanly from the observed variations and use the remainder to constrain forcing or sensitivity, at least over land.

The value of these AMIP simulations is that one can look in much more detail at the time evolution of the discrepancy between model and observations than is possible when working with a fully coupled model. Consider, for example, the difference between the models' and CRUTEM4 values in the last few years of this period. Is this due to problems with the land observations, the SSTs and sea ice driving the atmospheric model (we use HADISST), or the models themselves? One interesting point, which I also failed to mention above, is that when one prescribes sea ice in these kinds of AMIP simulations one often just varies ice extent and not thickness, due to the lack of an observational basis for prescribing thickness. (In contrast, fully-coupled climate models invariably try to simulate thickness variations directly). Could these AMIP models be missing some warming over land due to this deficiency, especially in the last few years of the simulations?

33 Can We Trust TC Statistics in Global Models

[Originally posted December 14, 2012]

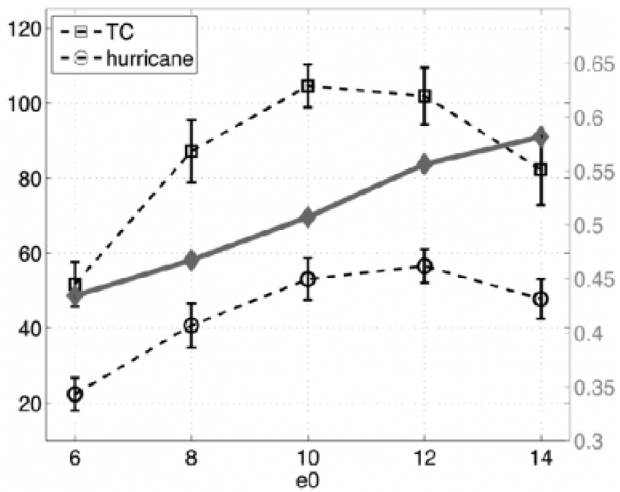


Figure 33.1: Globally integrated, annual mean tropical cyclone (TC) and hurricane frequency simulated in the global model described in Post 2, as a function of a parameter in the model's sub-grid moist convection closure scheme, from Zhao et al 2012.

It is difficult to convey to non-specialists the degree to which climate models are based on firm physical theory on the one hand, or tuned (I actually prefer *optimized*) to fit observations on the other. Rather than try to provide a general overview, it is easier to provide examples. Here is one related to post 2 in which I described the simulation of hurricanes in an atmospheric model.

In that post you can find an animation of the model output and some comparisons with observations. Here's a reprise of the figure on the seasonal cycle of hurricane frequency in the different ocean basins This version of the

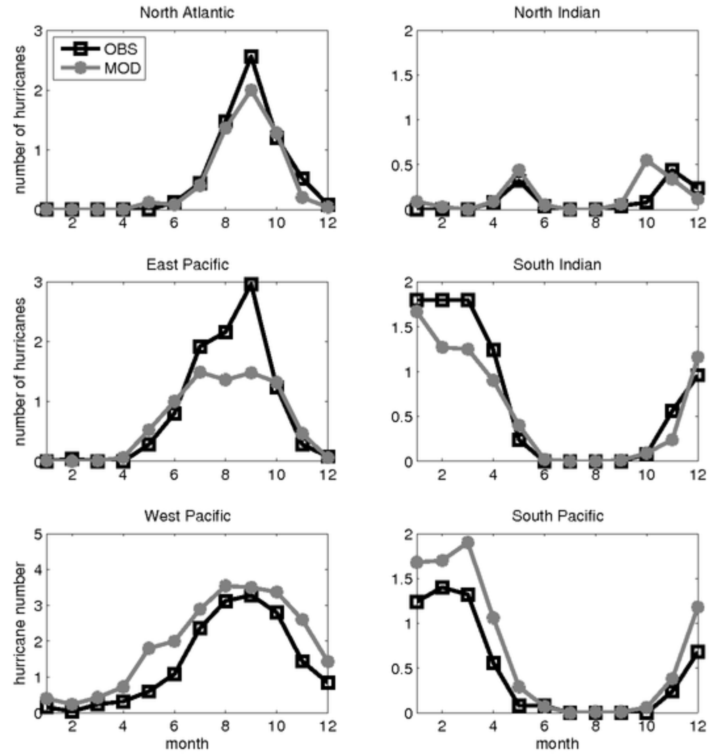


Figure 33.2:

model, which has about 50km horizontal resolution, seems to do a very good job at simulating the frequency of tropical cyclones – (max winds ¿ 17m/s) and the fraction of these storms of hurricane strength (¿ 33 m/s), but does not simulate very strong (cat 3-5) storms, although the intensity distribution looks better if you look at minimum pressure rather than maximum winds. I have been impressed by the quality of this simulation and similar simulations in other models. We also have a 25 km version that produces quite similar results. Yet many in the tropical cyclone research community remain skeptical that a model with 25-50km grid size can simulate the physics of TC formation.

When we first described these results in Zhao et al 2009 we knew very little about their sensitivity to model parameters. We still don't, because the model is computationally expensive. Why work with a model that is so resource-consuming? It's a tension that is always present in climate modeling: do you use increasing computer resources to create higher resolution models with the idea that improvements in the simulation will make the higher computational burden worthwhile, or do you stop with a more modest model that allows you to vary parameters systematically? If one is interested in phenomena that are difficult to resolve in typical global models, such as tropical cyclones. the choice is pretty obvious — you need to push the resolution to build a case for the credibility of the simulations.

A more recent paper Zhao et al 2012 describes the sensitivity of TC and hurricane frequency in this model to two parameters — one of these is shown in the figure at the top. The parameter e_0 is part of the sub-grid closure scheme for moist convection in the model. The plot shows the average number of TCs per year on the whole globe (one of the dashed lines), as well as the number of TCs of hurricane strength (the other dashed line). For convenience it also redundantly shows the fraction of TCs that are of hurricane strength (solid line with scale on the right). We run a 20 year simulation for each of 5 values of e_0; the "error bars" are the standard deviation of the 20 yearly values . As e_0 increases the total number of TCs and hurricanes first increases and then decreases. The fraction of TCs that reach hurricane strength, a crude measure of average intensity, increases monotonically with e_0.

In the tropics a lot of the vertical transport takes place in plumes generated by moist gravitational instability that extend from near the surface to just beneath the tropopause. The dominant horizontal scale of these plumes might be of the order of one or a few kilometers — although direct simulation of the turbulent entrainment into and detrainment out of these plumes, which affects their buoyancy, requires still smaller scales. If you don't have a sub-grid scale convection scheme in your model, "plumes" will still occur but in a distorted way on the scale of the model grid. In reality convection occurs even though the average conditions over, say, a 50 km square are not conducive to the generation of gravitational instability — due to spatial variability on smaller scales. Closure schemes for moist convection are based on an explicit or implicit picture of what is going on within a grid box that determines if convective plumes are triggered, how much mass is transported to the upper troposphere within the plumes, etc. In the case of the closure scheme used here, when e_0 is small deep convection occurs relatively easily; when it is large the convection is more inhibited.

It happens that the value we chose to use was $e_0 = 10$, close to the value that produces the maximum number of TCs. The main reason for this choice was the model's top-of-atmosphere energy balance, which is sensitive to e_0, varying by more than $10W/m^2$ over this range of values, due mostly to changes in low cloud. It is hard to find other ways of counteracting such large changes to rebalance the model. And the model becomes quite noisy on the grid scale at the higher values of e_0 examined. It was these considerations, rather than systematic examination of storm statistics vs. e_0, on which the initial choice of this parameter value was based. The

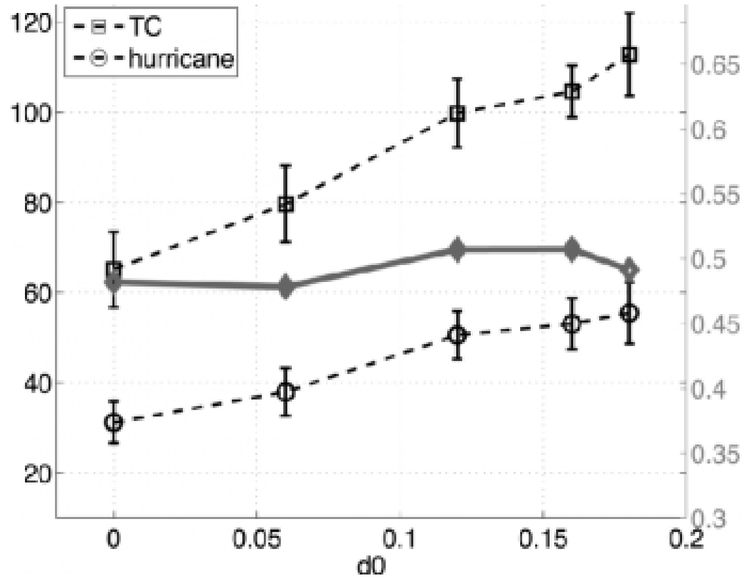


Figure 33.3:

other parameter we have examined directly controls the grid-scale noise in the model, especially in the tropics. (The horizontal flow on each model level can be decomposed into rotational and divergent components — the parameter d_0 controls the strength of the damping of the divergent component only, which affects the flow primarily in the tropics.)

As d_0 increases this damping of small scales increases and one might expect the number of TCs, which after all are only marginally resolved by the grid, to decrease, But the opposite occurs — the number of TCs increases as the small scale damping increases in strength. The intensity as measured by the fraction of TCs that become hurricanes stays about the same. In fact it is hard to find anything in the simulation that is affected by this parameter other than the number of TCs that the model generates — and explicit measures of how noisy the model tropics is close to the grid scale. Our interpretation of this result is that it is the competition for a resource (the evaporation of water at the surface) that is the key — if you have too may little nascent disturbances trying to grab their share it becomes difficult for vortices of TC strength to form. We think that this dependence on the noise level is also responsible for the reduction in storm counts at large e_0. (Other effects are dominant at small e_0.)

These dependencies are still under investigation. But it should be clear that the kind of results displayed in Post 2 are not entirely "first principles" simulations of TC statistics, and the picture could change as we move to finer and finer resolution, especially to the point of resolving some of the deep plumes dominating moist convective turbulence in the tropics. Are we justified in using this model as a tool to ask how hurricane statistics respond to warmer SSTs/increasing greenhouse gases?

I put a lot of weight on results such as the seasonal cycle figure above. The simulations hold together remarkably well. Nothing has been done to try to tune these seasonal cycles. I don't know how to quantify my level of confidence based on the quality of the simulations, but I would argue that tropical cyclone projections with this class of model should be taken seriously despite legitimate concerns about dependence on the treatment of sub-grid scale processes.

34 Summer Temperature Trends over Asia

[Originally posted December 31, 2012]

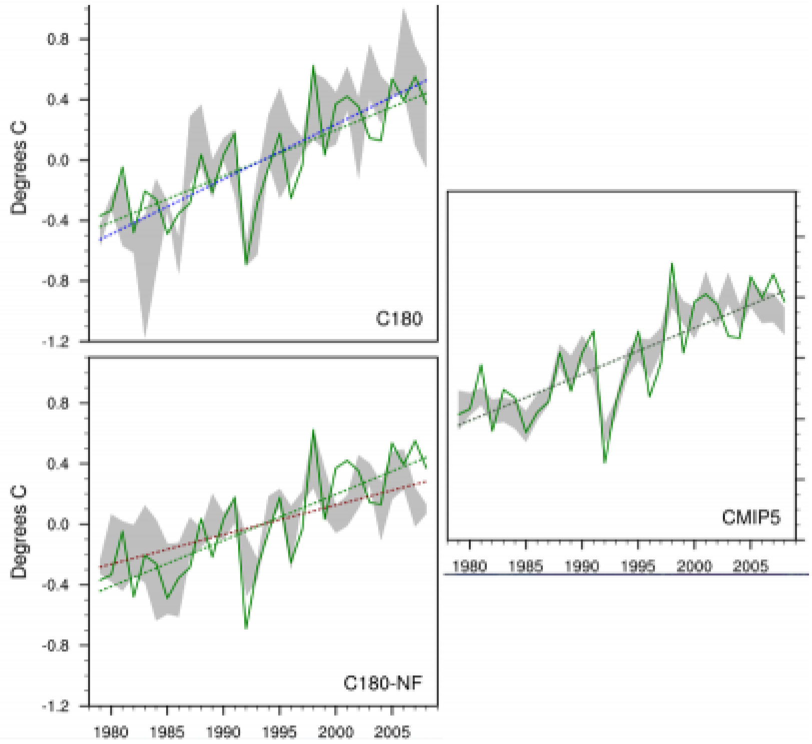


Figure 34.1:

This is a follow up to Post 32 on Northern Hemisphere land temperatures as simulated in models in which sea surface temperatures (SSTs) and sea ice extent are prescribed to follow observations. I am interested in whether we can use simulations of this "AMIP" type to learn something about how well a climate model is handling the response of land temperatures to different forcing agents such as aerosols and well-mixed greenhouse gases. If a model forced with prescribed SST/ice boundary conditions and prescribed variations in the forcing agents does a reasonably good job of simulating observations, we can then ask how much of this response is due to the SST variations and how much is due to the forcing agents (assuming linearity). If the response to SST variations is robust enough, we have a chance to subtract it off and see if different assumptions about aerosol forcing, in particular, improve or degrade the fit to observations.

In post 32, the focus was on the annual mean land temperatures averaged over the Northern Hemisphere. If you look at the model simulations in different seasons, in this prescribed SST/ice context, you see a lot more variation from realization to realization in winter than in summer due to internal atmospheric variability. If we are interested in confronting models with the observed spatial structure of trends over land, it helps to look at the system in such a way as to minimize the influence of internal variability. Focusing on summer is one way to get started. The idea to focus on Asia in addition is partially to try to maximize the influence of the forcing agents as opposed to the SST influence, but our AMIP simulations suggest that the spatial structure of the summertime trends is also less noisy over Asia than over N. America. There are more systematic ways of doing this, needless to say, but I think looking at Asia in summer might be a good way to get a feeling for whether this is worth pursuing.

The figure at the top of this post shows the spatial mean of 2m land temperatures in June-July-August over Asia (35E-170W, 7N-83N) from CRUTEM4 in green. As in Post 32, the AMIP simulations from 17 models in the CMIP5 archive are first averaged over all available realizations for each model, and anomalies are computed from the mean over the 1979-2008 period. The shading in the figure on the right shows the 25%-75% range of this ensemble of model anomalies.

The figure on the upper left shows the result from one of our atmosphere/land models, HiRAM C180. To confuse matters the shading here indicates the spread among three realizations, a measure of internal variability. The figure on the lower left is generated with the same model but including only SST and ice extent variations, holding the forcing agents fixed.

The temperature trends in the CMIP5 ensemble are very close to the observational estimate, while the C180 model's trend (upper left) is a bit high — one anomalously cold season, in '83 near the start of this time series, seems to be partly to blame. With forcing variations removed (lower left), the C180 model trend is reduced by more than 40%.

It is nice to see the cooling due to Pinatubo appear so clearly in the summer of '92 (the eruption was in June '91). It is well simulated by the models without any additional smoothing or removal of ENSO effects.

Of course, some of this is coming from the observed SST response, which is imposed here. From the "attribution" implied by the figures on the left above, it seems that about 1/2 to 2/3 of the cooling over Asia in the summer of '92 is communicated from the ocean, the rest coming directly from the volcanic forcing.

Putting the summertime trends over Asia in a global context, here is the observed spatial pattern of 30 year June-July-Aug trends as well as the results from the mean of the CMIP5 ensemble and the mean of our 3 C180 realizations.

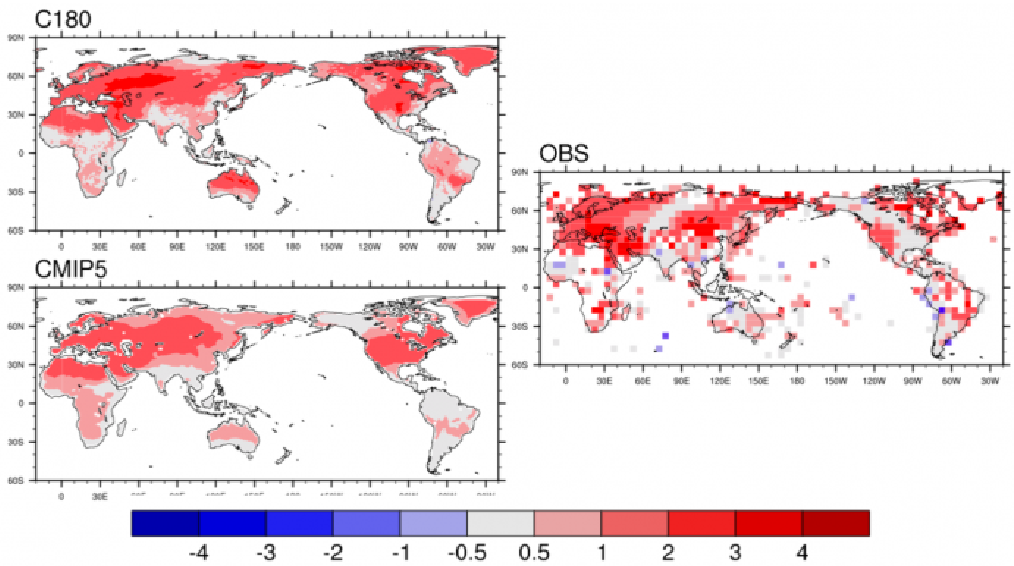


Figure 34.2:

The numbers on the color scale are in degrees C/30 yrs. In the OBS=CRUTEM4 plot, white means that we felt that too much data was missing to compute a trend; gray, as in the other plots, means that the absolute value of the trend is less than 0.5C/30yrs. The relatively small summer trends in this 30 year period in the eastern half of the US and in a curious slash through central Asia are some of the interesting discrepancies between these observational estimates and the mean of these model ensembles.

Decomposing the C180 model result over Asia into a part due to the SSTs and sea ice alone and the remainder, which we interpret as due to varying forcing agents, we get this: I have focused this plot on Asia because that seems to be where the model decomposition is particularly robust across the different realizations. (The analogous figure over N. America seems to be more strongly distorted by sampling of internal atmospheric variability.)

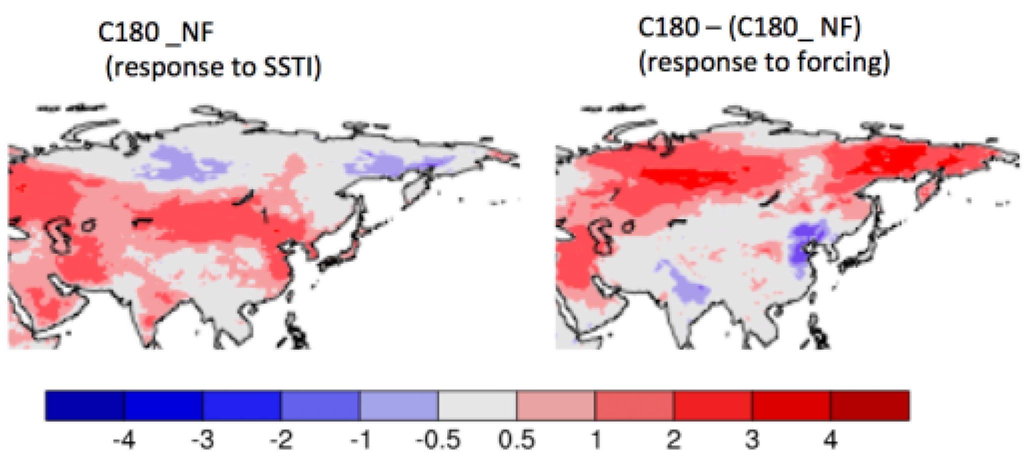


Figure 34.3:

We need more realizations to look at this decomposition carefully across the globe and to test the assumption of linear superposition. It is interesting that the response to SSTs and sea ice extent has weak cooling in Northern Asia. Over the Arctic in summer near surface temperatures are tightly constrained to be close to the melting temperature of sea ice, so to the extent that diffusion from the Arctic is relevant this would tend to minimize the warming in adjacent land, but this mechanism would not produce cooling. Cooling is most likely related to moistening of the soil, perhaps due to increase in Spring snowfall or summer rains. To what extent is this pattern robust across models? (A number of other modeling centers have simulations relevant for this kind of decomposition but they are not in the CMIP5 archive.)

The remainder (on the right in the figure), which we interpret as due to forcing variations, is also interesting. The cooling or lack of warming over the southern half of the continent is presumably due in large part to aerosols. I don't think Pinatubo torques the trend very much since it occurs close to the middle of this time period, but that needs to be checked. There does seem to be some potential for constraining aspects of the anthropogenic aerosol forcing with this approach.

35 Atlantic Multi-decadal Variability and Aerosols

[Originally posted February 15, 201]

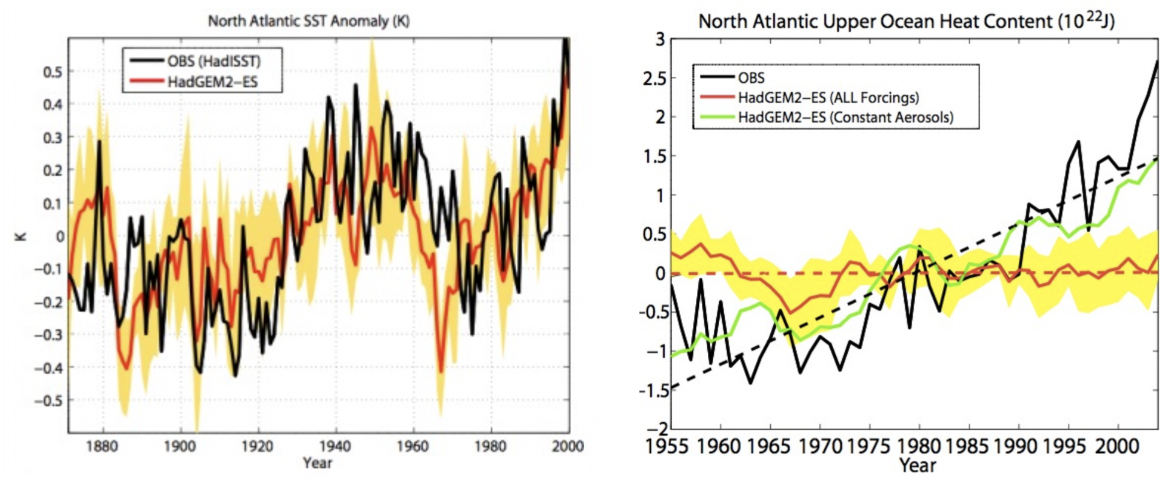


Figure 35.1: (Left) Sea surface temperature averaged over the North Atlantic (75-7.5W, 0-60N), in the HADGEM2-ES model (ensemble mean red; standard deviation yellow) compared with observations (black), as discussed in Booth et al 2012. (Right) Upper ocean (¡ 700m) heat content in this model averaged over the same area, from Zhang et al 2013 (green = simulation with no anthropogenic aerosol forcing, kindly provided by Ben Booth.)

A paper by Booth et al 2012 has attracted a lot of attention because of the claim it makes that the interdecadal variability in the North Atlantic is in large part the response to external forcing agents, aerosols in particular, rather than internal variability. This has implications for estimates of (transient) climate sensitivity but it also has very direct implications for our understanding of important climate variations such as the recent upward trend in Atlantic hurricane activity (linked to the recent rapid increase in

N.Atlantic sea surface temperatures) and drought in the Sahel in the 1970's (linked to the cool N. Atlantic in that decade). I am a co-author of a recent paper by Rong Zhang and others (Zhang et al 2013) in which we argue that the Booth et al paper and the model on which it is based do not make a compelling case for this claim.

The interest results from the figure in the left panel above. This model's forced response agrees very well with the observed surface temperatures averaged over the North Atlantic, so in this model one doesn't need to invoke internal multidecadal variability to match these observations. (The forced response is estimated by averaging over multiple realizations of the model with different initial conditions). Zhang et al list several aspects of this simulation that seem problematic, exemplified by the upper right panel, which shows a time series of the ocean heat content down to 700m over this same region. (observations from Levitus 2009). The model does not produce the upward trend in this N. Atlantic heat content. If one removes the anthropogenic aerosol forcing from the model (green line) it fits these observations better.

The flatness of the heat content in the N. Atlantic in this model is intriguing. Based on discussions with my colleagues Rong Zhang and Mike Winton, this seems to be a consequence of an AMOC (Atlantic Meridional Overturning Circulation) which builds in strength when the aerosol cooling is strong, trying to balance a part of the cooling at the surface with warm waters advected in from the tropics, but also — by a process that is not particularly straightforward — cools the subsurface waters.

Another problematic aspect of the N.Atlantic simulation is the co-variability of temperature and salinity. Decadal scale temperature and salinity variations in the subpolar Atlantic tend to be positively correlated in observations. In particular, the cold period in the 70's was marked by a fresh subpolar Atlantic. This is what one expects when the AMOC is weak, with less transport of more saline waters from the subtropics and more export of fresh waters from the Arctic. The model does not show this correlation, and in the 70's it has relatively high salinity (presumably due to the stronger AMOC mentioned in the previous paragraph). Our understanding of AMOC variability is admittedly limited, but the temperature-salinity correlations point towards there being a substantial internal component to the observations. These Atlantic temperature variations affect the evolution of Northern hemisphere and even global means (e.g. Zhang et al 2007). So there is danger in overfitting the latter with the forced signal only.

Our lab has a model, CM3 (Donner et al 2011), that also has strong indirect aerosol effects and that produces simulations of the past century that share many of the features of HAD-GEM2-ES discussed here, including the nice fit to the N. Atlantic SSTs. So this issue is naturally a hot topic of conversation in our lab. The issue has been around for a while. For example, Rotstayn and Lohmann 2002 made a case that strong aerosol forcing could explain the Sahel drought of the 70's by cooling the N. Atlantic. The same qualitative behavior is seen in many models, but we are left with the quantitative question of how big the aerosol effect is.

Differences of opinion make life interesting and always force us to sharpen our arguments. And there remain strong differences of opinion on the relative importance of AMOC variability and aerosol forcing for the non-monotonic variation of North Atlantic surface temperatures and all the phenomena that we think are affected by it (including hurricanes and African rainfall). But I remain skeptical that one can make a compelling case for aerosol dominance by focusing only on SSTs, without simultaneously considering salinities and sub-surface temperatures that are better able to distinguish between forced and free variations.

(conversations with Rong Zhang, Mike Winton, and Yi Ming have helped me think about this issue)

36 A Diffusive Model of Atmospheric Heat Transport

[Originally posted April 10, 2013]

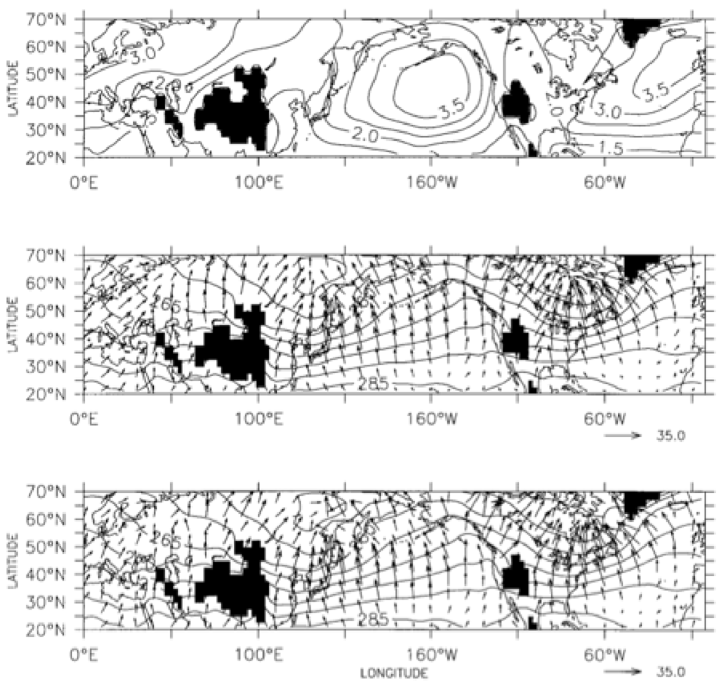


Figure 36.1: Lower panel: the observed (irrotational) component of the horizontal eddy sensible heat flux at 850mb in Northern Hemisphere in January along with the mean temperature field at this level. Middle panel: a diffusive approximation to that flux. Upper panel: the spatially varying kinematic diffusivity (in units of $10^6 m^2/s$ used to generate the middle panel. From Held 1999 based on Kushner and Held 1998

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Let's consider the simplest atmospheric model with diffusive horizontal transport on a sphere:

$$C\partial T/\partial t = \nabla \cdot C\mathcal{D}\nabla T - (A + B(T - T_0)) + \mathcal{S}(\theta). \tag{36.1}$$

Here $\mathcal{S}(\theta)$ is the energy input into the atmosphere as a function of latitude θ, $A+B(T-T_0)$ is the outgoing infrared flux linearized about some reference temperature T_0, C is the heat capacity of a tropospheric column per unit horizontal area $\approx 8 \times 10^6 J/(m^2K)$, and $\mathcal{D}$ is a kinematic diffusivity with units of $(length)^2/time$. Think of the energy input as independent of time and, for the moment, think of $\mathcal{D}$ as just a constant.

We can choose T_0 to be the steady state global mean temperature in some control climate and reinterpret the temperature as the departure from this reference so that

$$\mathcal{S}(\theta)/C = -\mathcal{D}\nabla^2 T + (B/C)T \tag{36.2}$$

If we are using this equation to model the time averaged north-south temperature gradients we can think of $\mathcal{S}(\theta)$ as the absorbed solar flux with its global mean removed. But the equation is linear and we can also think of it as modeling the temperature response to some perturbation in the energy input, for example that due to aerosol forcing or changes in ocean heat uptake or ocean heat redistribution.

We can talk about an atmospheric radiative relaxation time scale, $\tau_R \equiv C/B$ — which might be 45 days or so if we choose $B = 2W/(m^2K)$ for example — and a diffusive time scale for temperature variations on the length scale $\mathcal{L}$ of $\tau_D \equiv \mathcal{L}^2/\mathcal{D}$. For a diffusivity of $d \times 10^6 m^2/s$, which we'll see is the order of magnitude of interest, the two time scales would be equal for $\mathcal{L} \approx 2\sqrt{d} \times 10^6 m$, or about $20\sqrt{d}$ degrees of latitude. Let's call this length scale $\mathcal{L}_C$. The atmospheric response to perturbations on scales smaller than $\mathcal{L}_C$ would be spread over the distance $\mathcal{L}_C$ in this model. If the ocean redistributes heat from latitude A to latitude B, and if A and B are within $\mathcal{L}_C$ of each other, we might expect the atmospheric transport to closely compensate for this oceanic transport; if the heating and cooling are more widely separated than $\mathcal{L}_C$, the heating/cooling will be balanced more by radiation to space with atmospheric transport playing less of a role.

The bottom panel in the figure at the top is the eddy sensible heat flux, $c_p\overline{\mathbf{v}'T'}$, in January at 850 hPa, in the lower troposphere but above the planetary boundary layer, where $\mathbf{v}$ is the horizontal wind and a prime denotes the deviation from the mean seasonal cycle — computed from 4 times daily NCEP-NCAR reanalysis. The overline is a time average over all Januarys. Most of this flux is associated with midlatitude storms. Also shown by the contours is the mean temperature field for that month. The black splotches are where the surface protrudes above this pressure surface.

(Actually, before plotting the flux, we decompose it into a a part that has zero divergence on this surface and a part that has zero curl –this Helmholtz decomposition is unique on the sphere– and retain only the latter part, since we are only interested in the divergence of the flux here. If you don't do this, the flux is not as cleanly directed downgradient.)

The fluxes in the middle panel are generated with the same mean gradients and with the spatially varying diffusivity shown in the upper panel. The result is evidently in the right ballpark. The kinematic diffusivity has the dimensions of $(length^2/(time)$, or velocity times length. One could try to develop a theory for the relevant length and time scales or one could estimate them from observations in different ways. Here we do the latter, and take the shortcut of just looking at the streamfunction of the flow. The atmospheric flow is approximately non-divergent in the horizontal, so can be described by a streamfunction ψ . (Ignoring spherical geometry, the rotational zonal (eastward) component of the wind u and meridional (poleward) component v are related to ψ by $(u, v) = (-\partial\psi/\partial y, \partial\psi/\partial x)$.) So ψ has units of velocity times length, the same as kinematic diffusivity. We compute the standard deviation of the eddy streamfunction, $\sigma \equiv \sqrt{\overline{\psi'^2}}$ and allow ourselves a single constant of proportionality that provides the best fit of the form $\overline{\mathbf{v}'T'} = -\alpha\sigma\nabla T$ where α is uniform in space. (The plot uses $\alpha = 0.34$.) This may seem a bit arcane, but it is just a way to avoid having to estimate length and time scales separately. This approach was motivated by Holloway 1986, who used this same procedure with satellite data of sea level fluctuations (sea level is proportional to the streamfunction of a geostrophic current) to estimate horizontal transport due to ocean eddies.

A fascinating question for me, ever since I entered the field, is how the magnitude and structure of this diffusivity is determined. (In Held 1999, I discuss why turbulent diffusion might actually be a better approximation for the atmosphere, at least for the transport of sensible heat in the lower troposphere, than for typical shear or convectively driven turbulence studied in the laboratory.) We expect this effective diffusivity to change as the climate changes, since the diffusivity must be determined by some aspect of the large-scale environment giving rise to these storms. In particular, most theories have this diffusivity increasing with the magnitude of the north-south temperature gradient, making it harder to change this gradient than one might otherwise guess.

The values of the diffusivity in the middle of the oceanic storm tracks rise above $\approx 3x10^6 m^2/s$. It is the large value in midlatitudes, where north-south temperature gradients are strongest, that are most important for understanding the mean equator-to-pole temperature difference on Earth. A value of $d \approx 2-3$ is more or less what you need in this simple diffusive model to get reasonable north-south temperature profiles (see North et al 1981), depending on the vertical level at which you think it's most appropriate to diffuse the temperature field. From the previous discussion, we get the sense from this simple diffusive picture that north-south heat transport couples different latitudes within the same hemisphere rather strongly. In addition to the effective turbulent diffusivity, which is a key to north-south transport, there are strong zonal winds mixing even more strongly in longitude within a hemisphere. Too local a perspective is a common mistake when first being exposed to the climate change problem — ie, expecting the temperature response to reflect the spatial structure of the CO_2 radiative forcing or of the water vapor feedback..

But my motivation in bringing up this topic is a concern about the opposite tendency to ignore the difficulty that the atmosphere has in communicating temperature responses from extratropical latitudes of one hemisphere to extratropical latitudes of the other. A diffusivity of $2-3 \times 10^6 m^2/s$, if uniform over the sphere, is not large enough to mix from pole to pole in an atmospheric radiative relaxation time. The effective diffusivity gets small as one enters the tropics — one can see a bit of this reduction in the figure — seemingly making it harder still to communicate between hemispheres, but this is potentially misleading because the large scale overturning (the "Hadley Cell") is very efficient at destroying temperature contrasts across the tropics. This effect is sometimes mimicked in diffusive models by using a large diffusivity in the tropics, which can be confusing since this diffusivity would not be relevant for passive tracers. In addition the strong tendency for the tropical circulation to wipe out horizontal temperature gradients applies to deep temperature perturbations in the free troposphere, from which the surface can be protected by structure in the atmospheric boundary layer. In any case, the signal still has to move through the tropics, which provide a large area to radiate it away to space, so the difficulty in getting much of a signal to reach extratropical latitudes in the opposite hemisphere remains. GCMs provide an essential tool for navigating this complexity. (But uncertain cloud feedbacks, the familiar wild card when discussing global sensitivity, can also come into play in this problem.)

When thinking about aerosol forcing, which is heavily tilted to the Northern Hemisphere, no one is surprised if the response is strongly tilted to the Northern Hemisphere as well. But consider the concept of (global mean) transient climate response (TCR), discussed in several earlier posts.

The TCR is dependent on the efficiency of heat uptake by the oceans. Much of this heat uptake occurs in the North Atlantic and in the Southern Ocean. Consider two models, identical except for the Southern Ocean heat uptake. The one that warms more slowly in the Southern Ocean will have a smaller TCR, which is fine, but would the warming in the extratropical Northern hemisphere be substantially smaller? I don't think so.

A paper by Stouffer 2004 (Fig 5 in particular) is informative. This paper describes very long simulations of the response to doubling and halving of CO2 in a coupled atmosphere-ocean model (5,000 years — long enough for this model to approach its new equilibrium quite closely). In the $2 \times CO_2$ case at year 200 the Southern Hemisphere (SH) as a whole, held back in large part by the Southern Ocean, has reached about 40% of its final temperature response. Meanwhile the Northern Hemisphere (NH) has achieved over 80% of its equilibrium response. Even if all of the NH disequilibrium is due to the lack of warming in the Southern Hemisphere, which is unlikely, there is little room left for the rest of the SH warming to affect the NH — implying that a change in the SH relaxation time would have only a small effect on the NH in this model.

Thinking in terms of the global mean temperature in isolation can be valuable and it can also be misleading. I tried to argue in Post 7 that neither of the usual arguments for focusing on the global mean — reduction in noise and the connection to the global mean energy balance — is very compelling. (To think about one way in which the energy balance can get divorced from the mean temperature, just make B in this simple diffusive model a function of latitude.) It is seductive to focus on the global mean temperature response; whenever I do I have to continually remind myself not to be misled into thinking that the Northern and Southern Hemispheres, in particular, are more strongly coupled than they actually are.

(Thanks to Sarah Kang, Paulo Ceppi, Yen-Ting Hwang and Dargan Frierson for discussions on closely related topics.)

37 Tropical Rainfall and Inter-Hemispheric Heat Transport

[Originally posted May 16, 2013]

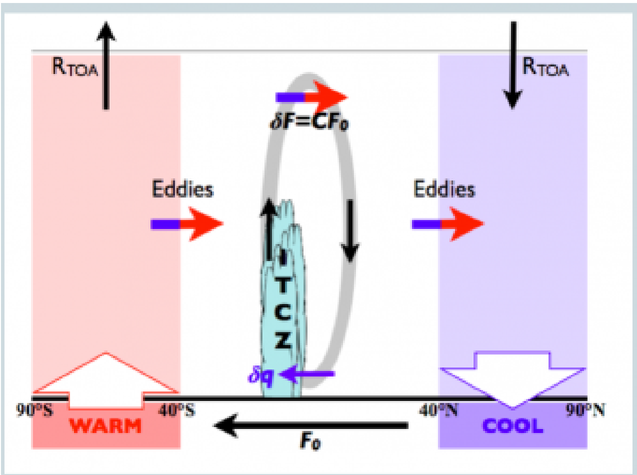

Figure 37.1: Schematic of the response of tropical rainfall to high latitude warming in one hemisphere and cooling in the other or, equivalently, to a cross-equatorial heat flux in the ocean. From Kang et al 2009.

When discussing the response of the distribution of precipitation around the world to increasing CO2 or other forcing agents, I think you can make the case for the following three basic ingredients: 1) the tendency for regions in which there is moisture convergence to get wetter and regions in which there is moisture divergence to get drier ("wet get wetter and dry get drier") in response to warming (due to increases in water vapor in the lower troposphere — post 13); 2) the tendency for the subtropical dry zones and the mid-latitude storm tracks to move polewards with warming; and 3) the tendency for the tropical rainbelts to move towards the hemisphere that warms more.

There are other important elements we could add to this set, especially if one focuses on particular regions — for example, changes in ENSO variability would affect rainfall in the tropics and over North America in important ways . But I think a subset of these three basic ingredients, in some combination, are important nearly everywhere. I want to focus here on 3) the effect on tropical rain belts of changing interhemispheric gradients.

My exposure to this issue started some time ago listening to Suki Manabe discussing early coupled atmosphere-ocean model simulations in which the latitude of the Pacific intertropical convergence zone (ITCZ) was sensitive to the cloud cover in midlatitudes of the Southern Hemisphere — these were the days in which cloud cover was prescribed in the models so it was easy to manipulate. By increasing the cloud cover in the South, well away from the tropical rainbelts themselves, one could move the ITCZ from south of the equator to north of the equator (where it is in reality).

In the late 80's a flurry of work on Sahel rainfall and particularly the severe drought in the preceding decade, starting with Folland et al 1986, argued that much of the decadal variability in the Sahel is tied to the differential warming of the hemispheres. Relatively cool Northern Hemisphere, as in the 70's, results in less Sahel rainfall, thinking of the Sahel as marking the northernmost extension of the ITCZ, or monsoonal rainfall, over Africa, which retreats due to the pull of the differential cooling of the Northern with respect to the Southern Hemisphere. While there are other things going on in the Sahel, most recent research supports this picture of variations in interhemispheric temperature gradients, whether produced by variability in Atlantic overturning or aerosol forcing, as being a big part of the Sahel drought picture.

John Chiang and collaborators have emphasized the importance of this mechanism for paleoclimate as well as higher frequency climate variations in a series of papers, see this recent review (Chiang and Friedman 2012). Another paper that affected my own work on this issue was that of Broccoli et al 2006, which helped shift the picture of the underlying dynamics from one focused on surface energy balances and changes in tropical ocean temperatures to one focused on the requirements of atmospheric energy balance. Several former students of mine, Sarah Kang, Dargan Frierson, and Tapio Schneider, have picked up on this energy balance perspective in more recent work, starting with Kang et al 2008.

Sarah has focused on a setup in which one takes an atmospheric model of the type used for climate simulations and couples it to a "slab ocean" of uniform depth with no ocean currents, which just provides some heat capacity and a saturated surface. Starting with the case in which there is no heat flux through the bottom of the slab ocean, the resulting climate

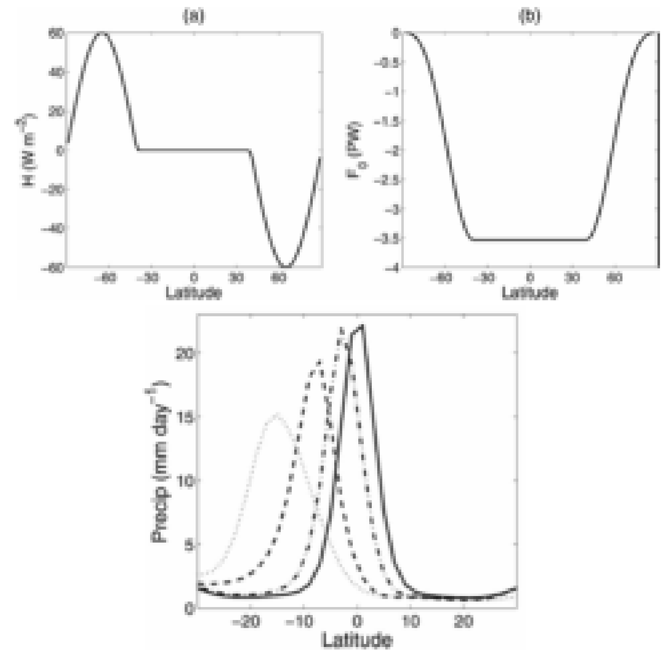

Figure 37.2:

is independent of longitude and symmetric about the equator, with most tropical precipitation confined to a sharp ITCZ located over the equator (the solid line in the lower panel). The model determines its own surface temperature, and the energy flowing into the slab will be zero everywhere if you average long enough. (One of the nice things about this setup is that, unlike models in which surface temperatures are prescribed, you never have to worry about generating a double ITCZ.) Heat is then added poleward of 40N in one hemisphere and the same amount is removed from the other hemisphere (as pictured in the upper left panel.) This is equivalent to prescribing a cross-equatorial heat flux in the ocean underneath the slab (upper right). No heat is being input or extracted equatorward of 40 degrees. After the model equilibrates, the ITCZ has moved into the warmed hemisphere. The larger the heating, the larger the displacement of the ITCZ. The lower panel shows the precipitation from simulations in which the peak in the imposed subpolar heating/cooling is 10, 20 and 40 W/m^2.

One way of thinking about this is to focus on how the surface temperatures in the tropics are affected by the extratropical heat sources/sinks, assuming that the ITCZ will follow the warmest surface temperatures. But

I prefer a perspective based on the atmospheric energy budget, as in the papers by Broccoli et al and Kang et al linked to above.

Before the system is disturbed, the northward heat flux F in the atmosphere is zero at the equator and has some slope in latitude as pictured below. The Hadley cells, symmetric about the equator, have poleward flow in the upper troposphere and equatorward flow near the surface, with rising motion mostly confined to the ITCZ. These cells transport energy in the direction of their upper tropospheric flow. In response to the high latitude heating and cooling, the atmosphere tries to resist the resulting interhemispheric asymmetry by transporting energy across the equator from the heated to the cooled hemisphere. In this setup, you can equivalently talk about how much of the prescribed oceanic flux is compensated by an atmospheric flux in the opposite direction. In the schematic at the top of the post, the fraction of the flux that is compensated is denoted by C. Putting aside how C is determined, we can estimate the new latitude of the "energy flux equator" where the atmospheric flux vanishes. (See sketch below.) If simple Hadley cells continue to dominate the horizontal energy fluxes in the tropics, with most of the rising motion in a sharp ITCZ, then the ITCZ will need to be close to this energy flux equator so that energy flows away from this latitude in both directions.

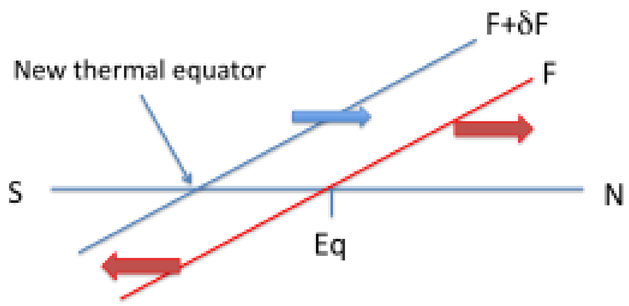

Figure 37.3:

But how do you estimate C? Start by ignoring any responses in clouds. Part of the input of energy into the warmed hemisphere is balanced more or less locally by an increase in the energy radiated away to space and the rest is transported to low latitudes. I picture the transport as a diffusive process (post 37), with a diffusivity that weakens as one approaches the tropics, where the mean meridional circulation (the Hadley cell) takes over a lot of the energy transport. The subtropical heating/cooling by midlatitude storms creates a problem because the tropical atmosphere can't sustain large horizontal temperature gradients. If the change in the net radiation at the top of the atmosphere is primarily a function of tropospheric temperature (ie if clouds don't change), then the changes in this net radiation have to be very uniform with latitude across the tropics. So the key from this perspective is the extent to which the eddy diffusive-like fluxes in midlatitudes manage to extract or input energy into the subtropics of each hemisphere, which the circulation must then redistribute.

As discussed in the Kang et a papers linked to above, if we either fix clouds in the GCM or use an idealized moist GCM with no clouds, this degree of compensation at the equator, C, seems to be of the order of 25-40%, a value you can get from a simple diffusive model with the diffusivity tuned to the atmospheric fluxes in the control climate. If you use the standard AM2 model that was used in our contribution to the CMIP3/AR4 database, you get something like 80%, but this number can be changed by manipulating the closure scheme for moist convection. It's not the convection per se that matters, but the effect of the convection scheme on the cloud feedbacks — the response of clouds to this extratropical heating/cooling perturbation and the effect of these changes on the top-of-atmosphere (TOA) balance. It is still the TOA that matters here, because the net surface fluxes are prescribed — one can only change the net atmospheric poleward fluxes if the TOA fluxes change. (It is this emphasis on the TOA fluxes that distinguishes this perspective from those focusing on surface temperatures.)

There are two distinct kinds of cloud feedbacks that come into play. First, there can be changes in clouds in the high latitude regions which are directly being heated or cooled. These modify the heating/cooling that the atmosphere feels, so they effectively renormalize the forcing. But in addition, once the tropical circulation is modified clouds in the tropics will react to these changes in circulation to alter the energy transports needed to homogenize the tropical temperatures. For example, the strength of the subsidence increases in the tropics and subtropics of the cooled hemisphere, which might result in an increase in low level cloudiness (due to the suppression of vertical mixing of vapor into the upper troposphere)– a positive feedback on the initial cooling. (The movement of the ITCZ would also directly generate changes in long and shortwave fluxes at the TOA, but these tend to cancel — the effects of shallow clouds are often dominant.) So this is a hard problem to get right quantitatively, as are all cloud-related issues it seems. But the qualitative effect is clear.

This mechanism is important when thinking about the tropics during past glacial periods, given the large cooling associated with Northern ice sheets. It is important for the response to aerosol forcing that preferentially cools the Northern Hemisphere. It is important for the response to variations in the Atlantic meridional overturning, which directly modifies the cross-equatorial ocean flux. And it can be important for understanding the mean climatology, as indicated by the reference above to Suki Manabe's early experience with coupled atmosphere-ocean models (see also Hwang and Frierson 2013 and Marshall et al 2013).

38 NH-SH Differential Warming and TCR

[Originally posted June 14, 2013]

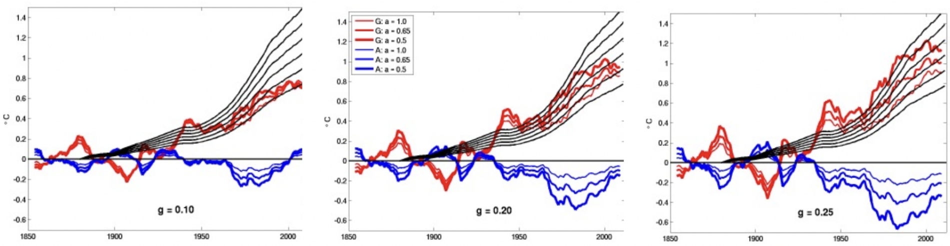

Figure 38.1: Rough estimates of the WMGG (well-mixed greenhouse gas — red) and non-WMGG (blue) components of the global mean temperature time series obtained from observed (HADCRUT4) Northern and Southern Hemisphere mean temperatures and different assumptions about the ratio of the Northern to Southern Hemisphere responses in these two components. Black lines are estimates of the response to WMGG forcing for 6 different values of the transient climate response TCR (1.0, 1.2, 1.4, 1.6, 1.8, 2.0C).

How can we use the spatial pattern of the surface temperature evolution to help determine how much of the warming over the past century was forced by increases in the well-mixed greenhouse gases (WMGGs CO_2, CH_4, N_2O, CFCs), assuming as little as possible about the non-WMGG forcing and internal variability. Here is a very simple approach using only two functions of time, the mean Northern and Southern Hemisphere temperatures. (See posts 7, 27, 35 for related posts.)

Suppose that the temperature record consists of the linear superposition of two parts — the WMGG part and everything else. The real distinction here is that these two parts are assumed to affect the Northern and Southern Hemispheres differently. If the global mean response to the WMGG is $G(t)$, assume that the Northern and Southern hemisphere responses are respectively $(1 + g)G$ and $(1 - g)G$. Similarly for the non-WMGG part, $A(t)$, I'll write the two hemispheric responses as $(1 + a)A$ and $(1 - a)A$.

Here the constants g and a, controlling the pattern of the responses, are assumed to be independent of time, so that the two parts of the response are individually separable in space and time. Given the Northern and Southern hemisphere mean temperatures, $N(t)$ and $S(t)$, I'll just write

$$N(t) = (1+g)G(t) + (1+a)A(t);$$

$$S(t) = (1-g)G(t) + (1-a)A(t).$$

Assuming g and a are given we can solve for the global mean responses G and A:

$$G(t) = -[(1-a)N(t) - (1+a)S(t)]/(2(a-g)),$$

$$A(t) = [(1-g)N(t) - (1+g)S(t)]/(2(a-g)).$$

For example, in the special case of $a = 1$ — in which the non-WMGG part is confined to the Northern Hemisphere — then $G = S/(1-g)$ — so the WMGG component is determined completely by the Southern Hemisphere only, being uncontaminated by the non-WMGG component there.

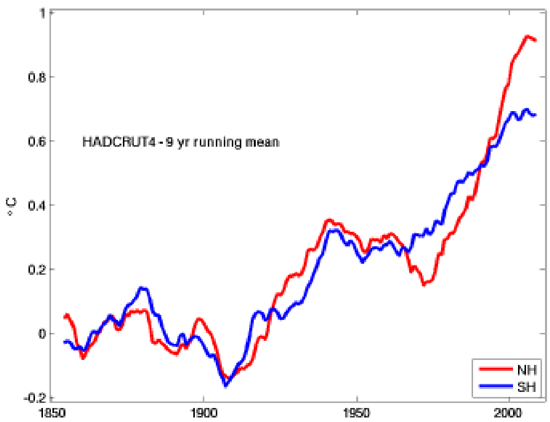

Figure 38.2:

Using $N(t)$ and $S(t)$ from HadCRUT4.2.0.0 and varying g and a over ranges of interest I get the figures at the top. Anomalies are computed from the mean of the first 60 years, starting in 1850, and smoothing with a running 9 year average (see Fig. 38.2). Each panel in the figure at the top of the post corresponds to one value of g (top: $g = 0.10$; mid: $g = 0.20$; bot: $g = 0.25$) and $G(t)$ and $A(t)$ are shown for three different values of a (0.5, 0.65, and 1.0 as indicated by the legend on the middle panel — the three values of a are the same in each panel). Also shown is the response to WMGG forcing using the GISS forcing estimate, normalizing by a multiplicative constant to agree with the value of Skeie et al 2011 of $2.83W/m^2$ in 2010, and then multiplied by $(TCR)/(2 \times CO_2)$, where $2 \times CO_2 = 3.7W/m^2$. The 6 black lines in the figure correspond to TCR = (1.0, 1.2, 1.4, 1.6, 1.8, 2.0).

If you can ignore phase lags between forcing and response, you can think of TCR (the transient climate response) in this context as a multiplicative constant that you multiply the WMGG forcing by to get the global transient climate response to the WMGGs, in degrees C, normalized to refer to doubling of CO_2. The TCR implicitly takes into account ocean heat uptake as well as radiative feedbacks.

One can read off the value of g simulated by a set of CMIP5 models run with varying WMGGs as the only forcing agents in Fig. 4 of Friedman et al 2013. I get about 0.15 or 0.20 eyeballing the figure. Also, if you assume that the non-WMGG component is primarily aerosol forced, the same figure implies a value of a of about 0.5 . (The figure also gives you a sense of how separable in time the responses are to WMGGs and aerosols in isolation.) If mutidecadal natural variability dominates over aerosols, then I would expect a value of a closer to 1, or even greater than 1, since variability in the Atlantic overturning, in particular, should, if anything, cool the southern while warming the northern hemisphere. (If the non-WMGG component consists of an aerosol part and an interannual variability part of comparable magnitude, and with different values of a, this kind of simple linear transformation will be of limited utility.)

If there are other forcing agents (ie increased stratospheric water or tropical volcanoes) that result in a modest interhemispheric contrast in warming or cooling similar to the assumed structure of the WMGG response, these would find themselves lumped together with the WMGG component. You don't have to smooth, but interannual variability (ie ENSO) most likely would not project cleanly onto one or the other component, so I don't see any advantage of leaving it in.

I have used these numbers and considerations in deciding which combinations of (g, a) to use in the plots, also keeping in mind that it makes no sense to allow g and a to get too close to each other, since the near-degeneracy would then result in a meaningless decomposition.

The idea here, which should be clear from the inclusion of the TCR-normalized WMGG forcing curves in the figures, is to use what we are relatively sure about — the time history and the radiative forcing from the WMGG's — to constrain TCR. while assuming as little as possible about aerosol forcing and natural multi-decadal variability.

The panel at the top is a case with g approaching 0. In this limit the non-WMGG component has to explain all of the interhemispheric difference, and since this component is assumed to be Northern-centric the observed larger increase in the north requires global mean warming from this term, pushing the WMGG component down to a TCR value of 1.0 or so. This is a picture that you might favor if you think, for other reasons, that the non-WMGG component is dominated by internal variability.

The middle panel has somewhat larger TCR, with the net non-WMGG component small near the present because the observed north/south ratio is similar to that implied by the assumed WMGG pattern in isolation.

In the bottom panel, the value of g is large enough to leave room for a substantial "aerosol" affect remaining at present, resulting in a larger TCR that depends more strongly on the value of a: smaller a results in less difference in the north-south ratios between the two patterns, producing more compensation.

These results will be sensitive to the input data set due to the role played by the relatively small interhemispheric differences. You could propagate the observational error estimates provided along with the HADCRUT4 data set through this transformation, as well as use information about the distribution of a and g from individual models in the CMIP ensembles. But the choice of the two hemispheric means for this analysis is arbitrary. I am sure that one could be more systematic along the lines of the fingerprinting literature (but much of this literature assumes more about the aerosol forcing time dependence than I would prefer). And one could look in more detail at the assumption of negligible phase lag in the WMGG response over the past century needed when trying to constrain TCR. I am thinking of this as being more exploratory than quantitative, nudging readers to think beyond the global mean time series.

39 FAT

[Originally posted June 16, 2013]

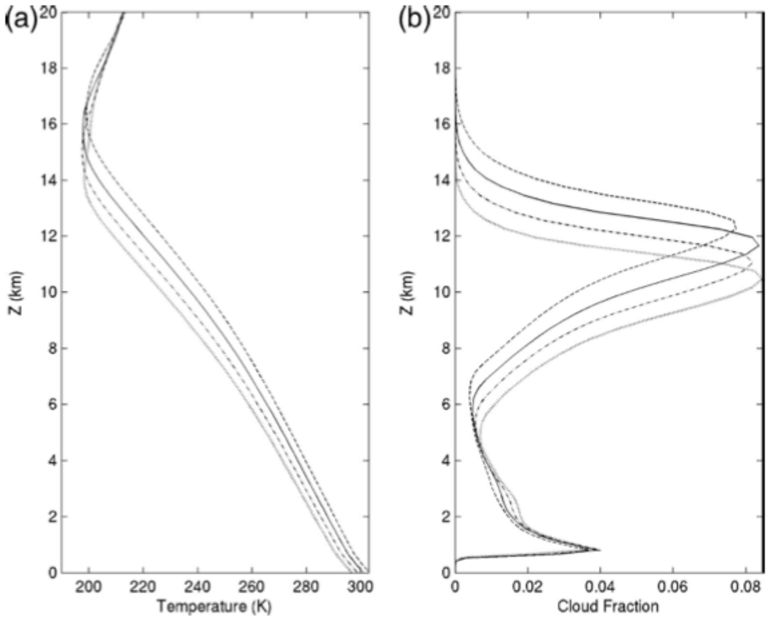

Figure 39.1: The response of a 1km non-rotating doubly periodic model of radiative-convective equilibrium to an increase in surface temperature, in increments of 2K. Left: temperature, showing a moist-adiabatic response; Right: fraction of area with cloud at each height, showing an upward displacement of upper tropospheric clouds. From Kuang and Hartmann 2007.

The presence of cirrus clouds in the tropics warms the troposphere because infrared radiation is emitted to space from their relatively cold surfaces rather than the warmer temperatures below the clouds. The response of these clouds can be important as feedbacks to climate change. A reduction in the area covered by these high clouds would be a negative feedback to warming through this infrared effect, compensated in part by a a positive shortwave feedback.] An increase in the average height of these clouds with warming, resulting in a colder surface than would be the case if this height did not increase, would be a positive feedback. It is the latter that I want to discuss here. GCMs have shown a positive feedback due to increasing height of tropical cirrus since the inception of global modeling (e.g., Wetherald and Manabe 1980). This is probably the most robust cloud feedback in GCMs over the years and is one reason that the total cloud feedbacks in GCMs tend to be positive. This increase in cloud top height has, in addition, a clear theoretical foundation, formulated as the FAT (Fixed Anvil Temperature) hypothesis by Hartmann and Larson 2002.

The FAT hypothesis argues that the temperature of these tropical cloud tops should remain at fixed temperature (to a first approximation) as the climate warms. The argument depends on the relative humidity remaining more or less unchanged in the tropical upper troposphere and on basic radiative transfer. The key aspects of radiative transfer relevant to FAT are discussed elegantly by Ingram 2010.

Most radiatively active cirrus clouds in the tropics are generated by the detrainment from deep convective clouds, starting out as the anvils of thunderstorms, thinning as they spread horizontally and evaporating as they subside and warm. Their characteristic height is determined by the height at which the strongest outflows from convective updrafts occur. What determines this outflow height?

The temperature profile in the tropics is close to a moist adiabat set by the deep convection that occupies a small fraction of the area. This moist adabat is efficiently communicated to the rest of tropics and sets the temperature profile in non-convecting areas as well. When air tries to descend in the clear regions, to compensate for the upward mass flux in the deep convective cores, its temperature will try to follow a dry adiabat, but these temperatures are too warm to be consistent with the moist adiabat set by the convection, and it is the radiative cooling that allows for a consistent picture, with the magnitude of cooling setting the rate of descent of air away from the convection. Specifically, the subsidence rate is proportional to Q/σ, where Q is the radiative cooling and σ is a measure of the dry stability of the air column, the departure of the temperature profile from the dry adiabat.

So the vertical structure of the radiative cooling in the clear sky must be consistent with the vertical structure of the upward mass fluxes in the deep convective cores, since the compensating subsidence is constrained by this radiative cooling. Suppose that the radiative cooling is constant below the height Z2 and drops to 0 at the higher level Z1 (see sketch below). The rate of subsidence needed to balance this profile of cooling will have a similar vertical structure (ignoring structure in σ). This increase in subsidence in the clear tropical regions, by conservation of mass, must be accompanied by a transfer of mass from the convective cores to the subsiding region. So the outflow from the cores will be concentrated where cooling decreases rapidly with height. Water vapor is responsible for most of the cooling of

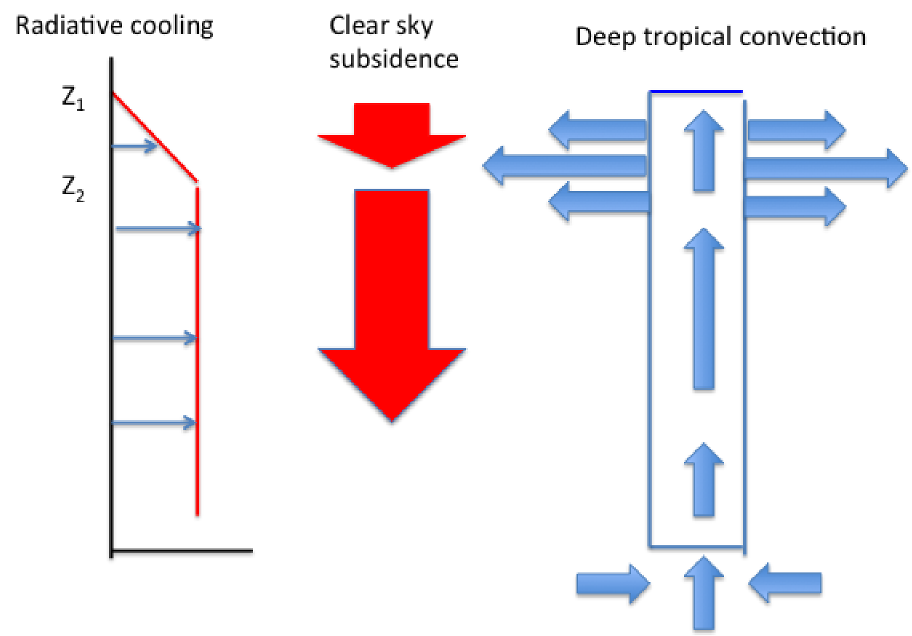

Figure 39.2:

the tropical upper troposphere. Roughly speaking the height at which the cooling has a sharp vertical gradient is determined by the total amount of water above this height. Since the saturation vapor pressure is a function of temperature only, if the relative humidity is fixed the profile of water vapor pressure in the vertical is determined by the temperature, and the temperature at which this gradient occurs will tend to be fixed. Ingram, in the paper linked to above, provides a clear discussion of the assumptions required to get the result that the infrared cooling rate is a function of temperature only. Besides fixed relative humidity (or more generally, relative humidity that is a function of temperature), the key assumption is that infrared absorbers/emitters other than water vapor, including the surface, can be ignored in computing the cooling in this region. I won't try to duplicate that analysis here.

This seems like a long argument with a lot of assumptions, but it is confirmed rather beautifully in high resolution cloud-resolving simulations in small domains. The figure at the top is from a high resolution (1km in the horizontal) model of non-rotating radiative-convective equilibrium of the sort discussed in posts 19 and 26. This kind of model allows us to directly simulate aspects of deep moist convection in the tropics. The domain is 64 x 64 km. (The small domain serves to avoid some of the issues related to spontaneous aggregation discussed briefly in 19.) A nice feature of this simulation is the relatively high vertical resolution compared to most other models of this type. The surface temperature is a prescribed boundary condition. The two plots above show the averaged temperature and cloud cover responses to warming of surface temperature. The warming on the left has the familiar structure of increasing amplitude with height consistent with a moist adiabat. And the clouds on the right clearly move upwards with warming.

But plotting the upper tropospheric cloud cover as a function of temperature rather than height, Kuang and Hartmann get the figure below (the modest changes in the magnitude of the cloud cover are removed from the plot by normalizing by the maximum value so as to focus on the vertical structure) The theory works remarkably well. The change in the temper-

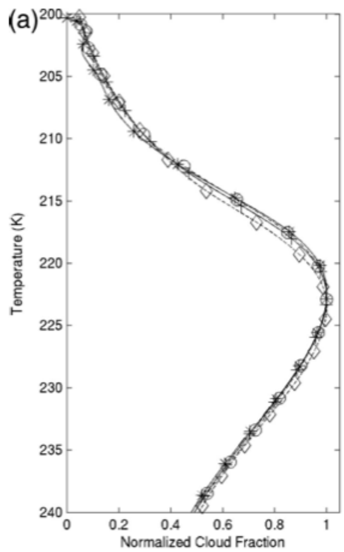

Figure 39.3:

atures at the height of the maximum in the cloud cover is an order of magnitude smaller than the change in temperature at fixed height.

The arguments underlying this result are quite general and don't depend on the changes in the extent of cloud cover or the spatial organization of the convection being negligible, as long as the relative humidity does not change much. So one should be able to see this FAT feedback in the interannual variability in the tropics, which is important since a lot of the satellite data that allows one to study this kind of thing is restricted to the last decade or so, which is not long enough to analyze trends usefully. Zelinka and Hartmann 2011 provide an extensive discussion of the observational picture based on analysis of internanual variability over the past decade. The result is very encouraging for the basic theory. They emphasize, however, that the way in which σ, the departure of the temperature profile from the dry adiabat, changes in interannual variability and in global warming might be sufficiently different to complicate inferences about subsidence profiles ($\propto Q/\sigma$) and outflow temperatures from cooling rates Q.

FAT cloud feedback is closely linked to water vapor feedback since it depends on relative humidity remaining relatively unchanged in the tropical upper troposphere (especially when plotted against temperature rather than height). Obviously, a lot rides on the response of relative humidity to warming.

40 Playing with a Diffusive Energy Balance Model

[Originally posted Auguest 8, 2013]

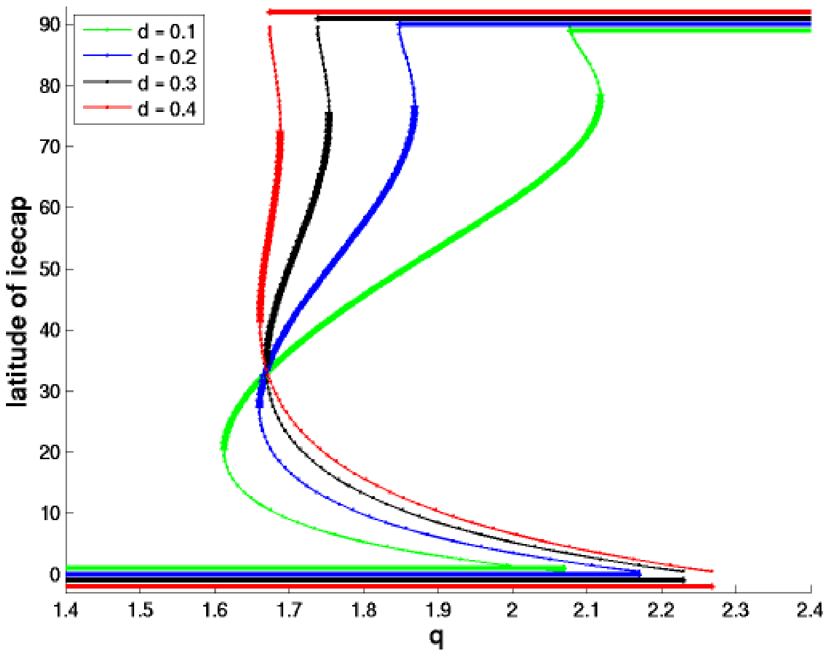

Figure 40.1: Latitude of ice margin as a function of a non-dimensional total solar irradiance q in the diffusive energy balance climate model described by North 1975, for different values of the non-dimensional diffusion d. Stable states are indicated by a thicker line.

When we were first starting out as graduate students, Max Suarez and I became interested in ice age theories and found it very helpful as a starting point to think about energy balance models for the latitudinal structure of the surface temperature. At about the same time, Jerry North had simplified this kind of model to its bare essence: linear diffusion on the sphere with constant diffusivity, outgoing infrared flux that is a linear function of surface temperature, and absorbed solar flux equal to a specified function of latitude multiplied by a co-albedo that is itself a function of temperature to capture the different planetary albedos for ice-free and ice-covered areas. Playing with this kind of "toy" model is valuable pedagogically — I certainly learned a lot by building and elaborating this kind of model — and can even lead to some nuggets of insight about the climate system.

Using the same notation as in post 36,

$$C\partial T/\partial t = \nabla \cdot C\mathcal{D}\nabla T - (A + B(T - T_0)) + \mathcal{S}(\theta)\mathcal{A}(T). \tag{40.1}$$

$\mathcal{S}(\theta)\mathcal{A}(\mathcal{T})$ is the absorbed solar flux, where $\mathcal{A}(\mathcal{T})$ is the co-albedo. We set $S = S_0\sigma(\theta)$, where S_0 is the global mean incident flux = total solar irradiance/4 so that $\sigma(\theta)$ averages to unity over the sphere. The form $\sigma(\theta) = 1 - 0.5P_2(sin(\theta))$, with $P_2(x)$ the second Legendre polynomial $(3x^2 - 1)/2$ is a pretty good fit to the annual mean incident solar flux as a function of latitude θ. The seasonal cycle is ignored here. $A+B(T-T_0)$ is the outgoing infrared flux linearized about the reference temperature T_0, C is a heat capacity, and $\mathcal{D}$ is a kinematic diffusivity. We can just set $T_0 = 0$ (and think of it as the freezing temperature when we set the albedo). We can define a non-dimensional temperature, $\mathcal{T} \equiv BT/A$, diffusivity $d \equiv CD/Ba^2$, and mean incident solar flux or total solar irradiance, $q \equiv S_0/A$. Finally, we can use the simplest possible albedo formulation that provides some ice-albedo feedback: $\mathcal{A}(T) = \beta$ for $\mathcal{T} > 0$ and $\mathcal{A}(\mathcal{T}) = \alpha$ for $\mathcal{T} < 0$. I use $(\alpha, \beta) = (0.4, 0.7)$ for the results described here.

Our equation for steady state solutions independent of longitude, writing out the divergence of the diffusive flux in spherical coordinates, is now

$$\mathcal{T} - \frac{1}{\cos(\theta)}\frac{\partial}{\partial\theta}(\cos(\theta)d(\theta)\frac{\partial\mathcal{T}}{\partial\theta}) = q\sigma(\theta)\mathcal{A}(\mathcal{T}(\theta)) - 1. \tag{40.2}$$

If both the diffusivity and the albedo are chosen to be spatially uniform one can solve this equation analytically for this specific choice of $\sigma(\theta)$ because $\mathcal{T}$ is then a constant plus a term proportional to the second Legendre polynomial P_2 which is an eigenfunction of the Laplacian. Comparing the result with $d = 0$ with that for non-zero d, one finds that the presence of diffusion reduces the equator-to-pole temperature gradient by the factor $1/(1 + 6d)$. One needs this reduction to be about a factor of 2-3 to get a reasonable temperature gradient, which translates into a value of d of 0.2-0.3.

We look for solutions that are symmetric about the equator and have temperatures below freezing poleward of a given latitude, the" icecap edge", and above freezing equatorward of this latitude. The resulting ice edge as a function of q for different values of d is shown in the figure at the top of the post. Ice-free states are indicated by the horizontal line at $\theta = 90$ and ice-covered "snowball Earth" states by the horizontal line at $\theta = 0$. Partially glaciated steady states also exist, some of which are unstable. The branches on which the ice edge moves equatorward with increasing q are unstable, not surprisingly. There is a small ice cap instability, with ice caps smaller than this threshold receding unstably to the ice-free state. And there is a large ice cap instability beyond which the ice grows unstably due to a runaway albedo feedback, until one reaches the snowball state. This kind of model attracted considerable attention because of the rather small range of solar flux for which partially glaciated states exist and the proximity of these state to the large ice cap instability, for plausible values of the diffusivity. As a point of comparison, Voigt and Marotske 2010, using a modern comprehensive coupled atmosphere-ocean GCM, find that a reduction of 6-9% in the solar flux is sufficient to generate the large-icecap instability. This is obviously an interesting number.

The small ice cap instability in this simple model captures the basic idea that an icecap has to have a certain size to protect itself from diffusion of warm air from lower latitudes. The critical size increases with increasing diffusivity. But this small ice cap instability (unlike the large ice cap instability) turns out to be fragile to modifications to the model such as smoothing the temperature dependence of the albedo or adding a seasonal cycle or adding some noise. To decide if the Arctic ice possesses a small ice-cap instability requires much more realistic atmospheric and sea ice models. But this simple model does get you thinking about the importance of heat transport from lower latitudes for this issue.

In post 36 there are some plots indicating that the effective diffusivity for heat in the atmosphere should be thought of as having a maximum in midlatitudes. To mimic this schematically, I have set $d = 0.4$ for $40° < \theta < 60°$ and $d = 0.2$ elsewhere. The solution is shown by the black line in the figure below. (The results for uniform values of $d = 0.2$ with $d = 0.4$ are copied over from the figure at the top for comparison.) A new instability has been created, with no stable ice caps ending within the region in which the diffusivity has been given the larger value of 0.4. Comparison with the uniform $d = 0.4$ case indicates that it is not simply the magnitude of the diffusivity that creates this instability but rather its horizontal structure. David Linder, Max, and I touched on this kind of behavior in an old paper Held et al 1981, Some related results are discussed in Rose and Marshall 2009, coming from the direction of trying to include the effects of ocean heat transport in an energy balance model. This kind of stability diagram could

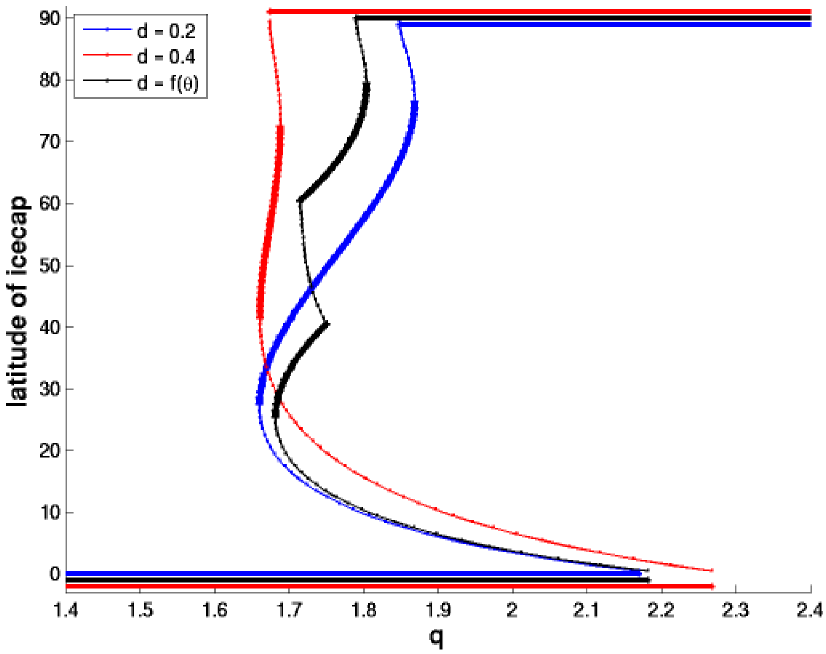

Figure 40.2:

generate some interesting hysteresis loops from a time-dependent parameter like the obliquity in Milankovitch ice age theories.

Irrespective of any imagined relevance for the ice age problem, I am interested in closing the gap between this kind of diffusive model and GCMs. In particular, it would be interesting to take an aqua planet atmospheric GCM over a slab ocean with some heat capacity but no horizontal oceanic heat fluxes, no seasonal cycle, no sea ice, but with a simple specified surface albedo as a function of temperature, and map out its behavior as a function of the incident solar flux (with and without cloud feedback). You could then try to mimic this idealized but still chaotic and turbulent GCM's behavior with steady state energy balance models incorporating theories for the atmospheric heat flux. Points of interest include how an advancing ice edge affects the effective diffusivity and how best to represent cross-equatorial influences.

[I think it is an excellent project for beginning students to generate these solutions on their own. You can reintroduce the time-dependence and integrate forward in time, but this will only give you the stable states. For this simple case of a step function albedo, there is an easy way of getting all of the states shown: specify the ice edge and, therefore, the albedo, then solve the boundary value problem directly for the steady state by inverting the tridiagonal matrix that you get from simple finite-differencing. The solutions will not have the $T = 0$ point coincide with the ice edge, so you then need to iterate and find the value of q that gives you consistency between temperature and albedo.]

41 The Hiatus and Drought in the U.S.

[Originally posted September 23, 2013]

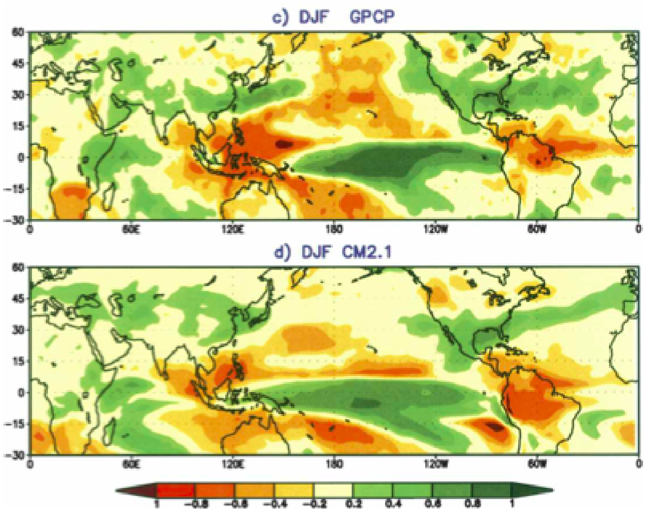

Figure 41.1: Correlation between seasonal mean precipitation (Dec-Jan-Feb) and sea surface temperatures in the eastern equatorial Pacific (Niño 3.4: 120W-170W and 5S-5N) in observations (GPCP) and in a free-running coupled atmosphere-ocean model (GFDL's CM2.1), from Wittenberg et al 2006. Green areas are wetter in El Niño and drier in La Niña winters; red areas are drier in El Niño and wetter in La Niña.

It is old news to farmers and water resource managers in the southern tier of the continental US that La Niña is associated with drought, especially with rainfall deficits in the winter months. Since the major El Niño event of 1997-8, our climate system has been reluctant to generate El Niño at the expected frequency and instead the Pacific has seen several substantial La Niña events with mostly near neutral conditions in between. This La Niña flavor to the past 15 years has been identified as causing at least part of the hiatus in global warming over this same period by simple empirical fitting and more recently by Kosaka and Xie 2013, in which a climate model is manipulated by restoring temperatures to observations in the eastern equatorial Pacific. I find the excellent fit obtained in that paper compelling, having no free parameters in the sense that this computation was not contemplated while the model, GFDL's CM2.1, was under development, and the model was not modified from the form in which it was frozen back in 2005. The explanation for the hiatus must, in appears, flow through the the equatorial Pacific. (I have commented on this paper further here.) These authors mention briefly an important implication of this connection — the extended drought in the Southern US and the hiatus in global mean warming are related.

The figure at the top compares the response of precipitation to ENSO in an observational estimate and in the same climate model as utilized by Kosaka and Xie. This result is obtained with a free-running model, producing its own ENSO variability. (The correlation averages over any asymmetries between warm El Niño and cold La Niña phases, which are not exact mirror images of each other, but does not change the basic picture.) The model evidently generates a reasonable simulation of the precipitation response over the US, justifying the discussion by Kosaka and Xie of the connection to the hiatus. Results such as these are what make the case that global models are of value in estimating the broad-scale changes in precipitation associated with climate change if not, as yet, detailed regional features.

In passing, I just want to put in a good word for the simulation of ENSO in current climate models. This comes in for a lot of criticism it seems, but from my perspective, having been around for a while, I am impressed by how far we have come. ENSO variability develops spontaneously, of course, just like midlatitude storms on much shorter time scales. The ENSO simulation in this model is not without its problems, needless to say. Its amplitude is too strong and the structure has some problems as well, most notably temperature anomalies spread too far westward on the equator in the Pacific, distorting the Indonesian drying among other things. This model was developed a decade ago — we (that is, my GFDL colleagues) are confident that we can do better now (Delworth et al 2012). Are these model's good enough to simulate the response of ENSO to increasing greenhouse gases or changing aerosols?

When I first started this blog I thought that I would try to focus on things other than the global mean surface temperature time series, but if you are a regular reader you know that I haven't been very successful at this. And it becomes even harder with the emphasis on the hiatus. But this connection between US drought and the hiatus emphasizes for me how important it is to look at things more broadly, especially when there is more than one thing going on (ie two different kinds of external forcing or external forcing plus internal variability). This model, like most others, predicts that increasing well-mixed greenhouse gases will dry the Southwest and South central US. So El Niño and increasing greenhouse gases have the same sign effect on global mean temperature, but opposite effects on rainfall in the Southern US.

We think of the response of precipitation to greenhouse gas forcing as a combination of a part that is controlled by the temperature increase — specifically the increase in water vapor accompanying the temperature increase, as discussed in Post 13 — and a part related to changes in atmospheric circulation. The response to ENSO is mostly due to changes in atmospheric circulation, which have little resemblance to the circulation changes induced by greenhouse gas forcing – in fact they tend to have the opposite character, with the large-scale circulation shifting equatorward rather than poleward with global mean warming — as indicated in the following figure, from Lu et al 2008: This plot shows results from CM2.1 once

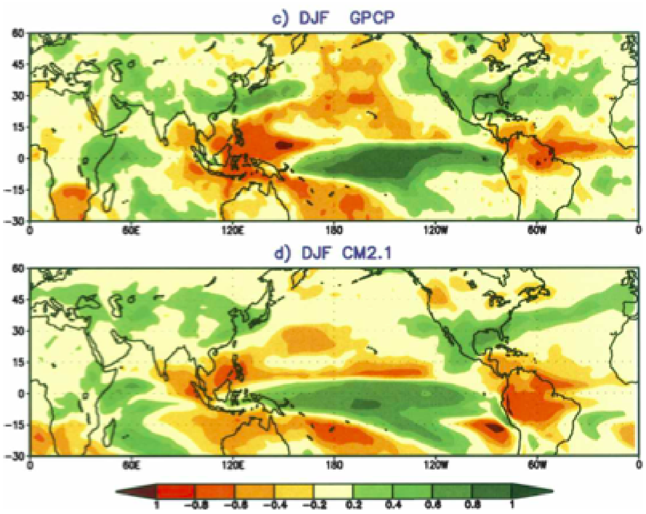

Figure 41.2:

again, focusing on the zonal mean (the average around a latitude circle) of the zonal wind (positive if from the west). The contours in each plot are the model's climatological mean in Dec-Jan-Feb and the colors the change due to ENSO (El Niño – La Niña) on the left and the trend over the 21st century in a projection using the A2 scenario on the right. Red is positive and green negative, so comparing the colors to the climatology, you can see that El Niño moves this entire pattern equatorwards while the model's 21st century trend, dominated by the response to greenhouse gases, moves it polewards. Flipping the sign on the left to correspond to the La Niña state would make the figures look more similar. The rainfall pattern outside of the tropics goes along for the ride on these circulation changes. There is a lot of literature on the theory for these large-scale shifts in circulation, some of it using variants of the fruit fly model (post 28). One thing these model results are telling us is that global mean warming, or the warming of the tropics in isolation, cannot be the primary reason for these shifts in circulation in general.

The magnitudes in this figure are also telling us something. It takes a century of global warming to reach the amplitude of the change in circulation associated with a flip from El Niño to La Niña. This might seem small, until you think about the implications of a shift in the mean comparable to the peak-to-peak variations in the ENSO cycle. Well before this point one would reach a situation in which the shift in the mean is comparable to changes in circulation or rainfall averaged over one or two decades. Whether we are already approaching the latter point is obviously a key question in climate research. The magnitude of the precipitation changes in the US due to an increase in greenhouse gases varies from model to model, so estimates of what role global warming has played in precipitation changes over the US during the hiatus will be model dependent. Some of the inter-model spread here is itself likely to be related to the spread in the response of the equatorial Pacific to greenhouse gas forcing.

42 Aquaplanet Hurricanes and the ITCZ

[Originally posted October 21, 2013]

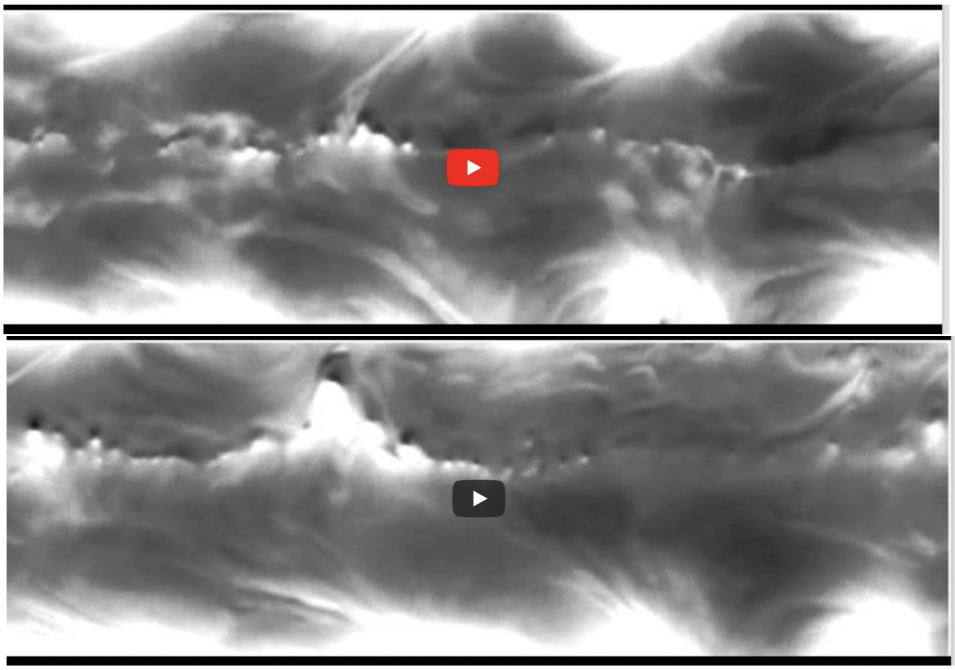

Figure 42.1: onal (east-west) wind in the lower troposphere (850mb) in two simulations with a 50km resolution atmospheric model with zonally symmetric boundary conditions. Only 180° of longitude within the tropics (30S-30N) is shown. The ITCZ is located at $3°N$ in the upper panel and $8°N$ in the lower panel. Simulations described in Merlis et al 2013. (White, Black) $\Longrightarrow$ winds from the (west, east).

For animations, 6 frames/day for 100 days, see here and here.

The frequency of formation of hurricanes/typhoons has mostly been studied in the past by trying to develop "genesis indices" – empirical relations between the frequency of storm genesis and the larger scale circulation and thermodynamic structure of the atmosphere. But there is an ongoing transition, picking up steam in a number of atmospheric modeling groups around the world, to using global atmospheric models that simulate hurricanes directly to study how genesis is controlled. One goal of this work is to understand how hurricane frequency responds to the warming resulting from increasing greenhouse gases. Posts 2, 10, and 33 describe some of our recent efforts at GFDL along these lines. That work uses models in a comprehensive setting, with a seasonal cycle and realistic distribution of continents. But Tim Merlis, Ming Zhao, Andrew Ballinger and I have started looking at analogous simulations with global models in more idealized settings.

The animation above is from a model described in Merlis et al 2013. The model has no continents, and no seasonal or diurnal cycles, and the ocean is replaced by a stationary slab of water 20 meters thick, providing some heat capacity and a source of water vapor. The temperature of the slab ocean is predicted by the model. Other than the boundary conditions and lack of seasonal forcing, the model is identical to the one that generates the simulations described in post 2 in which sea surface temperatures are prescribed.

We start with a circulation forced symmetrically between Northern and Southern Hemispheres. Tropical rainfall is then localized in an intertropical convergence zone (ITCZ) centered on the equator. No hurricanes form in this model configuration despite the fact that the model with realistic boundary conditions generates about the right number. Then, just as in Kang et al 2008 (see post 37), we move a given amount of heat within the ocean from high latitudes of one hemisphere to high latitudes of the other hemisphere, causing the tropical rain belt to move some distance off the equator, allowing hurricanes to form. The animations above show two cases, with the ITCZ located roughly at 3N and 8N. The two runs differ only in the prescribed cross-equatorial heat transport. The sensitivity of hurricane number to perturbations in ITCZ latitude int his model is impressive — about a 40% increase per degree latitude poleward displacement of the ITCZ when the ITCZ is at 8N.

[This increase is genesis as the ITCZ is moved off the equator is related to the magnitude of the vorticity in the larger scale environment, a parameter in all empirical genesis indices. Vorticity is the curl of the velocity field. If a fluid is in solid body rotation, with angular velocity $\vec{\Omega}$, a vector that points in the direction of the axis of rotation, the vorticity is simply $2\vec{\Omega}$. But the atmosphere is a thin shell on the surface of a sphere, so it is primarily the radial (locally vertical) component of the vorticity of the solid body rotation that the storms care about, $f = 2\Omega \sin(\theta)$ where θ is latitude. f vanishes at the equator and increases linearly as one moves off the equator.]

You sometimes hear the view expressed that there might be some simple, elegant theory for the average number of tropical cyclones that form per year. I guess this conviction is based on the idea that these cyclones play a fundamental role of some kind in maintaining the climate and that you need a certain number, more or less, to fill that role. (Also, the globally averaged number of tropical storms does not seem to vary much from year to year.) I don't have much sympathy for this view, as I have never understood what this role might be. I think a more plausible hypothesis is that tropical cyclones are the tail of the dog with weak effects on the general circulation as a whole, at least in a climate at all resembling what we have now. These simulations reinforce my view on this. If boundary conditions are idealized and conditions are modified so that the climate is zonally symmetric and the ITCZ lies along the equator, no hurricanes form in the model (there are a few weak storms that spin off midlatitude fronts penetrating into the subtropics). Has the role that these storms are needed to fill somehow changed with this change in boundary conditions?

Another way of eliminating tropical storms in the model is to reduce the heat capacity of the model "ocean", the depth of the stationary slab of water. If you take a simulation with a realistic number of hurricanes, this number decreases and eventually approaches zero as the depth of this slab ocean approaches zero. The surface that the atmosphere sees in this limit resembles a water saturated land surface — a swamp. A mature tropical cyclone is a strongly damped vortex that is continually extracting energy from the ocean. If the slab depth is too shallow, then, in response to the energy extraction, the surface cools too much to sustain deep convection. (Tropical cyclone statistics seem to converge for slab depths greater than 20m in our model.) The model's atmospheric climate as a whole changes in only rather modest ways as this heat capacity is decreased – in the absence of a seasonal cycle.

Once we have moved the ITCZ off the equator, we then increase the model temperature with the total solar irradiance or CO_2. The number of hurricanes increases – about 15% per $^\circ C$ of tropical warming. This is interesting to us because the number of tropical cyclones or hurricanes tends to decrease (or remain roughly constant) with warming in most models — when they are configured with realistic boundary conditions — and this model is no different. In the idealized model the ITCZ moves further poleward with tropical warming, about 0.6 degrees latitude per $^\circ C$. If we compensate for this poleward movement by decreasing the cross-equatorial heat flux in the ocean by just the right amount, we find that the number of hurricanes does decrease, by about 10% per $^{\circ}C$ tropical warming. With fixed oceanic heat transport, the increase due to displacement of the ITCZ overcompensates for this reduction.

In these particular idealized simulations, the response of hurricane frequency (N) to warming seems to breaks down into three different problems, each involving very different dynamical mechanisms: the dependence of ITCZ latitude on warming, that is, on an increase in insolation or CO2; the dependence of N on warming with fixed ITCZ latitude, and the dependence of N on ITCZ latitude at fixed tropical mean temperature.

There are mechanisms relevant for storm development in more realistic climate configurations that are muted or absent in this aqua-planet setup. But even in this idealized aqua-planet model, work underway by Andrew, Tim, and Ming indicates that there are other characteristics of the tropical circulation besides the ITCZ latitude that help control hurricane frequency. So this is still work in progress.

43 Rotating Radiative-Convective Equilibrium

[Originally posted December 31, 2013]

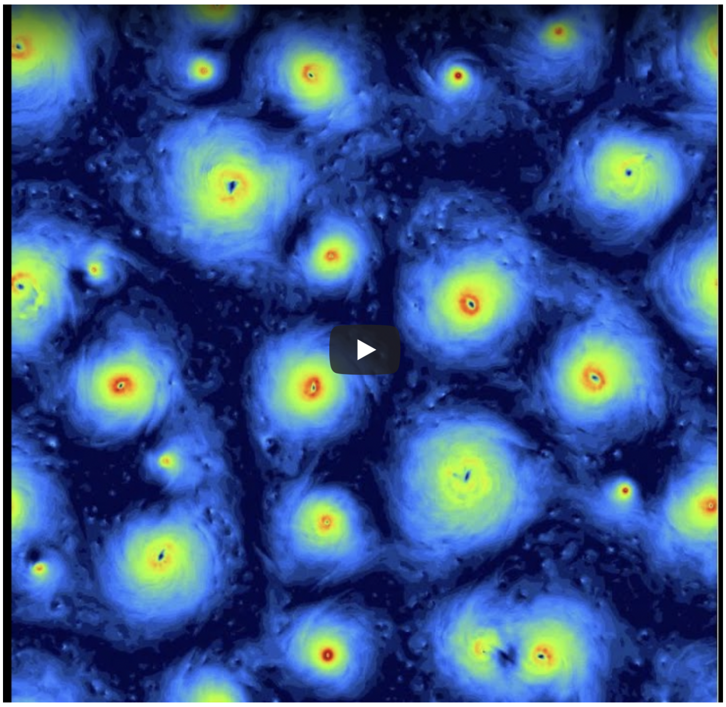

Figure 43.1: Sanpshot of near-surface wind speeds in rotating radiative-convection equilibrium, following Zhou et al, 2014

Animation can be found here.

I have discussed models of non-rotating radiative-convective equilibrium (RCE) in previous posts. Given an atmospheric model one idealizes it by throwing out the spherical geometry, land-ocean configuration and rotation, creating a doubly-periodic planar geometry re-entrant in both x and y, while also removing any horizontal inhomogeneities in the forcing and boundary conditions. In the simplest case, surface temperatures are specified and the surface is assumed to be water-saturated. The result is an interesting idealized explicitly fluid dynamical system for studying how the climate — especially that of the tropical atmosphere — is maintained by a balance between destabilization through radiative fluxes and stabilization through turbulent moist convection. There is a lot that we don't understand about this setup, which still contains all of the complexity of latent heat release and cloud formation. But even though we don't understand the non-rotating case very well, it is interesting to re-introduce rotation while maintaining horizontal homogeneity. Adding rotation has a profound influence on the results — the model atmosphere fills up with tropical cyclones! Some colleagues suggest referring to this system as TC World; others suggest Diabatic Ekman Turbulence. I'm going to stick with Rotating Radiative-Convective Equilibrium, or Rotating RCE for short.

You can include rotation while retaining horizontal homogeneity by adding a Coriolis force of fixed strength, independent of latitude. In fact, one typically ignores the vertical component of the Coriolis force and simply adds terms to the horizontal equations of motion that, in isolation, would cause the horizontal winds to rotate at a fixed rate f, the Coriolis parameter. (This geometry is referred to as the f-plane in textbooks and articles on geophysical fluid dynamics.).

Wenyu Zhou has been studying Rotating RCE in collaboration with several of us at GFDL. The first paper on this work is Zhou et al 2014. The animation above is the near-surface wind speed from one of the simulations analyzed in this paper. Red corresponds roughly to hurricane strength winds. $f = 2\Omega \sin(\theta)$ with $\theta = 20°$ latitude and Ω is the magnitude of the angular velocity of the Earth. Surface temperatures are fixed at 300K. A month of simulation is shown, after several months of equilibration starting from an initial condition with no TCs present.

In studies of RCE, we often push the horizontal grid down to 1 or 2 km to help in explicitly simulating at least the largest convective plumes that extend to the tropopause. In this paper we use a much coarser resolution, 25km, to the consternation of some reviewers — we simply take a global atmospheric model with 25 km resolution and place it in this idealized f-plane geometry. The model includes a a sub-grid closure scheme for moist convection. The number of grid points in the horizontal is 800×800, producing a 20,000km square domain. This is not meant as a model of a little patch of the atmosphere! We are, of course, interested in how a model with 1 or 2 km grid would behave, but that would be computationally expensive for us even in a smaller domain barely large enough to contain a few storms — we want to have enough storms in the domain that we can study things like how the average spacing between storms varies with rotation rate or SST (this distance increases with decreasing f and with increasing SST.) But we are also especially interested in how our global model, which simulates the geographical and seasonal distribution of TC genesis rather well (post 2), behaves in this idealized geometry. I was involved in an earlier paper taking the same approach of placing a global model in this idealized f-plane geometry, Held and Zhao 2008, but with even coarser resolution. That paper did not create much of a stir. This approach gets more interesting (and the review process becomes a bit less painful) when the global model that we start with has TC statistics that look realistic. Increasing computer power should make exploration of this kind of rotating moist-convective turbulence more common.

In a recent paper Khairoutdinov and Emanuel 2013 have generated simulations with 3 km resolution that produce multiple storms that qualitatively resemble the result shown above. They make the computation tractable by increasing f by an order of magnitude compared to Earth-like values, resulting in storms small enough that you can get into this multiple storm regime much more easily.

You can get a sense from the video that the model does produce storms with a relatively well-defined radius of maximum winds. Wenyu describes how this internal storm scale, despite our low resolution, changes systematically with model parameters. This is obviously one place where there is likely to be important sensitivity to resolution. But we also find that the size of the domain, if too small, can modify the sensitivity of this radius of maximum winds to other parameters by not allowing the storm to settle into its preferred horizontal structure. A nice comparison of theories for mature TC structure with numerical simulations, including Rotating RCE, can be found in a recent thesis Chavas 2013.

The most interesting qualitative result to me is simply that in this homogeneous system the natural equilibrated state is an atmosphere filled with TCs. In reality, and in this model when run over realistic boundary conditions on a rotating sphere, TCs are very far from being so all-pervasive. This seems partly to be due to the very long lifetimes of the vortices in this model. Nearly all of the storms in the video survive over the month shown. There is very little merging of vortices. And there is, by construction, no movement of vortices over land, cutting off their energy supply, or poleward drift into midlatitudes followed by being torn apart by jets and extratropical storms. (This poleward drift is due in large part to the increase in strength of the Coriolis parameter with latitude on a rotating sphere, a gradient not present in our f-plane setup.) The storms in rotating RCE just pile up, to a first approximation, until the occasional decay/merger is balanced by the occasional new storm managing to squeeze in and grab enough of the energy source at the surface.

In addition, if one can get far enough away from the influence of other storms the homogeneous environment here is always conducive to the genesis of new storms. There are no strong vertical shears of the large scale horizontal winds, or large-scale dry-air intrusions, and no SSTs that are too cold to allow convection up to the tropopause. All of these suppression mechanisms result from large-scale horizontal inhomogeneities.

Rotating RCE produces a distinctive kind of turbulence, dominated by vortices of one sign that are strongly dissipative and dependent for their survival on continuous access to their energy source. Are there analogies to turbulent flows that arise in other contexts?

Whenever setting up an idealized model like this you have to ask if detailed study would really help us understand nature. My intuition is that Rotating RCE will turn out to be very valuable — especially if we can devise clean ways of systematically reintroducing relevant inhomogeneities using the homogeneous case as a starting point.

44 Heat Uptake and Internal Variability – Part II

[Originally posted February 28, 2014]

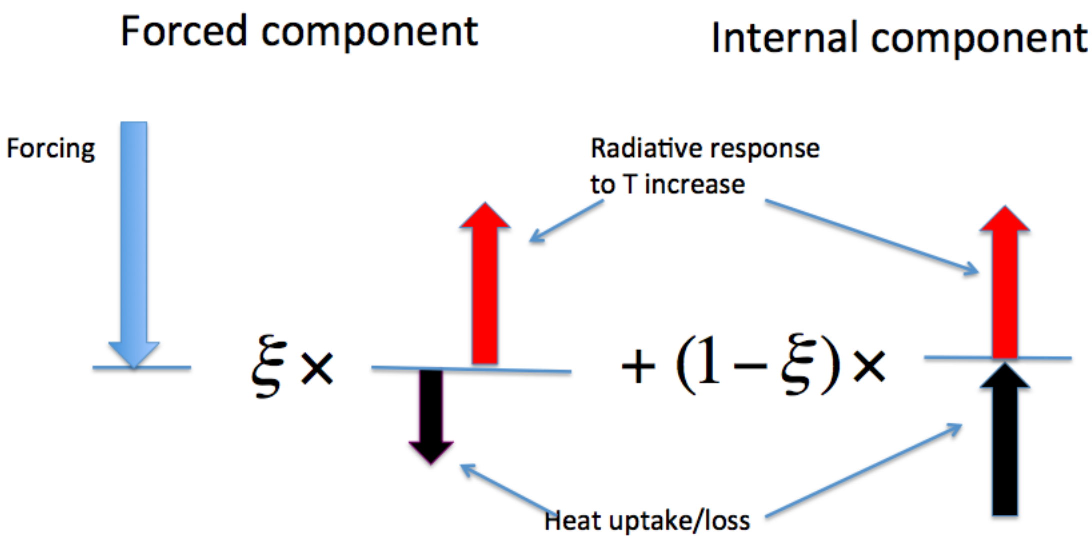

Figure 44.1:

I'm returning to an argument discussed in post 16 regarding the decomposition of the global mean warming into a part that is forced and a part that is due to internal variability. I am not looking here for the optimal way of doing this decomposition. I am just interested in getting a better feeling for whether an increasing ocean heat content over time is a "smoking gun" for the forced component being dominant, a term Jim Hansen and others have used in this context.

I'll assume that we know that the heat flux has been into, not out of, the earth system (ie the oceans) averaged over the period in question, which could be the last half century or any period longer than a decade or two to insure that we can think in terms of a transient climate sensitivity (or transient climate response TCR) for the forced component. (AR5 WG1 Ch. 3 has a synthesis of the observations of ocean heat content). We'll think in the most traditional terms, focusing on the global mean energy balance at the top of the atmosphere (TOA). Everything is considered to be a small perturbation from a control climate, and the assumption is that we can just linearly superpose the forced component of this perturbation and the component due to internal variability.

For the forced component, there is a 3-way balance between forcing F, heat uptake H, and the radiative restoring proportional to the temperature response, $-\beta T$, with strength β inversely proportional to the climate sensitivity $T_{EQ} \equiv F/\beta$. Here and in what follows, F is the change in forcing over the interval considered, so T_{EQ} is the usual sensitivity scaled by F/F_{2XCO_2} When I refer to TCR in the following, it is also normalized in the same way. So TCR is simply the forced response in global mean temperature T_F. H is positive into the ocean. For starters, I'll ignore the question of the efficacy of oceanic heat uptake.

The key assumption is that the relation between global mean temperature and the energy balance of the earth is the same for both the forced and internal components. So an internally generated perturbation in the global mean temperature T_I is accompanied by an increase in the net outward flux at the TOA of βT_I.

Set $T = T_F + T_I$ and similarly for the heat uptake $H = H_F + H_I = H_F - \beta T_I$. We can write the heat uptake in the forced response in terms of the equilibrium sensitivity and the TCR:

$$T_F = TCR = (F - H_F)/\beta \implies H_F = \beta(T_{EQ} - TCR) \tag{44.1}$$

So, adding the forced and internal components for the heat uptake:

$$H = H_F + H_I = \beta(T_{EQ} - T) \tag{44.2}$$

It is the full T that enters here. The heat flux is into the ocean if the equilibrium response is larger than the observed temperature perturbation.

This expression is transparent to the relative magnitude of the forced and free parts of T. For this purpose, as in post 16, we can rewrite $TCR = T_F = \xi T$ so that ξ is the fraction of the temperature anomaly that is forced. And we get

$$H = \beta(T_{EQ} - TCR/\xi) \tag{44.3}$$

One can write this in different ways (the way I chose in 16 being particularly obscure). We can just leave it is this form, from which we see that if the heat flux is into the ocean we must have (given all of our assumptions); $\xi > TCR/T_{EQ}$

This all seems reasonable, but now let's go back and re-examine our key assumption that an internal variation in temperature T_I perturbs the TOA budget by an amount βT_I, with the same value of β that occurs in the forced response. Why should it be the same constant of proportionality, especially if the internal variability has a different spatial structure than the forced response. So how do we relate the strength of this "restoring force" for internal variability to its strength for the forced response? Before getting back to this, we need to reintroduce the notions of efficacy of heat uptake.

For the forced response, when we try to emulate the behavior of GCMs, we find that we need to replace the expression $T_F = TCR = (F - H_F)/\beta$ with

$$T_F = TCR = F/\beta_F - H_F/\beta_H \tag{44.4}$$

The efficacy of heat uptake is defined as $\epsilon \equiv \beta_F/\beta_H$ and is almost always larger than one when emulating GCMs – see Post 5 and Winton et al 2010. This is because the response to heat uptake is typically more polar amplified than the equilibrated response to the forcing, and perturbations at higher latitudes are restored less strongly by radiation to space than those at lower latitudes. So you get more bang for your buck by forcing at high latitudes. (Different parts of the forcing can have different efficacies as well, which is the sense in which this term was first used in this context, but I'll ignore that here.) For a recent example of papers on this, see Rose et al 2014 which looks at the response in some aqua-planet atmospheric models to ocean heat uptake at different latitudes. Like most issues related to radiative responses, clouds feedbacks play an important role and are a major source of uncertainty in ϵ.

Just as for the forced heat uptake, it is natural to expect the radiative restoring of low frequency internal variability to be weaker than that relevant for the equilibrium forced response. Both the forced heat uptake and the low frequency variability involve coupling to deeper ocean layers and this coupling is strongest in subpolar regions. So could it be the case that the restoring for low frequency variability resembles β_H? It might be interesting to see where the assumption $\beta_I = \beta_H$ leads. Setting, $T_{EQ} = F/\beta_F$, we have $H_F = \beta_H(T_{EQ} - T_F)$ and

$$H = H_F + H_I = \beta_H(T_{EQ} - T_F) - \beta_H T_I = \beta_H(T_{EQ} - T) \tag{44.5}$$

So we still have the result that positive heat uptake implies an equilibrium response over the time period in question (ie a temperature change over this period computed by assuming no heat uptake) that is larger than the actual temperature change. Expressing this in terms of the transient response we once again get the result that to be consistent with positive heat uptake we need $\xi > TCR/T_{EQ}$. When efficacy is not equal to one, the assumption that $\beta_I = \beta_H$ saves these intuitive and simple expressions.

Does $\beta_I = \beta_H$ hold in GCMs? How does the strength of the radiative restoring resulting from low frequency internal variability relate to that in the model's response to heat uptake in the forced response? The smaller β_I the weaker the constraint on ξ. There is no reason to expect close agreement; there are undoubtedly different parts of the internal variability — focused on Northern compared to Southern subpolar latitudes, for example — that could be damped differently. But it would be interesting if β_I was at least correlated with β_H across models.

45 Dynamic Retardation of Tropical Warming

[Originally posted April 24, 2014]

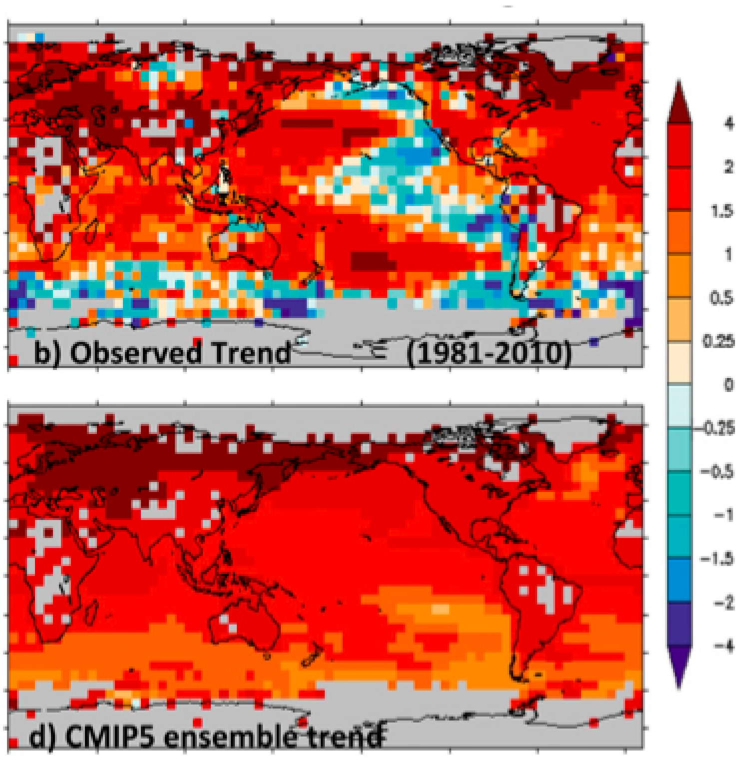

Figure 45.1: Observed (HADCRUT4) surface temperature trends from 1980-2010, compared to the estimate of the forced response over this time frame obtained from the multi-model mean of the CMIP5 models. From Knutson et al 2013.

In the late 80's, Mark Cane, Steve Zebiak and colleagues wrote a series of papers – Zebiak and Cane 1987 is one of the first – about a simple oscillatory atmosphere-ocean model of the tropical Pacific, with the goal of capturing the essence of ENSO evolution and providing dynamical predictions of ENSO. This model was then subjected to a forced warming tendency in Clement et al 1996, and showed that it then evolves towards a state that favors La Nina, and a cold Eastern Pacific, over the warm El Nino state.

It is easy enough to understand why the Cane-Zebiak model tilts towards la Nina as it warms. On the ocean side, this is a model of the waters above the thermocline in the equatorial Pacific. Crucially, the temperature of the water upwelling into this layer from below is fixed as a boundary condition. Most of the upwelling occurs in the eastern Pacific. When the waters of the surface layer are warmed, the upwelling of water from deeper layers, assumed to be unaffected by the warming, retards the warming in the East but not the West, increasing the east-west temperature gradient across the Pacific. One can then envision the basic mechanism underlying ENSO kicking in to enhance the temperature gradient.

Known as the Bjerknes feedback, a stronger east-west temperature gradient generates a precipitation distribution (more rain in the west, less in the east) that enhances the strength of the trade winds along the equator, pushing surface waters westward and enhancing the upwelling of cold waters in the east. The manner in which different negative feedbacks then develop due to slower transfers of heat between equatorial and off-equatorial waters is a main focus of ENSO theory, and these complicate matters, but presumably you can still think of la Nina conditions as being favored by upwelling waters that have not yet experienced warming.

The path taken by this water that upwells in the eastern Pacific is intricate. The major pathway is part of the shallow wind-driven overturning circulation. Subduction and last contact with the surface is primarily in the subtropics, mostly in the eastern half of the basin from where water masses can more easily drift westward and equatorward, typically reaching the western boundary first, where they proceed equatorward below the surface, eventually feeding the equatorial undercurrent which rises as it moves back eastward, mixing with surface waters in the east. An early paper describing the theory and modeling of this circulation is McCreary and Lu 1994. See also the schematic in Fig. 3 of England et al 2014. I was skeptical of the Clement et al result when it came out because of the extreme assumption that the upwelling waters are assumed not to have warmed at all. The time-scale of this shallow circulation is at most a decade or two, so one would have to visualize this modest delay being large enough to drive the system preferentially towards la Nina.

Waters subducted further polewards than the subtropics can also move equatorward and get caught up in the equatorial undercurrent and coastal (Peruvian) upwelling. Radiocarbon in tropical corals – Toggweiler et al 1991 – suggests that these denser source waters come from as far away as the Southern ocean north of the circumpolar current. This would lengthen the time lag, and maybe make it more plausible that the subsurface plumbing that emerges in our ocean models might be deficient.

Models don't typically generate a la Nina like forced response, as seen in the figure at the top. The discrepancy does not just affect the usual hiatus period, the past 15 or so years, but as shown in the figure it affects trends over the full satellite era (causing the discrepancy between models' and satellite (MSU) estimates of tropical tropospheric warming trends among other things). One possibility of course is that internal variability is the cause of this discrepancy between the observed and the forced component of the model trends. But the question here is whether the models could be missing a la Nina-like tendency in their forced responses.

In Held et al 2010, we tried to separate the response in our CM2.1 model, (in an ensemble of 20th century +A1B scenario simulations with stabilization of forcing agents after 2100) into fast and slow components with different spatial structures. We did this by returning all anthropogenic forcing agents to their pre-industrial values instantaneously at three times (2100, 2200, 2300). In response there is a fast cooling with e-folding time of just a few years, followed by a much slower "recalcitrant" cooling back to the pre-industrial climate. The slow part is computed by looking at what's left 20 years following the instantaneous return to pre-industrial forcing, long enough for the fast part to have decayed away. The slow component can be thought of as the effect of the warming of the sub-surface waters on surface temperatures.

The upshot is that the temperature response at any time is decomposed into two components, $T(x, y, t) = a_1(t)f_1(x, y) + a_2(t)f_2(x, y)$. The patterns f_1 and f_2 are normalized to integrate to unity over the sphere, so that the global mean temperature is $a_1(t) + a_2(t)$. Post 8 discusses the magnitudes of these two components. Their spatial patterns f_1 and f_2 are shown below . (We didn't try to estimate the slow part at 2100 because its amplitude is too small to get a good handle on its spatial structure — we were only using a single realization.)

The fast part resembles la Nina, with larger warming in the western than in the eastern tropical Pacific. The slow part provides a complimentary El-Nino like pattern, more or less as one would expect from the dynamic retardation argument of Clement et al (I am avoiding the word "thermostat" because this mechanism is not maintaining a particular temperature.) You also get the sense of the different tropical responses imprinting themselves

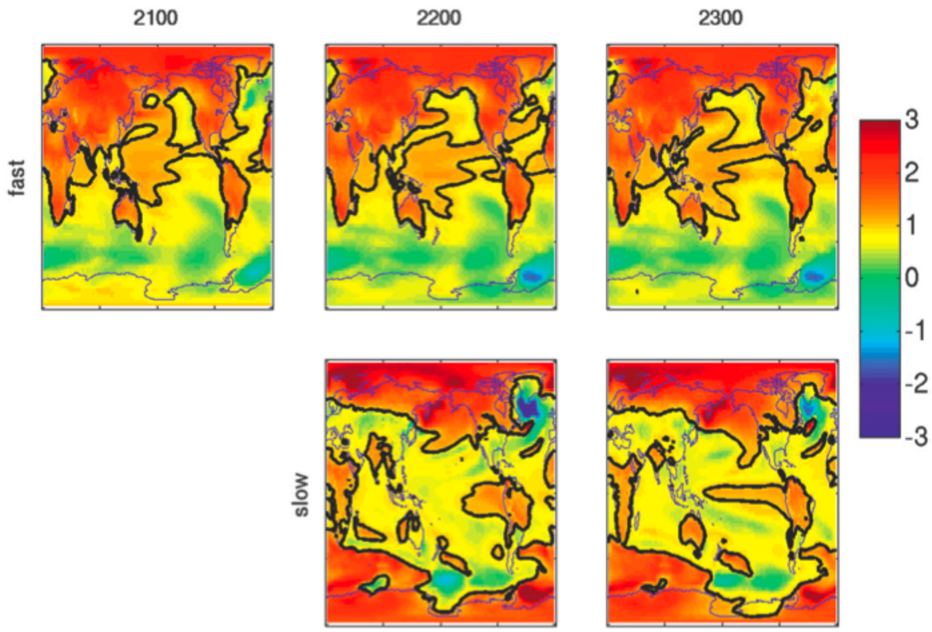

Figure 45.2:

on the North Pacific as expected from the known responses to ENSO. I am not sure why this distinction between the equatorial Pacific structure of the fast and slow responses shows up clearly here and not so clearly within the 20th century part of these simulations, which should be dominated by the fast response. (The oversimplification of there being only two effective time scales is probably to blame — ie, some of the equatorial response in the slow component may not be as slow as the global mean recalcitrant component discussed in post 8.) I am pretty confused about the whole range of issues related to forced responses and free multi-decadal variability in the tropical Pacific. But maybe there is something to the simple idea that when warming starts kicking in rapidly enough, the eastern equatorial Pacific holds it back temporarily.

46 How Can Outgoing Longwave Flux Increase as CO2 Increases?

[Originally posted May 31, 2014]

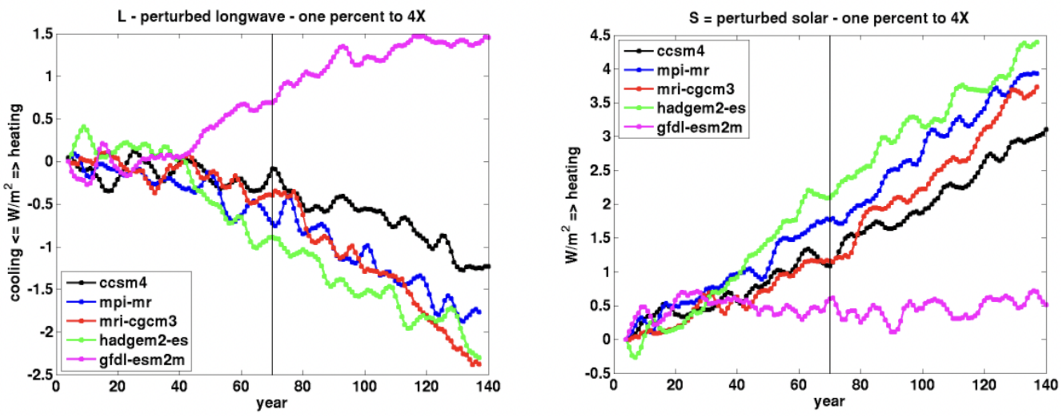

Figure 46.1: Evolution in time of fluxes at the top of the atmosphere (TOA) in several GCMs running the standard scenario in which CO_2 is increased at the rate of 1%/yr until the concentration has quadrupled.

A classic way of comparing one climate model to another is to first generate a stable control climate with fixed CO2 and then perturb this control by increasing CO_2 at the rate of 1%/yr. It takes 70 years to double and 140 years to quadruple the concentration. I am focusing here on how the global mean longwave flux at the TOA changes in time.

For this figure I've picked off a few model simulations from the CMIP5 archive (just one realization per model), computed annual means and then used a 7 yr triangular smoother to knock down ENSO noise, and plotted the global mean short and long wave TOA fluxes as perturbations from the start of this smoothed series. The longwave (L) and shortwave (S) perturbations are both considered positive when directed into the system, so $N = L + S$ is the net heating. The only external forcing agent that is changing here is CO_2, which (in isolation from the effects of the changing climate on the radiative fluxes) acts to heat the system by decreasing the outgoing longwave radiation (increasing L). But in most of these models, L is actually decreasing over time, cooling the atmosphere-ocean system. It is an increase in the net incoming shortwave (S) that appears to be heating the system — in all but one case. This qualitative result is common in GCMs. I have encountered several confusing discussions of this behavior recently, motivating this post. Also, the ESM2M model that is an outlier here is very closely related to the CM2.1 model that I have looked at quite a bit, so I am interested in its outlier status.

Since the radiative forcing due to CO_2 is logarithmic over this range, the radiative forcing increases linearly in time. Global mean surface temperatures $T(t)$ also increase roughly linearly in time, as does the heat uptake $N(t)$, as seen in the following:

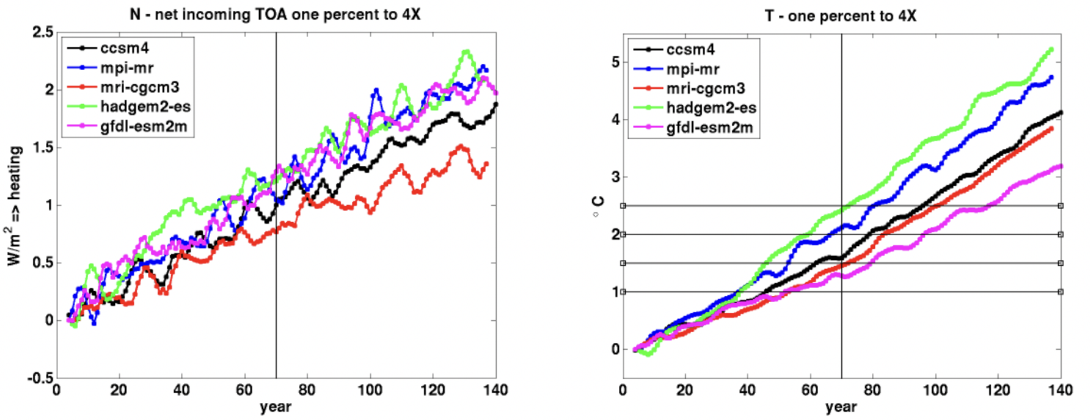

Figure 46.2:

Another reason that I am interested in this comparison is that ESM2M has a low transient climate response ($\approx 1.3C$ warming at the time of doubling) that I like for a variety of reasons. When thinking about this sort of thing, I tend to start with the energy balance of the ocean mixed layer, the surface layer of the ocean that is well-mixed by turbulence generated at the surface. Globally averaged we can think of this layer as being something like 50m deep, providing a heat capacity that is more than an order of magnitude larger than the atmosphere. Ignoring the latter, we can think of $N = L + S$ as heating this layer directly. This surface layer is cooled by transfer of heat H to deeper layers of the ocean:

$$c\frac{\partial T}{\partial t} = L + S - H \tag{46.1}$$

On the time scales of interest here the heat capacity of this layer is itself negligible and we can ignore the time-derivative in this equation, so that $H \approx L + S = N$. For small perturbations, I'll assume that $L = F_L - \beta_L T$ where F_L is the CO2 forcing and β_L is the sensitivity of the longwave flux to temperature. We could also write $S = F_S - \beta_S T$ in general, but in this case of CO2 forcing only , F_S is small and we can think of S as pure feedback.

Importantly for this discussion, I am also going to write $H = N = \gamma T$. γ is referred to as the efficiency of the heat uptake — the heat uptake per unit global warming. This allows us to define a transient climate response very easily — solving for T: $T = F_L/(\beta_L + \beta_S + \gamma)$ In previous posts, I have referred to the time scales of the forcing for which this is a useful first approximation as the intermediate regime (this hasn't caught on — maybe I should try something else) — intermediate between the faster time scales (due to volcanoes for example) for which the heat capacity of the mixed layer is important and the slower time scales over which the deeper ocean starts to equilibrate. With these sign conventions, β_L and γ are positive, while β_S is negative if shortwave feedback is positive (sorry).

If all of these coefficients are constant in time over these 140 years of simulation, and given our other approximations, we expect T and H to both increase linearly in time, as is roughly the case in these models. (Actually, T is typically a bit concave upwards while H is a bit concave downwards, but I think the simplest model is adequate here even if it can only fit these curves to the extent that they are linear in time.) Solving for L,

$$L = F_L - \beta_L T = F_L(\beta_S + \gamma)/(\beta_L + \beta_S + \gamma) \tag{46.2}$$

Whether L increases or decreases in time — that is, whether the forcing wins or the response to increasing temperatures wins — depends on the sign of $\beta_S + \gamma$. If the positive shortwave feedback is larger in magnitude than the efficiency of the heat uptake, L decrease as F increases. To create this counterintuitive behavior the short wave feedback does not have to compete with β_L. It need only compete with γ.

Averaging over the models (leaving aside ESM2M), and looking at the values averaged over years 60-80, at the time of doubling, I get $\gamma \approx 0.55$ in inits of $W/(m^2\,{}^\circ C)$. It is closer to 0.9 in ESM2M. The corresponding mean value of β_S is about -0.85 (and -0.3 in ESM2M). Assuming that F_L at the time of doubling is $3.5W/m^2$, I get $\beta_L \approx 2.1W/(m^2\,{}^\circ C)$ (with ESM2M roughly 2.2, so nothing special there.) Most of the spread among models in the shortwave feedback is undoubtedly due to clouds, but there is a non-cloud related background positive shortwave feedback, partly due to surface (snow and ice) albedo feedback and partly due to positive short wave water vapor feedback. The latter does not get mentioned much because it is often lumped together with the larger infrared feedback, but it accounts for something like 15% of the total water vapor feedback (water vapor absorbs solar radiation, reducing the amount of solar radiation reaching the surface, so more vapor mean means less reflection from the surface and less loss of energy to space through this reflection.) The surface albedo and water vapor shortwave feedbacks are probably enough in themselves to compete with γ. In ESM2M negative short wave cloud feedbacks bring the magnitude of β_S down and γ is relatively large, resulting in the intuitive response — the outgoing longwave decreasing with time with increasing CO_2.

Although it is not directly relevant to the simulations described above, it is interesting to consider the special case in which there is some positive solar forcing added to the positive longwave forcing. For simplicity, let's just assume that there is no shortwave feedback, so $S = F_S$ (we still have long wave feedback of course). The temperatures will increase if $F_L + F_S$ is positive, and this warming must be due to positive $L + S$ in our simple model (assuming once again that we are in the intermediate regime). But is it L or S that looks like it is causing the warming? A manipulation similar to that above shows that $L \propto \gamma F_L - \beta_L F_S$. So if the shortwave forcing is larger than γ/β_L times the longwave forcing — this ratio is something like 25% in the main group of models that we looked at above — the system is being heated by the shortwave rather than the longwave flux even though the shortwave forcing might be much smaller than the longwave forcing.

I guess the moral here, if there is one, is that it is useful to have an explicit model in mind, however simple, when thinking about the Earth's energy balance and its relationship with surface temperature.

47 Relative Humidity over the Oceans

[Originally posted June 26, 2014]

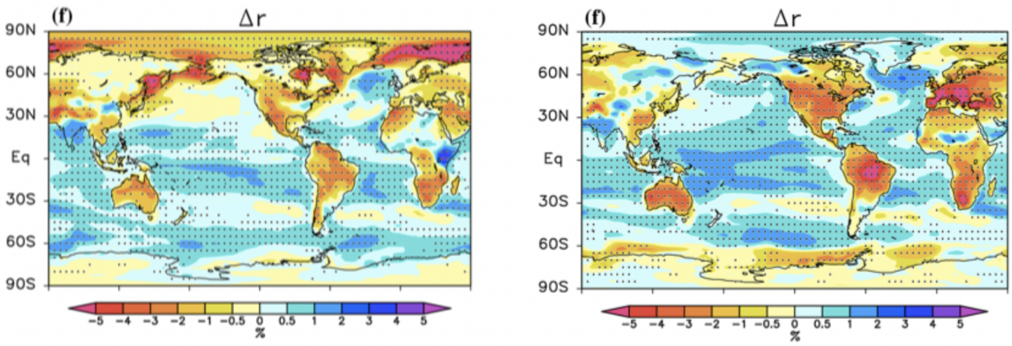

Figure 47.1: The change in near surface relative humidity averaged over CMIP5 models over the 21st century in the RCP4.5 scenario. Dec-Jan-Feb is on the left and June-July-Aug on the right. From Laine et al 2014.

We expect the amount of water vapor in the atmosphere to increase as the atmosphere warms. The physical constraints that lead us to expect this are particularly strong in the atmospheric boundary layer over the oceans. The relative humidity (RH — the ratio of the actual vapor pressure to the saturation value) at the standard height of 2 meters is roughly 0.80 over the oceans. At typical temperatures near the surface, the fractional increase in the saturation vapor pressure per degree C warming is about 7%. So RH would decrease by about the same fraction, amounting to roughly 0.06 per degree C of warming if the water vapor near the surface did not increase at all. Why isn't it possible for RH to decrease by this seemingly modest amount?

The figure shows what CMIP5 models predict will happen to RH near the surface by the end of the present century in the RCP4.5 scenario. In this scenario, which requires major mitigation efforts by mid-century, these models warm the tropics by about 1.6C on average, so fixed vapor concentrations would result in a decrease in RH of about 0.10. (I am avoiding expressing RH as a percentage to avoid having to talk about percentages of percentages.) The figure at the top summarizing climate model simulations shows RH over the oceans increasing by a modest amount, something like 0.01. (Over most land surfaces, RH is predicted to decrease — this is important, but I am going to focus on the oceans here since this is where most evaporation occurs.) So, to first order we can say that RH over the oceans does not change much in these simulations, relative to the decrease that would occur at fixed vapor concentrations. To second order RH near the surface over the oceans actually increases modestly.

To understand the first order picture, we need two pieces of information, one regarding the global energy balance of the troposphere and other regarding how the strength of the global hydrological cycle is related to near-surface RH.

The tropospheric energy balance to first order is a balance between radiative cooling and the release of latent heat when water vapor condenses. In the global mean there is roughly 80 W/m2 of latent heating. The change in this number in global climate models is typically only 1 or maybe 1.5 W/m2 increase per degree C warming in 1%/yr transient CO_2 simulations (Pendergrass and Hartmann 2014), or at most 2% per degree C warming. Pendergrass and Hartmann provide a nice deconstruction of this number, as does Prevedi 2010. There is a lot of literature on this energetic constraint on the strength of the hydrological cycle, starting, I think, with Mitchell et al 1987 and Betts and Ridgeway 1988. Aerosols — especially absorbing aerosols — can change things quantitatively quite a bit. But for our first order picture we only need to know not to expect large fractional changes in global mean evaporation or precipitation given the modest fractional changes in atmospheric radiative cooling involved.

The second point to appreciate is that the evaporation is controlled by the degree of sub-saturation of the air near the surface — roughly speaking by (1-RH) rather than RH itself. The air in contact with the ocean surface is saturated and it is the gradient in the concentration of water vapor between this surface air and the air near the surface that drives evaporation. If the relative humidity at the reference level is 0.80, the sub-saturation, 1-RH, is 0.20 and a reduction in relative humidity from 0.8 to 0.7 (as would be consistent with fixed vapor concentration in the warming simulation pictured above) would result in a 50%(!) increase in (1-RH). A 50% increase in evaporation is obviously ruled out by energy balance requirements. So we expect small changes in RH near the surface as the climate warms.

More precisely, evaporation E over the oceans can be approximated by the "bulk formula"

$$E = (\rho C V)[q_S(T_O) - RH q_S(T_A)] \tag{47.1}$$

Here $q_S(T_O)$ and $q_S(T_A)$ are the saturation humidities at the ocean surface and reference level temperatures respectively, RH and V are the relative humidity and wind speed at this reference level, ρ the atmospheric density and C a non-dimensional constant. A lot of physics and a lot of empirical evidence has been stuffed into the constant C, guided by what is affectionately known as Monin-Obukhov similarity theory. (All global climate models compute surface fluxes using Monin-Obukhov scaling as the starting point.) C depends on the height of the reference level, some properties of the surface (specifically surface "roughnesses"), and the gravitational stability of the atmosphere near the surface, which in turn is strongly coupled to the air-sea temperature difference.

If we ignore the air sea temperature difference $T_O - T_A$ as well as changes in wind speed and C, then we just have $E \propto (1 - RH) q_S(T_A)$. If the specific humidity does not change, then the large fractional reduction in 1-RH results in a huge increase in evaporation, as discussed above. But it even worse than that, because $q_S(T_A)$ will also increase by about 7%/C on top of the effect of the change in RH.

Can the other factors in the expression for evaporation compensate somehow? The changes in tropical weather would have to be profound to produce reductions in average wind speed large enough to compensate for a such a large increase in 1-RH. Fortunately, no models even hint at such profound changes. We can rewrite the expression for the evaporation as

$$E \approx (\rho C V)[q_S(T_O) - q_S(T_A)) + (1 - RH) q_S(T_A)] \tag{47.2}$$

and therefore

$$E \approx (\rho C V)[\partial q_S/\partial T)(T_O - T_A) + (1 - RH) q_S(T_A)] \tag{47.3}$$

For the term proportional to $T_O - T_A$ to compensate for the large reduction in RH this air-sea temperature difference would have to change sign, since the temperature difference is small — only +1 to +2C over the tropical oceans. But this temperature difference is itself constrained by an energy balance argument, as discussed by Betts and Ridgeway. [Due to mixing of water vapor in the turbulent boundary layer, the specific humidity is relatively homogeneous with height in this layer while temperatures decrease with height, so we often reach a point at which saturation occurs within the boundary layer, the cloud base. Latent heat release comes into play only above this level; something has to balance the radiative cooling below cloud base and it is the sensible heat transfer from the surface, proportional to the air-sea temperature difference, which has to pick up the slack.] And it is also extremely implausible that the value of C could cause this magnitude of an adjustment in evaporation (the easiest way of changing C is to change the air-sea temperature difference a lot). Something very dramatic would have to happen in the tropical atmosphere to avoid the constraint that near surface water vapor over the ocean must increase as the surface warms to maintain nearly constant relative humidity.

As for the second order picture, the small increase in RH over the oceans, note that the term $\propto (1 - RH)q_S(T_A)$ would result in an increase in evaporation of 7% per degree C warming even if the relative humidity were fixed, and that this increase is already too large to be consistent with the energy constraint. An increase in RH of about 0.01, that is, a decrease in 1-RH of about 5%, is about the right order of magnitude to restore consistency. This seems to be part of what is going on in the CMIP5 composite at the top of the post. But now the changes are small enough that reduction in average wind speed and modest change in air-sea temperature difference could also play a role, as they seem to do in models. However, the models do seem to take advantage of the simplest way of throttling back the evaporation — a small increase in RH.

This near surface relative humidity is not just relevant for the lowest few meters of the atmosphere, since these near surface values are coherent with the humidity of the entire planetary boundary layer — the lowest 1-2 kms of the troposphere — because of the strong turbulent mixing throughout this layer. While the boundary layer is not where most water vapor feedback originates, it does contain a large fraction of the mass of water vapor. The increase in total mass of water vapor with warming has lots of consequences — for example, for the increase in the amplitude of the pattern of evaporation minus precipitation discussed in Posts 13 and 14.

48 Increasing Vertically Integrated Water Vapor over the Oceans

[originally posted July 1 2014]

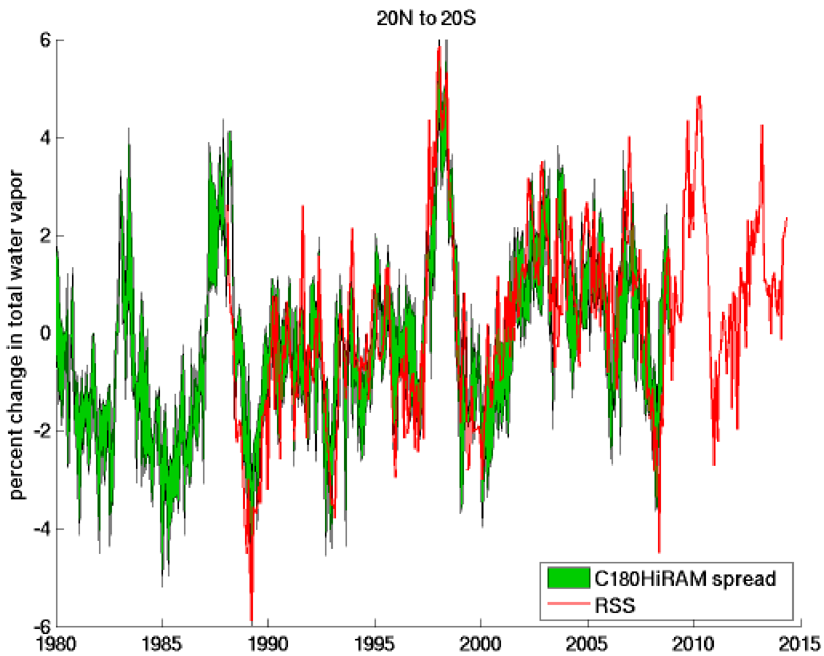

Figure 48.1: Percentage changes in total water vapor, vertically integrated and averaged over 20S-20N over the oceans only, comparing the RSS microwave satellite product (red) to the output of an atmospheric model running over prescribed sea surfaace temperatures (HADISST).

This is a continuation of the discussion in the previous post regarding the increase in water vapor near the surface and within the boundary layer more generally as the ocean surface warms. Models very robustly maintain more or less constant relative humidity in these lower tropospheric layers over the oceans as they warm, basically due to the constraint imposed by the energy balance of the troposphere on the strength of the hydrological cycle, and the tight coupling between the latter and the low level relative humidity over the oceans. Do we have observational evidence for this behavior? The answer is a definitive yes, as indicated by the plot above of microwave measurements of total column water vapor compared to model simulations of the same quantity. These are monthly means of water vapor integrated in the vertical. The observations are RSS Total Precipitable Water Product V7. The model is the 50km resolution version of GFDL's HiRAM discussed in previous posts (on hurricane simulations, MSU trends, and land surface temperature trends) which uses observed sea surface temperatures (SST)s and sea ice extent from HADISST1 for the lower boundary condition. Data are monthly means, deseasonalized by removing each month's climatology defined by averaging over 1988-2007, and plotted as percentage changes from the climatological average over the same domain. The model results plotted are the spread of three realizations with different initial conditions (these are the runs from this model deposited in the CMIP5 database).

This is not asking a lot of the model. One gets about the same quality of fit for tropical averages by using the SSTs directly, instead of the model's water vapor, and multiplying by 7%/C, roughly the fixed relative humidity value — see Mears et al 2007. Any model that strongly couples the SST and the humidity in the lowest 2 kms of the atmosphere so as to maintain roughly fixed relative humidity would give this result. Fully coupled models, although they would not duplicate all of the interannual features of this time series of course, still give the same tight relation between SST and total water vapor. So this clearly forces us to take seriously the implications (of which there are many) of this increase in lower tropospheric water vapor as it amplifies in the future. It also gives us some extra confidence in the SST data in addition.

The observational estimate of the observed total water vapor over this tropical ocean domain is 41.75 Kg/m2. In the model, averaging over the 3 realizations I get 40.51, with negligible standard deviation across the ensemble. Putting aside any issues with absolute calibration of the satellite sensors and more mundane things such as consistent land-sea masks, let's accept that the model is biased low 3%. This is of the same magnitude as the trends over the satellite era! Should we trust the result at all? Perhaps the radiative cooling is a bit too strong in the model's troposphere, causing near-surface humidity to drop a bit so as to supply the required evaporation. Or maybe the boundary layer is 3% too shallow, reducing the vertical integral. Or even more likely, the bias if real is due to a combination of these and other small model deficiencies. Does this bias matter?

I have a hard time seeing how it does matter. We are admittedly typically interested in the absolute increases in water vapor, not the fractional increase. If the fractional changes in water with surface warming are ok, as the figure suggests, this bias suggests that the change in water vapor would also be underestimated by 3% — that is by 3% of 7%, or 6.8% rather than 7% per degree. I think we can agree that there are bigger fish to fry.

Absolute biases of this kind are easy to find in models, and are often the target of critics. But you have to have a cogent argument why a particular bias matters. There are some absolute biases that do matter, of course, but comparison of the size of the bias to the size of the response in question may not be the most relevant criterion for whether a bias matters or not.

Water vapor feedback is only weakly related to the vertically averaged water vapor discussed here. There are some frequencies, particularly those associated with what is known as the water vapor continuum, where the infrared emission from the lower troposphere reaches the tropopause, and a lot of the feedback due to solar absorption by water vapor comes from optically thin lines as well, but these don't add up to a major fraction of the full water vapor feedback that you get from a model that maintains more or less constant relative humidity in the upper as well as lower troposphere. Do these results indirectly increase our confidence that the physics of the tropical upper tropospheric water vapor feedback is well simulated in our models?

I don"t think so. At a given level in the tropics above the boundary layer, water vapor concentrations are more closely related to what is going on above, not below, this level. Water is saturated by upward motion in the tropics, but most of this upward motion takes place in a small fraction of the total tropical area — even if this area changed it would not change area averaged relative humidity that much — and in these regions clouds tend to prevent the infrared emission by water vapor from reaching the tropopause anyway. What matter more is the humidity in the non-convecting drier areas. The relative humidity in those areas is determined by the previous history of air parcels arriving at the level in question, to first approximation. What was the temperature (and the pressure) at the higher levels at which these parcels were last saturated? This saturation event sets the mixing ratio of water vapor to dry air that is then conserved as the parcel descends. This has little to do with the humidity in the lower troposphere that dominates the vertical integral.

To finish up, here is a plot analogous to the one at the top, but for the Northern extratropics, from 20N to 45N, once again over oceans only. I have used a 5-month (1-2-3-2-1) smoother here to knock down the noise for aesthetic reasons.

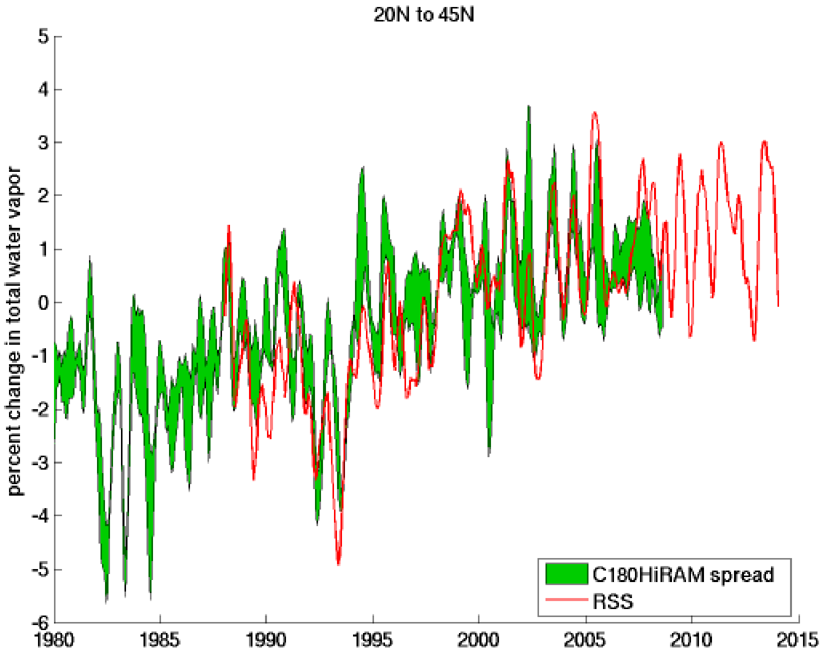

Figure 48.2:

(The model bias is about the same here as in the tropics, about 2-3% low.) The agreement is pretty good here as well. This region is interesting because this is where midlatitude eddies are transporting water polewards systematically, with poleward moving moist air and equatorward moving dry air. The increase in poleward transport due just to this increase in vapor, without any change in the eddies themselves, causes an increased poleward transport of water. The divergence of this transport must be balanced by evaporation (E) minus precipitation (P) — so the eddies, by sucking water out of the subtropics, are reducing P -E there, simultaneously increasing P-E on the poleward side of the storm tracks — qualitatively consistent with the salinity trends discussed in post 14. (This figure also provides a slightly different perspective on the hiatus, without as strong an influence of ENSO variability.)

49 Volcanoes and the Transient Climate Response - Part I

[Originally posted August 2 2014]

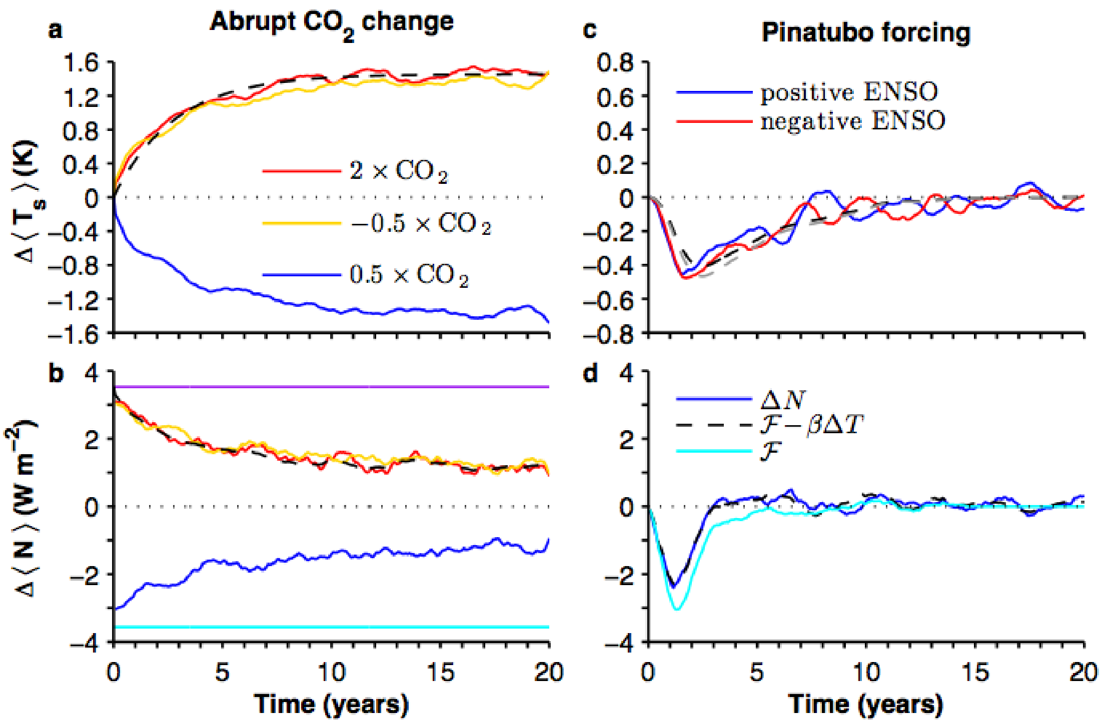

Figure 49.1: Some results on the response of a GCM (GFDL's CM2.1) to instantaneous doubling or halving of CO_2 (left) and to an estimate of the stratospheric aerosols from the Pinatubo eruption. From Merlis et al 2014.

The following is based on the recent paper by Merlis et al 2014 on inferring the Transient Climate Response (TCR) from the cooling due to the aerosols from a volcanic eruption. The TCR is the warming in global mean surface temperature in a model at the time of doubling of CO2 when the CO2 is increasing at 1% per year. You can generally convert the TCR of a model into a good estimate of the model's warming due to the CO2 increase from the mid-19th century to the present, or due to all of the well-mixed greenhouse gases, by normalizing the TCR by the appropriate radiative forcing. The TCR of GFDL's CM2.1 model, one of the two models discussed in Merlis et al, is 1.5K. Can you retrieve this value by looking at the model's response to Pinatubo? This paper was motivated by the feeling that the literature trying to connect volcanic responses to climate sensitivity has focused too much on equilibrium sensitivity rather than directly constraining the TCR.

Another simulation that has become standard for models is to just double (or quadruple) the CO2 instantaneously and watch the system equilibrate. This gives you more information about the various time scales involved in the equilibration. For CM2.1, the upper left panel shows the evolution over the first 20 years (this is an ensemble mean over 10 realizations with different initial conditions taken from different times in a control run). It also shows a fit with a function of the form:

$$T(t) = \mathcal{F}_{2X}\alpha_1[1 - \exp(-t/\tau_1)] \tag{49.1}$$

with $\mathcal{F}_{2X}$ the radiative forcing due to doubling of CO_2 (3.5 W/m2 here — all radiative forcings are computed by holding SSTs fixed, perturbing the system, letting the atmosphere+land equilibrate, and examining the imbalance at the top-of-atmosphere), $\mathcal{F}_{2X}\alpha_1 = 1.45K$ and $\tau_1 = 2.8yrs$. In previous posts, I have discussed how one can interpret this short-time scale response in terms of a simple box model for the surface layers of the ocean,

$$c\frac{dT}{dt} = \mathcal{F}(t) - \beta T - \mathcal{H} \tag{49.2}$$

where $\mathcal{F}$ is the radiative forcing, β is the strength of the radiative restoring, taking into account all of the radiative feedbacks, and $\mathcal{H}$ is the heat uptake into the deeper layers of the ocean. If we set $\mathcal{H} = \gamma T$ with γ the heat uptake efficiency, the heat uptake acts as an additional negative feedback. We then have $\alpha_1 = 1/(\beta + \gamma)$.

The figure also shows the mean of an ensemble of runs with an instantaneous reduction in CO_2 by a factor of 2. There is a small difference in the ensemble mean, marginally significant at the 10% level, between these warming and cooling switch-on simulations, with $\mathcal{F}_{2X}\alpha_1 \approx 1.35K$ fitting the cooling case. (We checked that the radiative forcing is almost exactly logarithmic in the model, so we can use the same forcing for doubling and halving). This qualitative result might be expected from the picture that cooling at the surface reduces the gravitational stability of the water column, increasing the heat uptake efficiency. But the difference between warming and cooling is small on this time scale, which is nice from the perspective of using a cooling perturbation like a volcanic eruption to infer the transient response to warming.

The model is still taking up heat at about $1W/m^2$ after 20 years (lower left panel). This model's equilibrium climate sensitivity is about 3.4K, but it approaches this equilibrium very slowly. Fitting the evolution over longer time scales with a sum of two exponentials,

$$T(t) = \mathcal{F}_{2X}[\alpha_1[1 - \exp(-t/\tau_1)] + \alpha_2[1 - \exp(-t/\tau_2)]] \tag{49.3}$$

we get something like $\mathcal{F}_{2X}\alpha_2 = 1.9K$ and $\tau_2 \approx 400 - 500yrs$. This large gap in time scales is clearly the best possible situation if you want to infer a response on the time scale of 50-100 years from the response to a much shorter time-scale forcing. See Geoffroy et al 2013 to place the shape of this response function in the context of that found in other GCMs.

As a first approximation to a volcano, we can set $\mathcal{F}$ to be a spike, a δ-function, with the result

$$T(t) = \tilde{\mathcal{F}} \sum_{i=1,2} \alpha_i/\tau_i \exp(-t/\tau_i) \tag{49.4}$$

where $\tilde{\mathcal{F}}$ is the integral of the volcanic radiative forcing, $\int \mathcal{F}dt$, which we might call the volcanic radiative impulse. The simple but important point to notice here is the appearance of the factor $1/\tau_i$. If the magnitudes of the temperature responses to a step increase in forcing on the fast and slow time scales are comparable, the response to impulsive forcing will be much smaller on the long time scale, by the ratio τ_1/τ_2. The long weak tail of the volcanic response has been discussed by Wigley et al 2005. Delworth et al 2005 and Gleckler et al 2006 have discussed the closely related long time scale recovery of sea level after a volcano in GCMs. The surface temperature signal on long time scales in the response to a single volcano is effectively unobservable in CM2.1. But given a sequence of volcanoes, the weak long time scale tail would accumulate.

To the extent that one is able to focus on the fast response in isolation we can average over time, returning to the single box interpretation if you like,

$$\int_0^{t_0} T(t)dt \approx \frac{1}{\beta + \gamma} \int_0^{t_0} \mathcal{F}dt = \frac{1}{\beta + \gamma}\tilde{\mathcal{F}} = \alpha_1\tilde{\mathcal{F}} \tag{49.5}$$

where t_0 is a time long compared to the fast decay and short compared to the slow decay. The setup for computing TCR involves linearly increasing radiative forcing (since this forcing is logarithmic in CO2) for 70 years. For a two-box model mimicking CM2.1, this results in a TCR very accurately given by $\mathcal{F}_{2X}\alpha_1$. So the estimate of TCR provided by the volcano is

$$(\mathcal{F}_{2X}/\tilde{\mathcal{F}})\int_0^{t_o} T(t)dt \tag{49.6}$$

This integral method does not involve an estimate of the time scale of the fast response.

Merlis et al piggyback on the ensemble of simulations of the response in CM2.1 to the Pinatubo eruption described in Stenchikov et al 2009 which used an ensemble of 20 runs, 10 initialized during an El Nino event in the model's control simulation and 10 initialized in La Nina events. (Pinatubo occurred during an El Nino, so it is of interest if this modifies the forced response to the volcano. The response is nominally a bit larger in the La Nina ensemble mean, but larger ensembles would be needed to quantify this difference.) The Pinatubo forcing in this model is shown as the light blue line in panel d above. It's not a δ-function, but its duration is less than the model's dominant fast response time. The volcanic radiative impulse is -6.5 Wm-2-yrs.

The temperature response is shown in panel c. The ensemble mean integrated response up to year 20 is 2.35K-yrs. This gives an estimate of TCR of 1.3K. This is close to the models TCR of 1.5K but a little low. The figure also shows the fit that you get with this one-timescale model, constraining it to fit the integral of the response and using the time scale from the instantaneous doubling simulation. You can also fit the volcanic response varying the two paramaters τ_1 and α_1 simultaneously. This two-parameter fit gives an estimate of TCR that is smaller still — about 1.1K. The single time scale model is not a perfect fit to the GCM response.. One can understand the sensitivity to fitting procedure qualitatively if you assume, for example, that the fast response in the GCM actually occurs on two time scales — let's say 1 year and 4 years, conserving the sum of these two responses and playing with their ratio.

Since the model's TCR is 1.5K, the underestimate 1.1K is not trivial. It is the sum of little things in this model — the slight difference between warming and cooling perturbations, a small effect in this model of time scales longer than the dominant fast response time, plus a distortion due to the fitting procedure when the fast response itself is not well fit with a single time scale.

We do not need to estimate the separate effects of radiative feedbacks and heat uptake, or β and γ, to estimate TCR in this way, and there is no need to refer to equilibrium climate sensitivity.

We have used 20 realizations of the response to Pinatubo to get these results. How is this relevant to the problem of determining TCR from a single realization (and without a no-volcano control)? Merlis et al describes what you get if you take one realization of CM2.1, remove the average of the 10 years before the eruption and also remove an estimate of the ENSO contribution based on the relationship between global mean temperature and NINO3.4 SSTs in this GCM. You have to do something like this to get any meaningful results from a single realization, and we don't claim that this is optimal . We also find it difficult to use the integral method with single realizations, so use the two parameter fitting procedure that results in 1.1K using the ensemble mean response. We get the following:

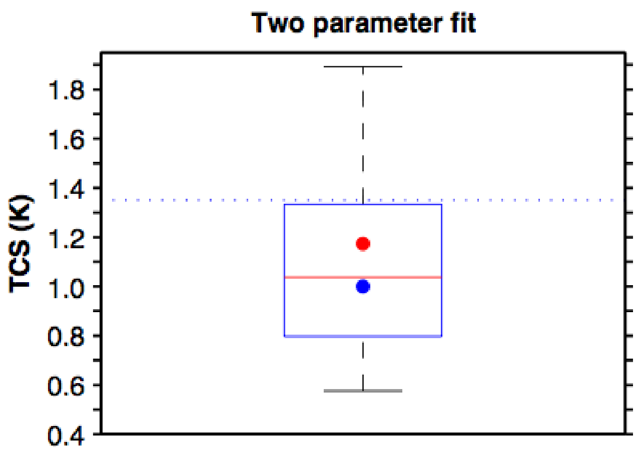

Figure 49.2:

The whiskers span the entire range of values obtained from the 20 realizations, the box represents the middle half (25-75%) and the red line the median (with the red and blue dots corresponding to the La Nina and El Nino ensembles). The median is close to the "correct" value of 1.1K for the two-parameter fit. The blue dotted line indicates the value inferred from fitting to the fast response in the 0.5X instantaneous cooling simulation. My suspicion is that this spread is too large, partly because the interanual variability of global mean surface temperature in this model is too big, mostly due to too large an ENSO amplitude — and partly because you can probably do better than this with a better algorithm, possibly multivariate, for isolating the volcanic signal in a single realization. Even with this much uncertainty, this would be useful as one piece of information among others, if coupled to some theoretical guidance for the bias involved.

There's the rub, I think — because this underestimate could be much larger in reality than in CM2.1 if intermediate time scales play a larger role than they do in this particular model. I'll return to this issue in Part II.

50 Volcanoes and the Transient Climate Response - Part II

[Originally posted September 2 2014]

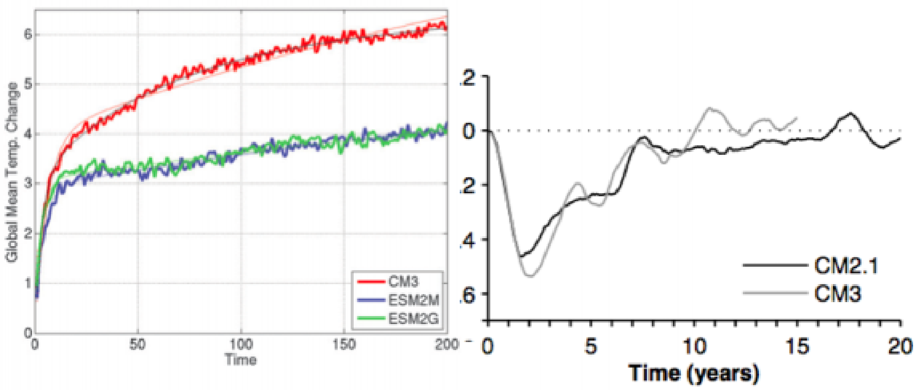

Figure 50.1: Left: The response to instantaneous quadrupling of $CO2$ in three GFDL models, from Winton et al 2013a. Right: The ensemble mean response of global mean surface air temperature to Pinatubo in two GFDL climate models, from Merlis et al 2014

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This is a continuation of post 49 on constraining the transient climate response (TCR) using the cooling resulting from a volcanic eruption, specifically Pinatubo. In order to make this connection, you need some kind of model that relates the volcanic response to the longer time scale response to an increase in CO_2. Our global climate models provide the logical framework for studying this connection. Using simple energy balance or linear response models to emulate the GCM behavior helps us understand what the models are saying. The previous post focused on one particular model, GFDL's CM2.1. The figure on the right (from Merlis et al 2014 once again) compares the response to Pinatubo in CM2.1 with that in CM3, another of our models. The CM2 curve is an average over an ensemble of 20 realizations with different initial conditions; the CM3 curve is an average over 10 realizations. These two models have essentially the same ocean components but their atmospheric components differ in numerous ways. Most importantly for the present discussion, the different treatments of sub-grid moist convection result in CM3 being a more sensitive model to CO2 increase, whether measured by the TCR or the equilibrium response. One sees this difference in sensitivity in the left panel, showing the response to instantaneous quadrupling in three models, one of which is CM3. One of the others, ESM2M, is very closely similar to CM2.1 (it also has the option of simulating an interactive carbon cycle, driven by emissions rather than specified concentrations of CO2, so is referred to as an Earth System Model.) ESM2G has the identical atmospheric component as ESM2M but a different ocean model. As discussed in Winton et al 2013a, the different ocean models have little effect on this particular metric. The analogous simulation with CM2.1 would be very close to the green and blue curves in the left panel. Evidently the temperature responses to Pinatubo are not providing any clear indication that CM3 is the more sensitive model.

What is clear from the left panel is that the large difference between CM3 and the CM2-based models begins to build up between 10 and 50 years after the increase in CO2. The two models are close to each other during the initial (less than 10 year) fast response. Presumably this is why the fast response to the volcanic forcing is similar in the two models. (There is also a slower component to the volcanic response, but as discussed in 49 this is too small to see in the presence of the model's noisy temperatures but is clearly seen in ocean heat content or sea level — Stenchikov et al 2009.) Comparing the time integral of the temperature response over times less than 10 years or so with the integrated volcanic forcing provides a modest underestimate of the TCR in CM2.1, as discussed in 49; in CM3 this underestimate is much more substantial.

Winton et al 2013a provide a three time-scale fit to CM3's response to instantaneous quadrupling of CO2 (their Table 5). Dividing by 2 to convert to doubling of CO2, the result is

$$T(t) = \mathcal{F}_{2X} \sum_{i=1}^{3} \alpha_i [1 - \exp(-t/\tau_i)] \equiv \mathcal{F}_{2X} h(t) \tag{50.1}$$

with $\mathcal{F}_{2X}[\alpha_1, \alpha_2, \alpha_3] = [1.5, 1.3, 1.8]K$ and $[\tau_1, \tau_2, \tau_3] = [3.3, 58, 1242]yrs$, with $\mathcal{F}_{2X} = 3.5W/m2$. The response to a δ-function spike in forcing is obtained by differentiating, $g(t) = dh(t)/dt$,

$$g(t) = \sum_{i=1}^{3} (\alpha_i/\tau_i) \exp(-t/\tau_i) \tag{50.2}$$

For an arbitrary time evolution of the forcing $\mathcal{F}(t)$, you can then write the response as a sum over contributions from the forcing at each earlier time $\xi < t$,

$$T(t) = \int_0^t \mathcal{F}(\xi) g(t-\xi) d\xi \tag{50.3}$$

where the forcing is assumed to vanish for $t < 0$. If you plug in a linearly increasing forcing reaching $\mathcal{F}_{2X}$ at year 70, you get a transient response of about 2.0K. You can get a feeling for how it might be difficult to infer TCR directly from the surface temperature response to a volcanic eruption by playing around with this expression.

The difference in the shapes of the response functions in CM2.1 and CM3 is important, putting aside the difference in sensitivity. A much larger fraction of the response by year 100 is realized in the first 10 years in CM2.1 than in CM3. These different shapes have implications for attribution and near term projection of the forced response. The plateau-ish character of CM2's response is likely related to the behavior of the model's Atlantic Meridional Overturning Circulation (AMOC). AMOC weakens in response to increasing CO2 in almost all models but then typically recovers slowly as the system equilibrates. Weaker AMOC results in colder North Atlantic and colder global mean (warming in the Southern Hemisphere is invariabily weaker than the cooling in the north). Consistently, AMOC strengthens in the CM2.1 Pinatubo simulations (Stenchikov et al 2009). Winton et al 2013b examine a version of ESM2M in which the ocean currents are fixed and compare the response to CO2 (1%/year) in this model with the standard model in which currents, including AMOC, are free to change. The model with fixed currents warms more rapidly on these intermediate time scales. In this picture the plateau is not due to the absence of oceanic adjustment on multi-decadal time scales but due to a cancellation between the effects of the AMOC weakening and a gradual warming and reduction of heat uptake efficiency that would occur with fixed AMOC, as one might expect from something like a diffusive model of heat uptake.

The curious point is that ESM2M and CM3 share the same ocean model. The two models show similar reductions in AMOC in response to a warming perturbation. It is the atmospheres that are different between these two models. One hypothesis is that the different atmospheres respond differently to similar changes in AMOC, due to different cloud feedbacks perhaps, resulting in different shapes to their response functions on multi-decadal time scales. The importance of cloud feedacks for the response to full suppression of AMOC (generated by adding a lot of freshwater to the North Atlantic) is analyzed in CM2.1 in Zhang et al 2010, although the focus there is on the changes in tropical rainfall rather than global mean temperature. If this picture is correct, it is interesting that modeling uncertainty in cloud feedback can result in uncertainty in the time evolution of global mean efficiency of heat uptake.

Once one moves beyond the two-time scale fit to three or more time scales a simple emulator with discrete time scales begins to lose its appeal, as compared to models that start from a picture of vertical diffusion or some other continuous process. The latter would potentially have fewer disposable parameters. In fact, some colleagues have questioned why I haven't started from a diffusive picture in these posts. It doesn't bring us much closer to the underlying physics (ie the vertical diffusivity that one ends up using to emulate GCMs on this 100 year time scale has no simple physical interpretation) but if one has to rely on the response to AMOC to suppress the response on multi-decadal time scales to justify a model with well-separated fast and slow responses, then a diffusive starting point might be more parsimonious. Ill try to return to this topic in a future post.

I realize that a theoretical discussion like this, in which I haven't confronted the model with data on the response to Pinatubo, strikes some readers as unbalanced. But I think we need a theoretical framework to think about how the volcanic responses and TCR are related, from which vantage point we can then think about the implications for TCR estimates of any discrepancies between modeled and observed volcanic responses.

(Thanks to several colleagues, especially Mike Winton, Tim Merlis, and Rong Zhang, for discussions on this topic.)

51 The Simplest Diffusive Model of Oceanic Heat Uptake and TCR

[Originally posted October 17 2014]

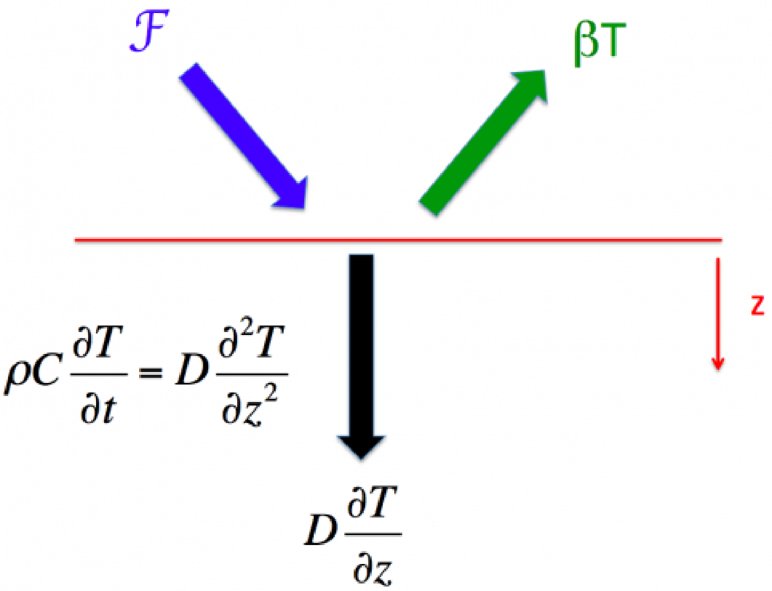


Figure 51.1:

Vertical diffusion of heat has often been used as a starting point for thinking about ocean heat uptake associated with forced climate change. I have chosen instead to use simple box models for this purpose in these posts because they are easier to manipulate but also because I don't feel that the simplest diffusive models bring you much closer to the underlying ocean dynamics. But diffusion does provide a simple way of capturing the qualitative idea that deeper layers of the ocean, and larger heat capacities, become involved more or less continuously as the time scales increase. So let's take a look at the simplest possible diffusive model for the global mean temperature response. This model is typically embellished with a surface box, representing the ocean mixed layer, as well as by some attempt at capturing advective rather than diffusive transport (ie Hoffert, et al 1980, and Wigley and Schlesinger 1985), but let's not worry about that. I want to use this model to make a simple point about the value of the concept of the transient climate response (TCR).

Our equation for the ocean interior is the linear diffusion equation with depth z positive downwards and constant thermal diffusivity D

$$\rho C \partial T/\partial t = D\, \partial^2 T/\partial z^2 \;\; or \;\; \partial T/\partial t = \mathcal{D}\, \partial^2 T/\partial z^2 \tag{51.1}$$

where C is the heat capacity of water per unit mass, ρ the density of water, and $\mathcal{D} \equiv D/(\rho C)$ is a kinematic diffusivity with units of $(length)^2/time$. The boundary condition at the surface $z = 0$ is

$$-\,D\,\partial T/\partial z = \mathcal{F} - \beta T \tag{51.2}$$

where $\mathcal{F}$, the radiative forcing, is a prescribed function of time. I am assuming that the ocean is infinitely deep. Fits of this simplest diffusive model to CMIP5 GCM output for idealized forcing scenarios are discussed by Caldeira and Myhrvold 2013.

If we write the boundary condition in terms of the kinematic diffusivity,

$$-\,\mathcal{D}\partial T/\partial z = \mathcal{F}/(\rho C) - (\beta/(\rho C))T, \tag{51.3}$$

the radiative restoring β appears in the combination

$$\mathcal{V}_\beta \equiv \beta/(\rho C) \tag{51.4}$$

which has units of velocity. Plugging in $\rho = 10^3 Kg/m^3$ and $C = 4.22 \times 10^3 J/(KgK)$ and, for example, $\beta = 2.0W/(m^2K)$, we get $4.74 \times 10^7 m/s$ for this velocity, or 15 m/year. One way of thinking about this velocity scale is to pick a time scale and compute how deep $\mathcal{V}_\beta$ takes you in this amount of time, which tells you the depth of the layer whose heat capacity gives you a radiative relaxation time for this layer comparable to the time scale that you chose — I think I got that right.] It's magnitude gives you some feeling for why the oceans are effectively very deep in many climate change contexts.

Together with the kinematic diffusivity we can now define a depth scale $\mathcal{H}$ and a time scale $\mathcal{T}$:

$$\mathcal{H} \equiv \mathcal{D}/\mathcal{V}_\beta; \;\; \mathcal{T} \equiv \mathcal{D}/{\mathcal{V}_\beta}^2 \tag{51.5}$$

Climate sensitivity in this simple model is inversely proportional to β, which is assumed to be independent of time here for simplicity. So the depth scale $\mathcal{H}$ is directly proportional to climate sensitivity, while the time scale $\mathcal{T}$ is quadratic in the climate sensitivity. This strong dependence of the characteristic time scale on climate sensitivity, with the response of high sensitivity models much slower than in low sensitivity models, is a much commented on feature of this diffusive model. A typical value of the kinematic diffusivity obtained from fits to GCMs is around $5 \times 10^{-5} m^2/s$, giving a time scale $\mathcal{T}$ of about 7 years for $\beta = 2$ and about 28 years for $\beta = 1$.

Defining non-dimensional depth $\zeta = z/\mathcal{H}$ and time $\tau = t/\mathcal{T}$, we get for the response to a step increase in forcing, $h(\zeta, \tau)$:

$$\partial h/\partial\tau = \partial^2 h/\partial\zeta^2 \tag{51.6}$$

with the initial condition $h(\zeta, 0) = 0$ and the boundary condition at $\zeta = 0$

$$-\partial h/\partial\zeta = 1 - h. \tag{51.7}$$

Once you non-dimensionalize in this way there are no parameters in the problem at all and you only need to solve the equation once. As in the previous post the response to a spike in forcing is then $g = \partial h/\partial\tau$ and the response to a arbitrary time-dependence in $\mathcal{F}$ is

$$T(\tau) = T(t/\mathcal{T}) = \int_0^\tau \mathcal{F}(\xi) g(\tau - \xi) d\xi \tag{51.8}$$

The step-response function $h(\tau)$ is shown below. (I generated this pretty quickly so don't use it for anything important without checking). Also shown in the figure is a rough approximation to $h(\tau)$ that gets better for large times $h(\tau) \approx 1/(1 + (\pi\tau)^{-1/2})$

Since the heat uptake by the diffusion is $1-h$, the heat uptake efficiency (heat uptake per unit temperature) in this approximation is $(\pi\tau)^{-1/2}$ — or, returning to the dimensional form, $\gamma(t) = \beta\sqrt{\mathcal{T}/\pi t}$ — decreasing like $1/\sqrt{t}$ with increasing time. You can use this approximation for h, the response to a step function in forcing, to estimate the response for arbitrary forcing evolution.

Assuming that the forcing is linearly increasing in time, we can compute the fraction of the equilibrium response that is realized at $t = 70 yrs$, as a function of $\mathcal{T}$ — which we can equate to the TCR. Increasing CO_2 at 1% per year until doubling, which takes 70 years, is the standard way of defining TCR, and since the radiative forcing is logarithmic in CO_2, this implies a linearly increasing forcing. In the context of the linear models being

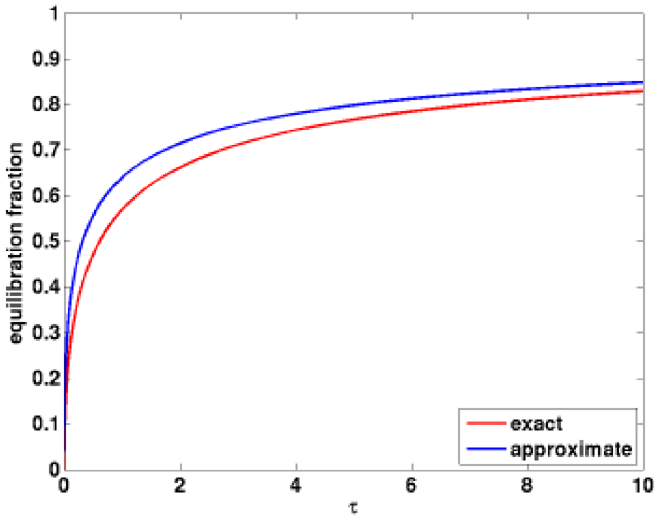


Figure 51.2:

discussed here, the magnitude of the linear trend in forcing is irrelevant;l it is only the time scale of 70 years that is important, as well as the linear shape.) For example, $\mathcal{T} = 20yrs$ corresponds to TCR/TEQ of about 60%. I have also shown a fit of the following form (which is impressively accurate)

$$TCR/TEQ \approx 1/(1 + 1.35\sqrt{\mathcal{T}/70yrs}) = 1/(1 + 1.35\sqrt{(\mathcal{D}/70yrs)/V_\beta)}). \tag{51.9}$$

Here's a plot of results for a scenario in which the forcing increases linearly for 70 years and then stabilizes, for values of $\mathcal{T} = 5, 20, 80 years$, but normalized so that they all have the same temperature at year 70, ie the same TCR. It is interesting how tightly the different curves cluster during the growth stage when normalized in this way, separating as one would expect only after the forcing has stabilized.

Given the TCR for a particular choice of parameters in this diffusive model, we can also compute the response to the forcing due to well-mixed greenhouse gases (WMGGs) over the past 100 or so years. Does the warming due to the WMGG forcing, which increases monotonically but is far from linear, scale accurately with the TCR? The answer is yes. To see this I have divided the response to WMGGs by the TCR for different values of $\mathcal{T}$, and plotted them below. (I have used the GISS WMGG forcing). (For this purpose is it just the shape as a function of time that is relevant, not the amplitude.

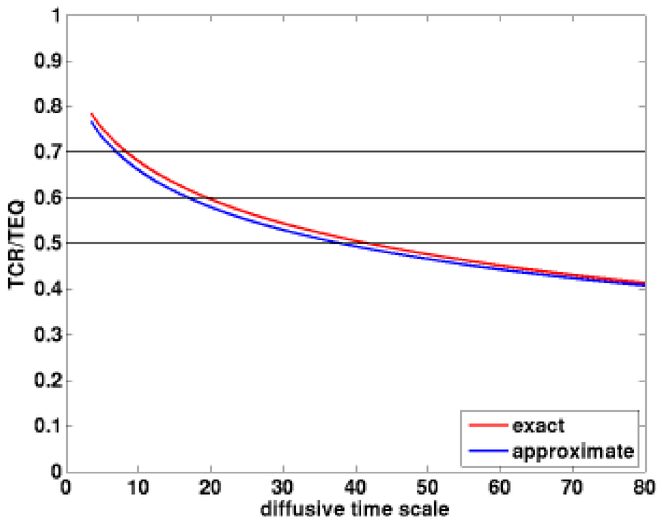


Figure 51.3:

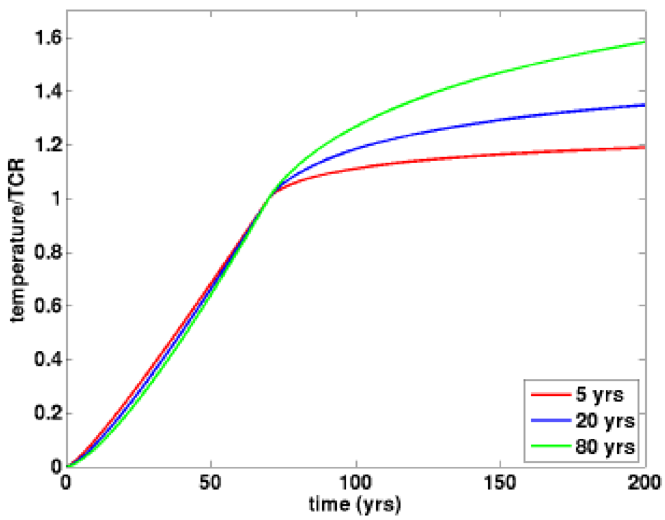


Figure 51.4:

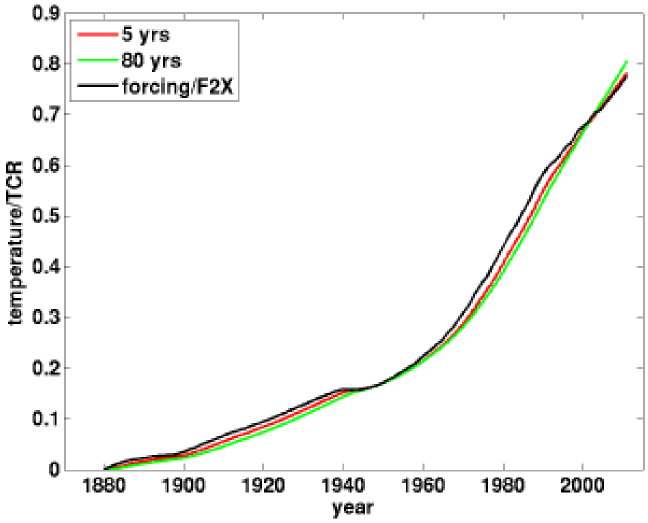


Figure 51.5:

These are identical for most practical purposes, despite the large differences in the diffusive time scale controlling the degree of disequilibrium. I haven't plotted the case with $\mathcal{T} = 20yrs$ since that line would have to squeeze between the red and green lines and would be invisible. The claim supported by this plot is that we can confidently use the TCR to predict a model's response over the 20th century to WMGG forcing. The concept of TCR is sometimes thought of as rather academic since there is no close analog of linearly increasing forcing for 70 years in reality. But in fact TCR provides us with precisely what we want when we try to attribute observed warming to increases in WMGGs. This identification is robust to large changes in heat uptake efficiency. Analyses of GCMs give the same result — see post 3, A statement about the likely range of TCR is equivalent to a statement about the likely size of the forced response to the well-mixed greenhouse gases, or to CO2 in isolation. This is the main reason that TCR is such a a useful quantity to focus on.

52 Warming and Reduced Vertical Mass Exchange in the Troposphere

[Originally posted November 15 2014]

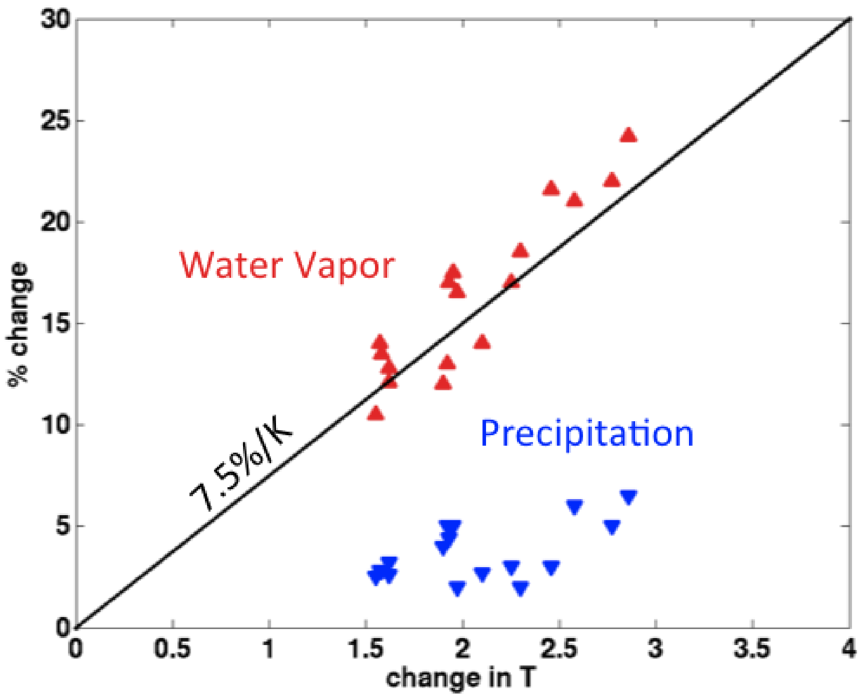


Figure 52.1: Fractional change in global mean precipitation (blue) and global mean (horizontally and vertically integrated) water vapor (red) as a function of change in global mean surface air temperature, over the 21st century in the A1B scenario in CMIP3 models. Redrawn from fig. 2 in Held and Soden 2006.

The figure at the top describes a very robust result in the responses to warming in global climate models: the fractional increase in the total amount of water vapor in the atmosphere is much larger than the fractional increase in global mean precipitation. While this figure shows the responses in CMIP3 models for a particular scenario of increasing forcing over the 21st century, the results from CMIP5 and different scenarios are all similar. This disparity in the magnitude of the increases in water vapor and precipitation and its important consequences for many other aspects of the climate response have been discussed since relatively early days in GCM simulations of climate change (e.g., Mitchell et al 1987). Perhaps the most fundamental consequence is the reduction in the vertical mass exchange between the lower and upper troposphere. That is, the "amount of convection" in the atmosphere decreases — or, by this particular measure, the atmospheric circulation slows down, especially in the tropics where a large fraction of this exchange takes place.

The connection with the atmospheric circulation is most easily understood by a simple argument that goes back at least to Betts and Ridgway 1988. Think of a picture in which parcels of air leave the boundary layer and enter the drier free troposphere, carrying a mass of dry air per unit time and unit area M and therefore carrying the water vapor Mr_B, where r_B is the mixing ratio (the ratio of the mass of water vapor to the mass of dry air) in the boundary layer. The same amount of mass returns to the boundary layer carrying Mr_T where r_T is a typical mixing ratio in the returning air, which is a lot drier. The water that is lost $M(r_B - r_T)$ equals the precipitation. Suppose that r_T is negligible compared to r_B so that the precipitation is $\approx Mr_B$. Since the vertically integrated water vapor is dominated by the vapor in the lowest few kilometers, r_B will look like the integrated water vapor in the figure at the top. If the precipitation increases more slowly, the mass flux M must decrease. If r_T is not negligible and does not change proportionally to r_T then this will change the quantitative result, but with r_T small compared to r_B the qualitative result should hold up. (In response to an email: you can think of this exchange of air as partly in shallow non-precipitating circulations for which $r_B \approx r_T$ and partly in deeper precipitation-generating circulations; if this distinction is sharp then it is only the mass flux in the deeper precipitation-generating flows that are constrained in this way.)

The slope in the temperature vs total water vapor plot is about what you expect from the Clausius-Clapeyron (C-C) dependence of saturation vapor pressure on temperature, but you have to be a little careful. For example, Back et al 2013 point out that the proportionality constant is smaller for more equilibrated climate changes, like glacial-interglacial differences. This is because the ratio of tropical warming to global warming is smaller when the climate is more equilibrated due to greater warming in polar latitudes, especially in the Southern Hemisphere. Since water vapor is dominated by the tropics, you get get less increase per unit global warming. But the bottom line is that fixed relative humidity in the lower troposphere still explains the results to first approximation. The proportionality constant is well-defined in the figure because the spatial patterns of warming are similar enough across these different models that there is a consistent relation between the changes in the globally averaged saturation vapor pressure in the lower troposphere and the globally averaged temperature change. As discussed in post 48, models beautifully reproduce satellite observations of vertically integrated water vapor averaged over the tropical oceans when these models use observed ocean surface temperatures as a boundary condition. So I think the relation between total water vapor vs temperature is very solid.

The global strength of the hydrological cycle is not determined by the C-C scaling but rather by the the energy balance of the free troposphere, the troposphere above the planetary boundary layer, where the release of latent heat associated with precipitation balances the radiative cooling to first approximation — see O'Gorman et al 2012 for a recent review. (Focusing on the troposphere above the boundary layer allows you to avoid thinking about the turbulent sensible heat flux which is important in the boundary layer.) The radiative transfer is such that the radiative cooling (in our models) just can't increase fast enough to keep up with the C-C increase in water vapor. If the atmosphere tries to increase precipitation a lot without balancing it with increased radiative cooling, the free troposphere will warm, creating a more stable environment which will eventually reduce the mass exchange and precipitation to rebalance things. It is interesting to explore the routes by which this rebalancing occurs, but whatever the mechanisms the reduction in mass exchange needs to occur for the atmosphere to re-equilibrate.

The observational record is not nearly as clear cut in this respect. In fact, there are claims (Wentz et al 2007) that precipitation has increased at close to the C-C rate over the satellite era. Others see differences between models and observations in the tropics but estimate weaker overall trends in mean precipitation (see the O'Gorman et al review for some references). I am not aware of a convincing proposal for how atmospheric radiative cooling can increase by the amount needed to balance such a large increase in precipitation per unit warming– so my working hypothesis is that there is, in fact, a substantial difference between the rates of increase of water vapor and precipitation with warming. It is important to clarify this issue. Its resolution can affect estimates of climate sensitivity as well as circulation changes. You can change the atmospheric cooling of the free troposphere by either changing the fluxes at the tropopause or at the top of the boundary layer. If you do it at the tropopause you also affect climate sensitivity, with increasing radiative cooling per unit warming decreasing temperature sensitivity while increasing the sensitivity of the mean precipitation. If you do it at the bottom of the free troposphere without compensating changes at the top, as is the case if you modify how the absorption of solar radiation responds to warming you change the precipitation sensitivity with minimal change in temperature sensitivity. (modified for clarity on Nov. 17)

Regarding circulation changes, it is sometimes assumed that a reduction in vertical mass exchange in the troposphere, dominated by the tropics, would result in weaker mean tropical circulations — weaker Hadley and Walker circulations in particular. This doesn't necessarily follow. A simple (oversimplified) picture of the tropics that I have discussed before in these pages is that most of the air is descending at a rate determined by the radiative cooling, with upward motion confined to a relatively small fraction of the area. If there are more than the average number of plumes of rising air in some large region, the Western Pacific warm pool or the ITCZ in the eastern Pacific say, the mean motion is upward, while the mean motion is downward where convective plumes are relatively scarce. In the regions of mean upward motion there has to be convergence of air at low levels, and low level divergence out of the regions with mean descent – and the surface flow can be thought of a driven by this pattern of convergence and divergence (rotation makes the connection between this convergence/divergence pattern and the flow itself a little counterintuitive). The average of the north-south flow around latitude circles is referred to as the Hadley circulation, while the Walker circulation is a strong westward flow at low levels over the Pacific. Even if the total mass exchange decreases, if the pattern of convection becomes more organized the large-scale circulation could be enhanced — for example, if more convection moved to the regions where convection is already prevalent and even less occurred in the relatively quiescent regions.

But having said all that, if not much happens to the pattern of convection, you would expect the large-scale circulation to weaken on average. In models a lot of this weakening occurs in the east-west Walker circulation rather than the north-south Hadley circulation. It seems like the latter is prevented by other constraints from changing as much. Models generally do predict a weakening Walker circulation with warming, and I think that this overall weakening of the mass exchange in the tropics is part of the explanation for this model result. And this did seem to be an emerging signal in observations (Vecchi et al 2006) — until the recent 15 years or so in which the continuing hiatus/persistent La Nina/strong Walker circulation has muddied the picture of what the long-term forced trend might be.

53 The Rapidly Rotating "Fruit Fly"

[Originally oosted November 25 2014]

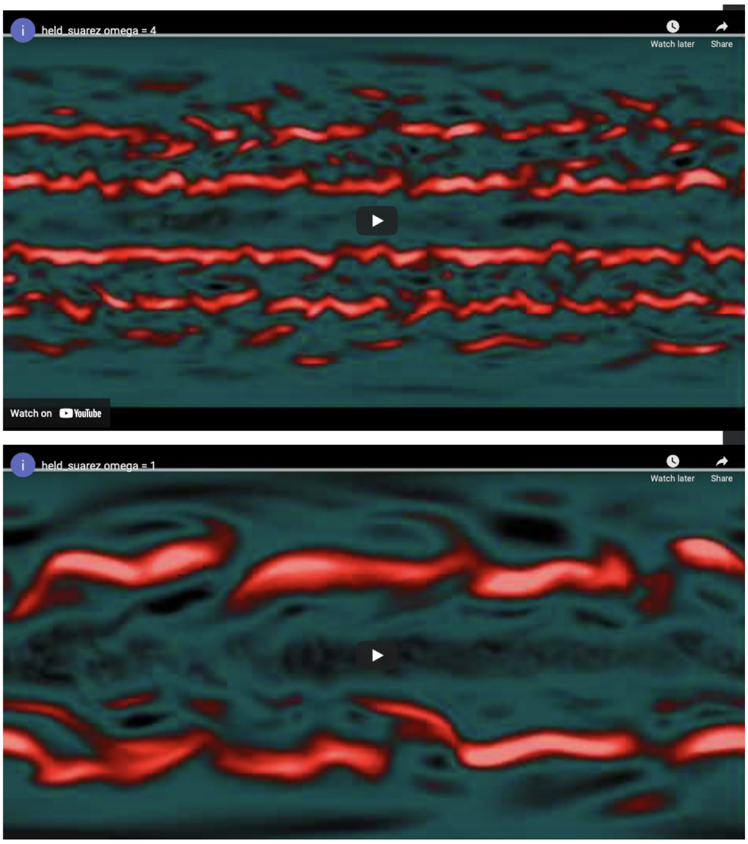


Figure 53.1: Snapshots of the magnitude of eastward winds in the upper troposphere in the *fruit fly* model using the Earth's rotation rate (bottom) and using a rotation rate that is 4 times larger (top). The red saturates at 50m/s eastward flow in the lower panel and 30 m/s in the upper panel. (Lat-lon plot over the entire globe)

Animations for 50 Earth days can be found here and here.

In post 28, I described a model of an ideal gas atmosphere with no latent heat (no water vapor for that matter) with radiative heating/cooling a function of temperature only, with no seasonal cycle, and with linear drag near the surface relaxing the flow back to zero in a rotating reference frame (this is how the atmosphere knows that it is rotating). The lower boundary condition is a homogeneous spherical surface (no mountains, no continents, no oceans). I think of this model as part of a hierarchy of models of increasing complexity so with an admiring reference to the way in which biological research is organizing around model organisms I refer to this as the *fruit fly* model. In that post, I mentioned that the surface westerlies move polewards as the rotation rate is decreased. Poleward movement of the westerlies is what we expect in a warming world. There is no guarantee that what we learn by varying the rotation rate in this very controlled setting will be directly relevant to that problem but I think it does stress our understanding in interesting ways . The animation above compares the evolution of the zonal (east-west) component of the wind in the upper troposphere when using the Earth's rotation rate with the evolution you get with 4 times the Earth's rotation rate.

The striking behavior that one sees with rapid enough rotation is the appearance of multiple jets in each hemisphere. With 4 times the rotation rate in this particular case, there are three eastward jets in each hemisphere, with the most poleward jet struggling to get organized. You can get a sense form the animations that the eddies are a lot smaller in the more rapidly rotating case. Our understanding is that the ratio of the size of the eddies to the size of planet is critical for the formation of multiple jets. The way in which rotation influences the eddy size is a fascinating issue with a long history, but for this post we can just accept the result that eddy size decreases with increasing rotation rate. We find that if there is room for a lot of eddies between the equator and the pole you get multiple jets. The eddies on Earth are too big and don't have enough room to create more than one jet. You get similar results by increasing the radius of the planet and holding the rotation rate fixed.

Each of these westerly jets (with winds from the west) extends to the surface, producing 3 regions of surface westerlies per hemisphere rather than one as on Earth. The figure that follows shows the time and zonally averaged near-surface and upper tropospheric zonal winds: As you increase the rotating rate from the Earth's value, the surface westerlies first move equatorwards, and then when there is enough room another region of westerlies and the associated upper level jet emerge. Increasing the rotation rate further, the circulation continues to compress equatorward, until another jet emerges, etc

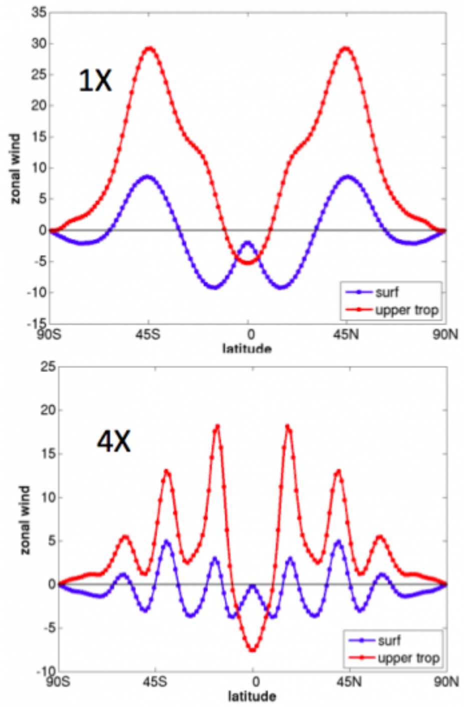


Figure 53.2:

There are different starting points for thinking about the underlying fluid dynamics. Relating these different perspectives is challenging:

- **Baroclinic instability**: The dominant eddies in the troposphere, responsible for the structure of midlatitude weather on Earth, are generated by what we refer to as baroclinic instability. These eddies work to reduce the north-south temperature gradient and, speaking a bit loosely, they extract their energy from that gradient. But this temperature gradient is associated through "thermal wind balance" — that is, geostrophic balance plus hydrostatic balance – with an increase of the eastward winds with height. When these eddies grow and reduce the north-south temperature gradient they also reduce the vertical gradient of these zonal winds — that is, they transport eastward momentum from the upper troposphere down to the lower troposphere,producing surface westerlies in the process and providing a drag on the upper level flow. But if there is a hint of a jet-like structure in the upper tropospheric winds to begin with, the eddies importantly prefer to exert this drag on the sides of the jet and not at its center, so they have a propensity to create jets. The resulting upper tropospheric jets also act as waveguides for the eddies, helping to create a coherent jet/storm track structure that has a meridional width related to the characteristic size of the eddies. See Panetta 1993.

- **Two-dimensional turbulence**: Two-dimensional flows tend to cascade energy to larger scales, rather than smaller scales as in the more familiar three-dimensional turbulence. On a rotating sphere this energy is channeled into zonal jets with a width determined by the energy level of the eddies in the flow, as described in a seminal paper by Rhines 1975.

  These multiple jet flows obviously bring to mind the circulation of Jupiter and the other gas giant planets. There are also important differences. For example, no long-lived Great Red Spot-like vortex forms in this simulation. And there is an eastward jet at the equator on Jupiter, a feature we refer to as superrotation, that is not present in this simulation either. But we shouldn't expect to simulate everything in the Jovian atmosphere by changing one parameter in an Earth-like model! (You move further in the Jovian direction by reducing the strength of the drag exerted by the surface in this model.) A review of Jovian meteorology that I like is Vasavada and Showman 1995.

A former colleague at GFDL, Gareth Williams, passed away shortly before this post was composed. Gareth was fascinated by the circulation of Jupiter's atmosphere and returned to it repeatedly throughout his research career. Before he turned to this problem, theories were centered on the possibility that the jets and banded structure were superficial manifestations of very deep convective cells resembling concentric cylinders, driven by heat release in the interior. Starting in the 1970's, Gareth argued for a picture resembling terrestrial meteorology with the multiple jet structure set by the dynamics of a thin spherical shell of fluid, as discussed above — e.g., 1978, 1979, 1982, 2002. Precisely how these two ideas fit together is still discussed (the jets can be deep even though driven from the surface) but there is little doubt that the surface-driven perspective is a big part of the story. I would like to think of his post as a small tribute to Gareth's memory.

54 Tropical Tropospheric Warming Revisited - Part I

[Originally posted December 19 2014]

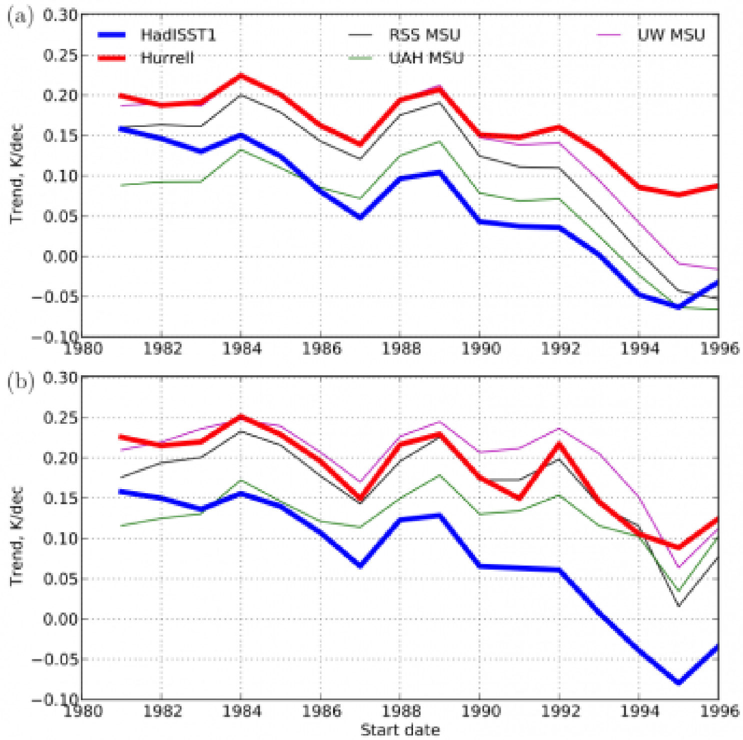


Figure 54.1: Mid-tropospheric temperature trends (TTT channel — see below) from a given start date till 2008, plotted as a function of start date, in three analyses of the MSU data (thin lines) and in an atmosphere/land model running over two estimates of observed sea surface temperatures: HadISST1 (blue), Hurrell (red). The upper panel is the trend from ordinary least squares while the lower panel uses the Theil-Sen estimator.

In a paper just published, Flannaghan et al 2014, my colleagues (Tom Flannaghan, Stephan Fueglistaler, Stephen Po-Chedley, Bruce Wyman, and Ming Zhao) and I have returned to the question of tropical tropospheric warming in models and observations — Microwave Sounding Unit (MSU) observations specifically. This work was motivated in part (in my mind at least) by the material in Post 21, but the results have evolved significantly. All the figures in this post are from the Flannaghan et al paper.

There are important discrepancies between models and observations regarding tropical tropospheric temperature trends. It is informative if we can divide these into two parts, one associated with the sea surface temperature (SST) trends and the other with the vertical structure of the trends in the atmosphere and how these trends are related to the SST trends. The former is associated with issues of climate sensitivity and internal variability; the latter is related to the internal dynamics of the atmosphere, especially the extent to which the vertical structure of the temperature profile is controlled by the moist adiabat. A moist adiabatic temperature profile is what you get by raising a parcel which then cools adiabatically due to expansion, with part of this cooling offset by the warming due to the latent heat released when the water vapor in the parcel condenses.

The tropical atmosphere is observed to lie close to this profile, as do our models. Models continue to approximately follow this profile as they warm., so they invariably produce larger warming in the upper troposphere than at the surface in the tropics — simply because the water vapor in the parcel increases with warming, so there is more heating due to condensation as the parcel rises. All models do this, from global models to idealized "cloud resolving" models with much finer resolution — see Post 20 for a discussion of the latter. The same top-heaviness is seen in the tropospheric temperature changes accompanying ENSO variability, which models simulate very well. Why should lower frequency trends behave any differently than the year-to-year variability resulting from ENSO? If models have this wrong it has a lot of implications.

We study the vertical structure part of the problem using atmosphere/land models running over estimates of the observed SSTs as boundary conditions. The appropriateness of this uncoupled setup will be addressed in the following post, but let's just assume that it's OK for the moment. There has been other work along these lines. In particular Fu et al 2011 and Po-Chedley and Fu 2012 helped to motivate our paper. The figure at the top shows some of our results. For this plot, following Fu et al, we use the linear combination of the mid-tropospheric (T2 or TMT) and stratospheric (T4 or TLS) MSU channels, TTT = 1.1 T2 – 0.1 T4, that tries to minimize the weight given to the stratosphere (TTT is also referred to as T24). See also Fu et al 2004. Without this modification, T2 has enough weight in the stratosphere that the large cooling trend there compensates for part of the tropospheric warming trend in T2, making it harder to interpret. The stratospheric trend is primarily forced by ozone changes and is not closely coupled dynamically to the tropospheric trend. I don't see any reason not to use an adjustment of this kind.

The figure at the top shows the TTT trends averaged from 20S-20N from different start years to 2008, plotting the results as a function of the start year, using three versions of the MSU data and two versions of the model results. (These end in 2008 because some of these runs are taken directly from the CMIP5 archive –we really should update them.) The model results are obtained by using the appropriate vertical weighting of the simulated height dependent trends. The upper panel is the least-square linear slope while the bottom panel is the median of the slopes between all possible pairs of data points. The two model results are from the same model (HiRAM — also discussed in this context in Post 21) but using different SST estimates as boundary conditions — HadISST1 and Hurrell 2008 – the latter being a blend of HadISST and the higher resolution NOAA Optimal Interpolation dataset. Hurrel is the boundary dataset recommended for the AMIP (prescribed SST and sea ice) simulations in the CMIP5 archive. Our contribution to the archive with the HiRAM model used the HadISST data instead. Why? It was more convenient for continuity with some other runs that were ongoing and we didn't think that it made a difference. (We weren't the only group to do this — GISS evidently did the same thing.) We eventually got around to doing the same simulations with Hurrell and saw the big differences shown in the plot. So on the one hand our stubbornness created some confusion But on the other hand we might not have noticed this sensitivity to the SST dataset if we had not departed from the script originally.

If you look at the tropical mean SST trends themselves they differ in the same sense, with HadISST having smaller trends, but the difference is too small, after accounting for moist adiabatic amplification with height, to explain this tropospheric trend difference. This does not mean that one cannot think of a moist adiabat as connecting the surface air and the upper troposphere, but one has to ask "which moist adiabat?"

The upper tropospheric temperature field is quite flat spatially, smoothed out by wave propagation — the appropriate analogy is the flattening of the surface of a pond due to surface gravity waves when you dump a bucket of water in one spot. But the SSTs have a lot of spatial structure in the tropics. Since it is deep convection that couples the surface boundary layer air to the upper troposphere, a natural assumption is that it is the SSTs in the regions of deep convection that matter. Since most of the precipitation in the tropics is related to these deep convective systems, we can think of the precipitation as providing a natural weighting for the importance of the SSTs in different regions. (We are assuming that the relative humidity over the ocean can be thought of a constant here, so the water vapor that is critical for determining which moist adiabat you follow as you rise is itself determined by the temperature.) So we define a precipitation-weighted average of the SST:

$$T_P \equiv \frac{< PT >}{< P >} \tag{54.1}$$

where the brackets are an average over the oceans from 20S to 20N. (The rationale for thinking of precipitation over the oceans only in this context will also be discussed in the next post.) Sobel et al 2002 use the same approach when discussing the warming of the tropical troposphere due to ENSO, but in that case the difference between the precipitation-weighted average and straight average is subtle. In the case of trends, this distinction is evidently more important and provides a consistent picture of why the model run over these two different SSTs generates such different upper level trends.

The following figure shows the trends in SSTs (for two different starting years) in different parts of the distribution of tropical SSTs, with the coldest on the left and the warmest on the right. The differences in trend are largest

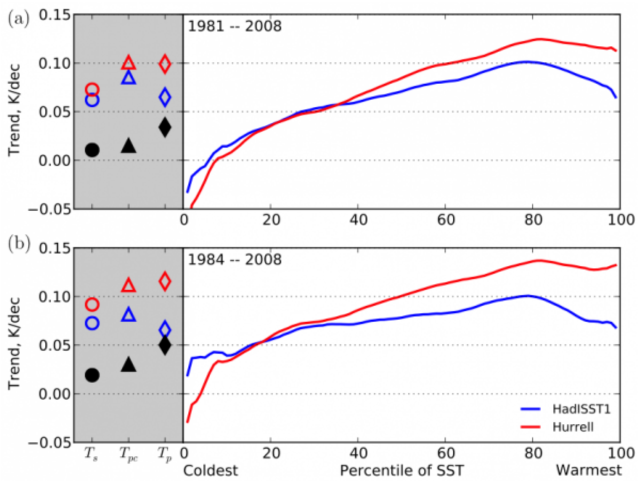


Figure 54.2:

at the warmer end of the distribution of SSTs, which is where the bulk of the precipitation occurs and where the entropy of the boundary layer air is communicated to the rest of the troposphere.

The three columns of symbols on the left show the unweighted mean SSTs (circles), the SSTs weighted by a time-independent weighting function equal to the climatological precipitation in the model (triangles), and T_P, the SSTs weighted by the model precipitation as it varies month-by-month (diamonds). The red and blue correspond to HadISST and Hurrell and the black to the difference. The time-independent weighting moves you in the right direction, but we need to use the SSTs weighted by the time-evolving precipitation to get a big enough difference to explain the differences in the upper level trends.

The vertical profiles of the trends in the GCM simulations are shown below, for two different starting dates once again. The shading is the spread of the trends in three realizations of the model with each SST. The smallness of this spread indicates how tightly tropospheric trends in this atmosphere/land model are coupled to the SSTs. The large cooling trends in the model above 100mb are not evident in this plot. As in Post 20, to

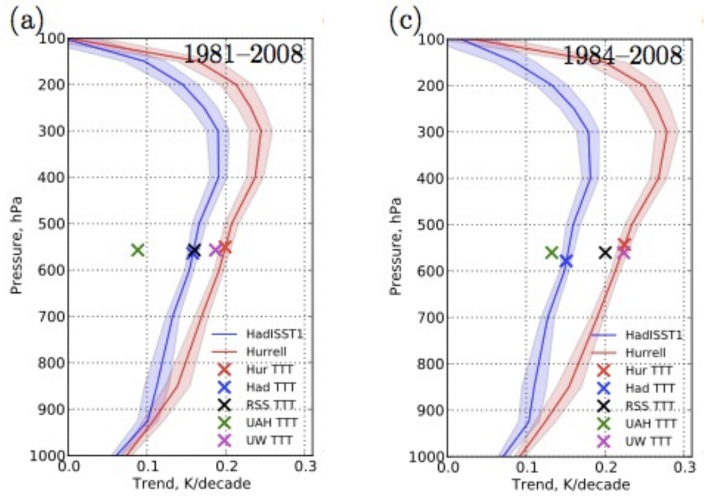


Figure 54.3:

show the estimated MSU trends and the atmospheric trend profiles on the same plot we first compute the levels at which the simulated TTT values obtained from the model — labelled HurTTT and HadTTT — are identical to the actual model temperature trend. These are slightly different between HadISST and Hurrell model runs, so we average them together, and then plot the MSU observations at this level. (If we used the same convention and plotted the unadjusted T2 trends in the same way, this level would be close to the ground!) I like this way of plotting the model profile and the MSU data together — it reminds us that the MSU weighting functions are too broad to catch the actual maximum in the model's warming trend near 300mb, even though the maximum weight for TTT is near that level. This figure shows pretty vividly how the time period chosen can influence one's conclusions, as is also evident in the figure at the top. Stephen Po-Chedley obtained very similar results using the Community Atmospheric Model CAM4.

So, if you are trying to make a case for inconsistency between the vertical structure of tropospheric temperature trends in models and observations, you have to pay attention to the SSTs as well as the treatment of the raw MSU data (or radiosondes as the case may be). Not surprisingly, it is the SST trends in the regions of deep convection and precipitation that matter, and this puts more pressure on the quality of the SST data set.

I'll return in the next post to the question of the legitimacy of decoupling atmosphere and ocean by prescribing SSTs in this way. Can we really use this model setup for quantitative analysis of this kind or is there the potential for significant distortion?

(Special thanks to Stephan Fueglistaler for many conversations on this topic over the past year.)

55 Tropical Tropospheric Warming Revisited - Part II

[Originally posted January 20 2015]

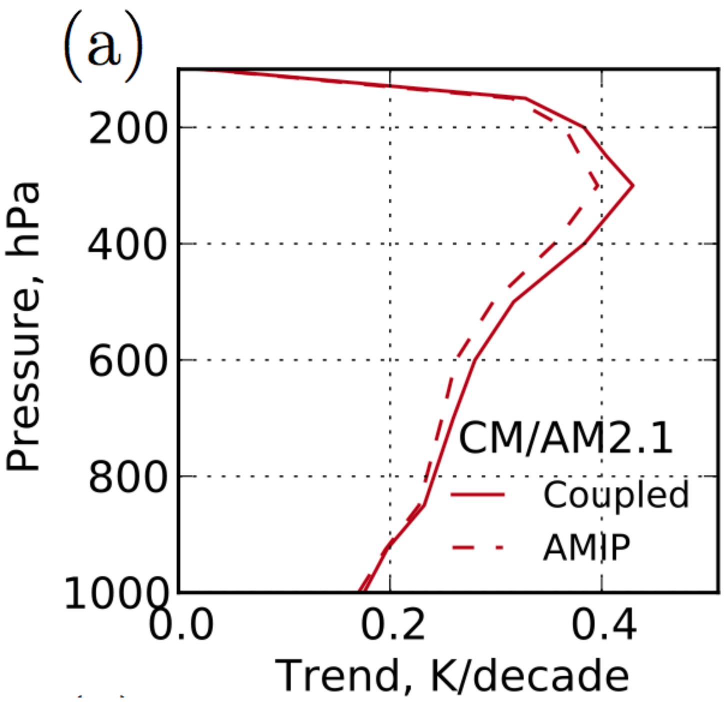


Figure 55.1: Vertical profile of temperature trends averaged over 20S-20N in two models. Solid: trends in a 30-year (1970-2000) realization of the CM2.1 coupled model using estimated forcing agents from 1970-2000. Dashed: simulation using the atmosphere/land component of CM2.1 with the same forcing agents but running over the sea surface temperatures generated in this realization of the coupled model.

The previous post summarizes the results from a recent paper, Flannaghan et al 2014, that uses atmosphere/land models running over observed sea surface temperatures (SSTs) to look at the consistency between these models and observations of tropical tropospheric temperature trends. The idea of using this kind of uncoupled model is to try to put aside the issue of SST trends in the tropics and focus more sharply on the vertical structure of the temperature trends. Because models are so consistent in producing a warming trend that is top-heavy in the tropical troposphere, due to the strong tendency to follow a moist adiabatic profile, and because this pattern of change has numerous ramifications for tropical climate more generally, any possibility that this warming profile is wrong takes precedence over other issues in tropical climate change, in my view. I interpret the results in Flannaghan et al to say that microwave sounding data, at least, does not require us to reject the hypothesis provided by climate models for the vertical profile of the tropical temperature trends.

To follow up on the last post, I would like to discuss, or at least mention, some other issues regarding this setup, in which SSTs are simply prescribed as a boundary condition. Could there be something fundamentally flawed about this approach? This may seem like a technical issue, but a large fraction of atmospheric model development takes place in this prescribed SST framework to try to separate biases due to atmospheric model imperfections from those due to the ocean/sea ice model, so it is important to understand its limitations. I have used this kind of setup in a number of posts to address other issues, for example 2,10, 11, 32, 34; any limitations to this decoupled framework could affect my own thinking about a variety of climate change issues.

In a coupled model we integrate the atmosphere/land state A and the ocean state O forward in time

$$\partial A/\partial t = \mathcal{A}(A, O); \quad \partial O/\partial t = \mathcal{O}(A, O) \tag{55.1}$$

Let's assume that the atmosphere/land model $\mathcal{A}$ is deterministic and that the SST is the only piece of information about the state of the ocean that the atmosphere feels, so the first equation can be replaced by $\partial A/\partial t = \mathcal{A}(A, SST)$. Run the coupled model and store off the SSTs; then run the atmospheric model in isolation, reading in these time-evolving SSTs as needed. You'll get the same answer. So what's the problem?

The problem is that this kind of perfect substitution is not physically relevant since the whole point is to run over observed SSTs and compare to atmospheric observations. The atmospheric model is imperfect, we don't know the initial conditions precisely, and the atmospheric model is chaotic — any perturbation in the atmosphere due to model imperfections no matter how small or due to differences in initial conditions will grow in time.

Think of a hurricane that develops in the coupled model and imprints a cold wake on the SST. In a slightly perturbed realization of the atmospheric model no storm is present at that point and the cold wake appears out of thin air. And a storm develops elsewhere with no corresponding cold SST signature, possibly resulting in a biased storm strength. Could biases in storm strength get rectified somehow and bias the tropospheric temperature trends? Or consider the Madden-Julian Oscillation (MJO), an important mode of variability in the tropical Indian and western Pacific oceans with a 30-50 day time scale. A number of models indicate that coupling dynamically to SSTs affects the amplitude and frequency of the MJO. If uncoupling the atmosphere from the SSTs alters MJO variability, could this difference in variability be rectified to affect tropospheric temperature trends?

On the other hand, our theories of ENSO variability are typically consistent with a picture in which the atmospheric component of this coupled variability can be understood as the response to the SST anomalies, so in this picture one can regenerate the atmospheric state through ENSO cycles by running an uncoupled atmospheric model over observed SSTs. The difference in the case of ENSO is evidently that there is little atmospheric variability at this low frequency in the tropics in the absence of the SST anomalies. In the storm and MJO examples intrinsic atmospheric variability imprints itself on the SSTs and the back effect of these SSTs on the atmosphere is then distorted if the coherence of this atmospheric variability and the SSTs is destroyed. (At least that is my understanding). See Bretherton and Battisti 2000 for another interesting example, involving the North Atlantic Oscillation.

How do you tell if this kind of thing is important or not? One way to start is through a "perfect model" test. Take the SSTs from a coupled model and prescribe these as the boundary condition for the atmosphere/land component of this same model — perturbing the initial conditions to create another realization of the atmospheric state. Bruce Wyman at GFDL did this using 30 years (1970-2000) from a single run of the CM2.1 coupled model (this is run2 in the CMIP3 archive) and comparing what we get running the atmosphere/land model over these SSTs. The uncoupled model turns out to be slightly warmer in the tropical upper troposphere, by 0.15C on average. Interestingly, this difference is biggest during cold (La Nina) phases in the model — as seen below, using 12-month running means. (As pointed out in other posts, this model produces too many super- El Nino events, creating too much variability in tropical temperatures compared to observations.)

The figure at the top of the post compares the vertical profiles of the

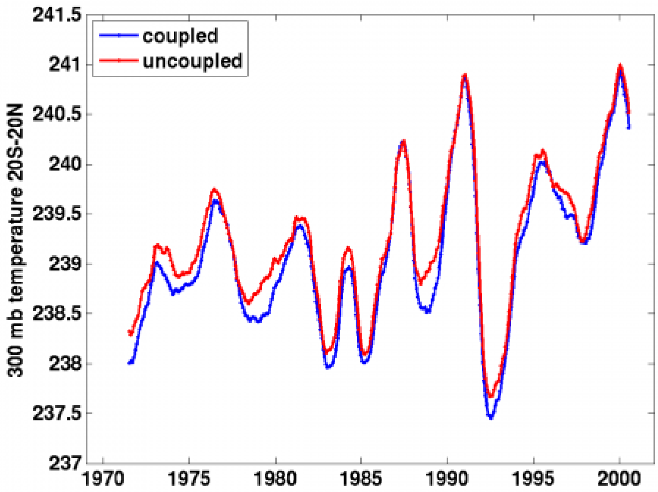


Figure 55.2:

trends in these two models. (Like many other models, this coupled model warms too much in the tropics over this time period.) The coupled model warms a bit more aloft than the uncoupled model running over the coupled model's SSTs, consistent with the difference between the blue and red lines in the time series plot converging over time. I think this is worth pursuing, to understand these differences better, and to check with other models to see if they are consistent. But the bottom line is that the differences in trend are small in this model. Hopefully this smallness is robust, providing confirmation that we can in fact use prescribed-SST models to address this vertical-profile-of-tropical-trends issue. This conclusion could be different for other aspects of climate change.

On a related point, in discussions of surface vs upper tropospheric trends you often see comparisons with the land+ocean surface temperatures rather than SSTs in isolation. But in the kind of simulations described here, the land is thought of as part of the "atmosphere" — land temperatures are free to change in response to SSTs and forcing agents. Part of the rationale for running over prescribed SSTs is that the SSTs in large part evolve more slowly than the intrinsic variability in the atmospheric state. But the land surface has time scales comparable to the atmosphere — decoupling them could distort the diurnal cycle among other things. So we typically do not try to prescribe the land temperature in this kind of simulation. One could try to get around this by prescribing the temperature of land layers at depths not affected by the diurnal cycle, for example. But there is a more important reason to avoid prescribing land temperatures when considering this vertical profile issue.

The moist adiabatic temperature profile depends not only on the temperature of the parcel near the surface that one starts with, but also its water vapor content. Over the ocean we can hopefully get away with thinking in terms of temperature only because the relative humidity of air near the surface is so strongly constrained as the climate warms (see Post 47). But over land relative humidity is freer to change. One can argue that, at least in regions that maintain some convection, the surface temperature changes will try to maintain consistency with the changes in near-surface relative humidity so that one still ends up at more or less the same temperature as the oceanic parcels when lifted to the upper troposphere. To accomplish this the land has to warm more in regions that dry out with warming. This is a topic that I plan to return to soon in a future post. Comparing trends in tropical land+ocean near-surface temperatures to trends in upper tropospheric temperatures is confusing without taking the changes in relative humidity into account.

Another issue that come up when thinking about these prescribed SST atmosphere/land models is the extent to which changes in the forcing agents can modify the tropospheric tropical temperature profile even with fixed SSTs — particularly reductions in ozone affecting temperatures near the tropopause and greenhouse gases/aerosols affecting lower tropospheric temperatures over land.

56 Tropical Ocean Warming and Heat Stress over Land

[Originally posted February 23 2015]

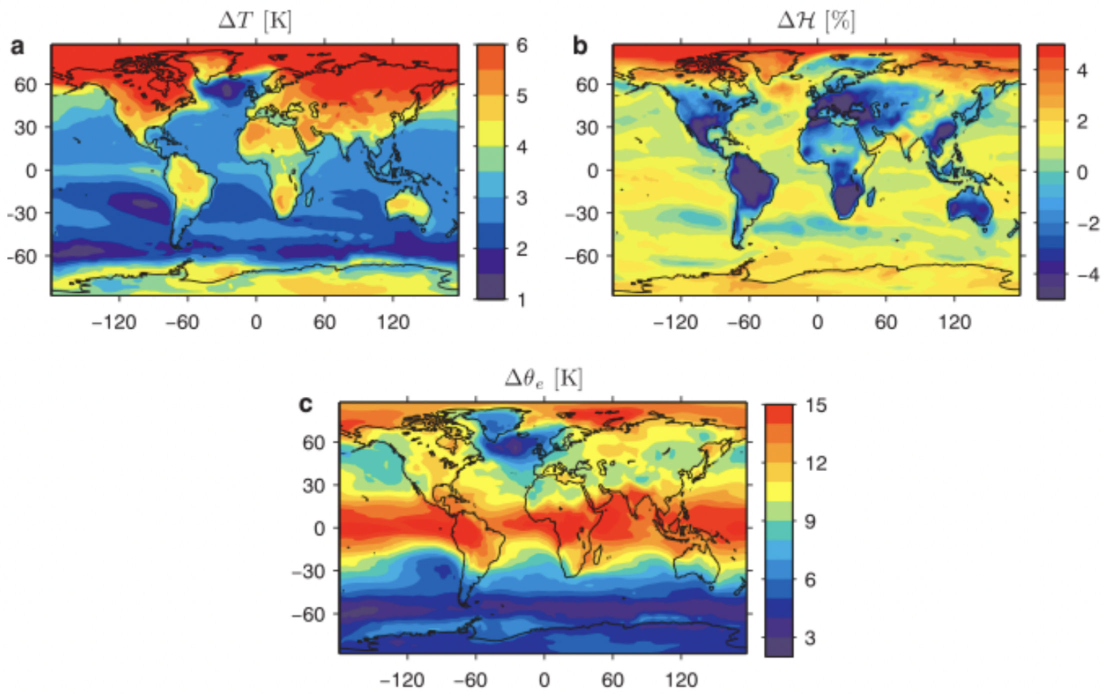


Figure 56.1: Multi-model median of changes in near surface a) temperature, b) relative humidity, and c) equivalent potential temperature between the historical simulation (1975-2004) and the RCP8.5 (2079-2099) simulations in CMIP5. From Byrne and O'Gorman 2013.

In global warming simulations the surface air over land warms more than over the oceans in the tropics, while the relative humidity decreases over land, increasing a bit over the oceans, as illustrated in panels a) and

b) above from a recent paper by Michael Byrne and Paul O'Gorman. A quantity that is relevant for the convective instability of the atmosphere and the profile of temperature on a rising air parcel is the equivalent potential temperature, Θ_e. Strikingly the increase in Θ_e is quite uniform in the deep tropics, irrespective of whether the underlying surface is land or ocean. This is not an accident and provides us with a simple way of thinking about increasing heat stress with warming.

As an air parcel rises adiabatically it maintains its entropy, or its value of Θ_e (the entropy of an air parcel is $\approx c_p \ln(\Theta_e) + constant$). Θ_e increases with increasing temperature and with increasing humidity. Suppose two parcels start out near the surface, one relatively cool and moist (over the tropical ocean) and the other warm and dry (over tropical land), but with the same Θ_e. As the parcels rise and cool due to adiabatic expansion, with their temperatures decreasing at 9.8K/km, the dry adiabatic lapse rate, they will eventually become saturated, with the dry warm parcel needing to be raised higher to reach its lifting condensation level. Above the level at which they are both saturated the temperatures should be equal, and remain equal as they are raised further, the temperatures now following a moist adiabat due to the latent heat release that accompanies saturated ascent.

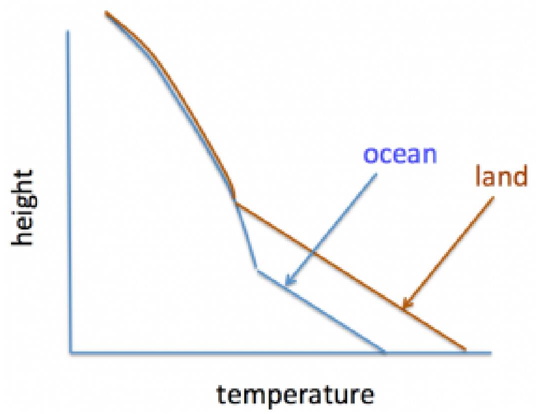


Figure 56.2:

What is the dynamics underlying this picture? Suppose we know the mean ocean temperature in the tropics. In previous posts I have discussed the case for the relative humidity near the surface over the oceans being strongly constrained. So let's take the mean oceanic Θ_e as known, which through convection sets the temperature profile of the free troposphere (given the last two posts, perhaps it is better to think of the mean precipitation-weighted value of Θ_e as setting the free tropospheric temperatures). Land relative humidity is not as strongly constrained, so the best we can do, until we agree on a theory for the magnitude of the continental drying, is to ask about the temperatures consistent with a given amount of drying (circling back to the humidity question when ready to do so).

Now think of the near surface temperatures as being warmed by the sun, convecting only weakly if temperatures are not sufficient to produce oceanic Θ_e's, because a rising parcel's density will be higher than that of the environmental air at the same level. Once this critical Θ_e is reached, substantial convection to the upper troposphere is possible. But moist convection doesn't just mix the air vertically, it warms the air locally and generates a circulation with net upward motion in the convecting regions, converging air at low levels and diverging air aloft – a circulation that transports energy away from the convecting region. So one can imagine that this energy transport is a strongly nonlinear function of land temperature, kicking in hard to prevent Θ_e and tropospheric temperatures larger than the environmental values controlled by the oceans (until the amount of convection is strong and wide-spread enough that it can effectively wrestle control from the oceans and control its own environment).

Taking this picture at face value, we should expect the climatological Θ_e over land and ocean in the tropics to be quite uniform, or at least that the oceanic value should be an upper bound on the land values. And we should expect the change in Θ_e in response to global warming to be uniform as well, providing a simple explanation for the result in panel c above. As discussed in the Byrne/O'Gorman paper, the change in Θ_e is more uniform within the tropics than the climatological distribution. See also Joshi et al 2008. I would like to understand this distinction better.

Turning to the issue of heat stress: The concept of wet bulb temperature is the usual starting point for discussions of the effects of warming on the comfort of human beings. To compute the wet-bulb temperature, assume that you have a reservoir of water (in your skin for example). Start with the temperature T and the specific humidity q (the density of water vapor divided by total density of the air), then take some energy out of the air (at fixed pressure) and use it to convert water in the reservoir to vapor. Continue to do this, lowering the temperature and increasing the humidity, until the air is saturated, at which point you are at the wet bulb temperature, T_{WB}. That is,

$$h \equiv c_p T + Lq = c_p T_{WB} + Lq_s(T_{WB}) \tag{56.1}$$

Here L is the latent heat of evaporation, and $q_s(T)$ is the specific humidity at saturation at the temperature T and at the pressure that has been fixed throughout. $c_pT + Lq$ is the enthalpy of moist air. So the wet bulb temperature and the enthalpy are the same thing, in the sense that there is a one-to-one correspondence between them.

[What I have defined here is sometimes referred to as the thermodynamic wet bulb temperature. It can differ in principle from what a wet-bulb thermometer measures, for which the rates of exchange of heat and water vapor between the air and the reservoir play a role. See here for a discussion of the distinction. Also, heat stress is often quantified with the wet bulb globe temperature, the major component of which is the wet bulb temperature, but which also involves the temperature itself as well as the amount of sunlight. See Willett and Sherwood 2012 and Dunne et al 2013.]

There is a close connection between the enthalpy, or the wet bulb temperature, and Θ_e . One can write (corrected 3/10/15)

$$\Theta_e \approx \Theta \exp(\frac{Lq}{c_pT_L}) \tag{56.2}$$

where Θ is potential temperature of dry air, the temperature of the air if brought adiabatically to a reference pressure, and T_L is the temperature at the lifting condensation level discussed above. Perturbing this expression, you get

$$\delta\Theta_e/\Theta_e \approx \delta\Theta/\Theta + (L/(c_pT))\delta q \tag{56.3}$$

where I have ignored $\delta T_L/T_L$ compared to $\delta q/q$ and approximated T_L by the surface temperature T (these are all absolute temperatures). Experts in thermodynamics of moist air will be nervous about the assumptions needed to justify these expressions — ignoring the partial pressure of water vapor compared to that of dry air, the temperature dependence of L, the effect of the water vapor on the heat capacity of the air, etc — these small effects do end up making a significant difference when looking at the climatology but I think they are less important when thinking about perturbations of the magnitude of relevance here. Ignoring the surface elevation over land and referencing the potential temperature to a typical pressure at sea level, so that $\delta\Theta/\Theta = \delta T/T$ we get

$$\delta h/c_p \approx (T/\Theta_e)\delta\Theta_e \tag{56.4}$$

So the change in h/c_p should have more or less the same structure as the change in Θ_e but with a somewhat smaller amplitude overall.

Michael Byrne has kindly generated $\delta h/c_p$ from the same model simulations as used in the figure above, confirming this qualitative picture. In particular, the increases in enthalpy and, therefore, wet bulb temperature, are of similar magnitude over land and ocean in the tropics. Over the

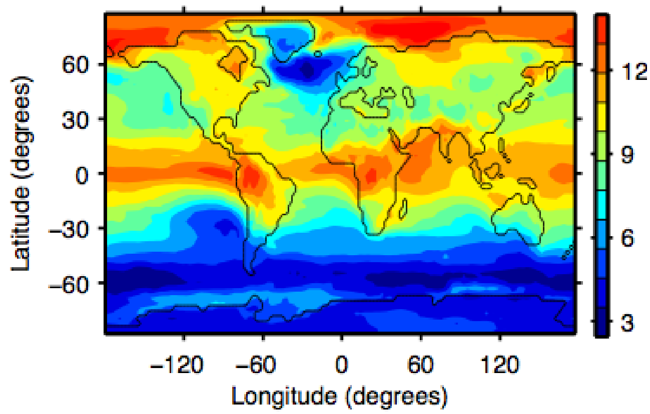


Figure 56.3:

oceans, the relative humidity is high, so the change in wet bulb temperature is close to the change in temperature itself. (Students may wish to check this, assuming a temperature of 27C and a relative humidity of 80%, and then increasing temperature at fixed relative humidity and comparing the resulting change in web bulb temperature to the change in temperature.) Therefore, the change in wet bulb temperature over land is comparable to (and, by the previous arguments, controlled by) the mean change in ocean temperature.

Another implication of this line of argument is that the model spread in the predictions for increases in the enthalpy or wet-bulb temperature over tropical continents should be smaller than the spread in temperature and in humidity separately. This is what one sees, with this cancellation carrying over to changes in extremes as well as mean responses (Fischer and Knutti 2013). Comparisons of model simulations of enthalpy or wet bulb temperature trends in the past with observations should be a high priority (as advocated from a somewhat different perspective by Pielke et al 2004)

The high extremes in wet bulb temperature are obviously of importance for heat stress. Values of wet bulb temperature approaching body temperature are of special concern, given the difficulty in cooling the body by evaporation under these conditions (Sherwood and Huber 2010) We can be thankful that ocean temperatures in the tropics have warmed less rapidly in recent decades than the average projection from our climate models. The stakes are high.

(Thanks also to Steve Garner for some helpful discussions while writing this post.)

57 Teleconnections and Stationary Rossby Waves

[Originally posted March 9 2015]

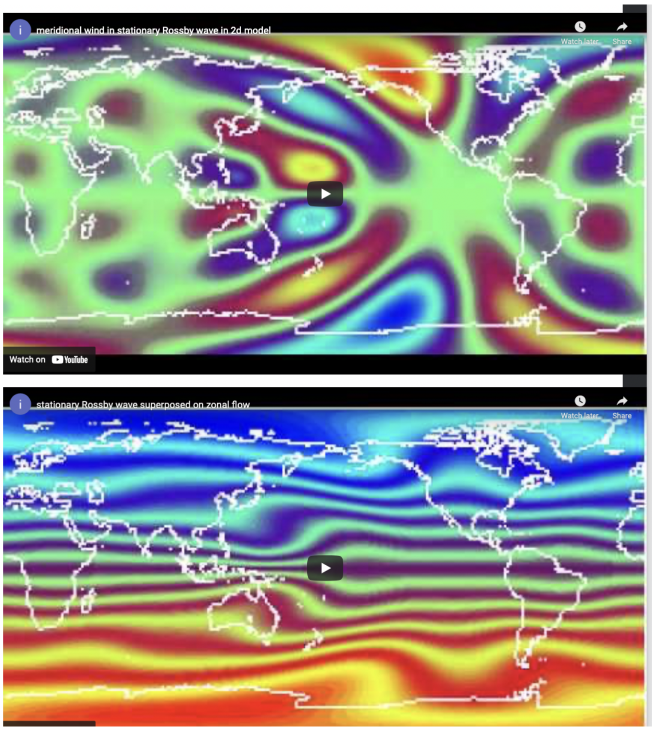


Figure 57.1: Snapshot of the response of a two-dimensional flow on the surface of a rotating sphere to a source that mimics stationary localized heating centered on the equator. The top panel is a north-south component of the wind — red is northward and blue southward. The bottom panel is the streamfunction of the flow –lines of constant streamfunction are the trajectories of fluid particles once the flow becomes steady.

The animations can be found here and here.

At the start of the animation the flow is purely zonal and the forcing is turned on instantaneously and then maintained. The loop covers about 40 days, but the pattern is fully set up in less than half that time. The continental outlines are just meant to help orient the viewer; the surface in this model is featureless. The setup is a classical one for generating a stationary Rossby wave propagating from the tropics into midlatitudes described by Brian Hoskins and colleagues in the late 1970's and early 80's (Hoskins et al 1977; Hoskins and Karoly 1981. This kind of wave is the essence of the teleconnections that atmospheric scientists talk about so frequently — patterns of flow that connect widely separated regions. Sometimes the correlations introduced into climate time series by these remotely forced responses can seem like spooky action-at-a-distance. But nothing could be further from the truth. They are just Rossby waves at heart.

The characteristic spatial scale of these stationary Rossby waves (not surprisingly known as the Rossby stationary wavelength) plays an important role in a lot of problems, not just in the response to tropical heating. About half a wavelength fits into the Continental US for example. So when the conditions are such as to favor anomalously poleward flow in the western half of the country, it is not unusual for there to be anomalously equatorward flow in the other half. There are no temperatures in this model, but you can imagine superposing this flow on an equator-to-pole temperature gradient with the equatorward and poleward flows advecting in cold and warm air.

(Is it advantageous for a political unit to control enough land to encompass at least half of a Rossby stationary wavelength, so that everyone is not hit simultaneously with the same kinds of weather extremes?)

The role of teleconnections in the extratropical response to ENSO was clarified by the observational analysis of Horel and Wallace 1980 combined with the theoretical work of Hoskins and collaborators mentioned above– see Trenberth et al 1998 for a review. The figure below is an example of the simulation of these Rossby-wave ENSO teleconnections in an atmospheric GCM running over observed sea surface temperatures. These are polar projections of the regression of eddy geopotential at 200mb (the height of the 200mb pressure surface with the zonal mean removed) onto an ENSO sea surface temperature index — for Dec/Jan/Feb with the Northern Hemisphere on the left and the Southern on the right. Reds are highs (anticyclonic in NH) and blue lows (cyclonic in NH). The model is on the top and observations from reanalysis on the bottom. (This height of a pressure surface can also be thought of as proportional to the streamfunction.) Results are from the GFDL's AM2.1 atmosphere/land model. The southern wavetrain gets less attention than its northern counterpart, but among other things plays a role in connecting trends in the Pacific to the pattern of temperature change around Antarctica.

There are some differences but there seem to be no fundamental mysteries here — no zeroth-order missing physics in the atmospheric model. Even freely running coupled models, if their ENSO variability is good enough, can generate teleconnection patterns with realistic amplitude and phase..

The animation at the top is a very idealized 2-dimensional model that is not meant to simulate the detailed pattern of response to any particular tropical heating, it is just meant to capture the essence of the underlying wave dynamics. To understand these responses you can use the geometric optics approximation which allows you to trace out the ray paths of the waves as they propagate through their inhomogeneous planetary-scale environment. To do this all you need is the local dispersion relation — the relation between the wave's frequency ω and its wavenumber $\boldsymbol{k} \equiv (k_x, k_y)$ for solutions that locally look like $\exp(i[k_x x + k_y y - \omega t])$. ($x$ increases eastward and y northward.) The remarkable dispersion relation for these simplest 2D Rossby waves propagating on a background zonal flow U is

$$\omega = Uk_x - \beta k_x/(k_x^2 + k_y^2) \;\; or \;\; c \equiv \omega/k_x = U - \beta/(k_x^2 + k_y^2) \qquad (57.1)$$

where c is the speed with which the phase of the wave propagates eastward. Understanding β is the key to understanding Rossby waves, but let's not worry about it for the moment (It is vital that β is positive.) So the phase speed is always westward with respect to the zonal wind U on which the wave is propagating; stationary (c=0) Rossby waves can only exist if this zonal wind is positive. This is the typical situation in the troposphere, so stationary Rossby waves do exist. In the idealized model underlying the animation, U is set equal to $(20m/s)cos(\theta)$ where θ is the latitude. This is not particularly realistic, but among other things it avoids the question as to whether the wave can get out of the tropics, where U is weak in reality. The wavelength of the stationary wave is

$$2\pi/\sqrt{k_x^2 + k_y^2} = 2\pi\sqrt{U/\beta}$$

.

A distinctive property of this dispersion relation that colors much of meteorology is that waves longer than the stationary wavelength propagate to the west, the longer the wavelength the faster the westward propagation, while shorter waves propagate to the east with respect to the surface — but still to the west with respect to the flow U that they are riding on. These shorter waves are the essence of the eastward propagating highs and lows we are all familiar with from midlatitude weather. They are more nonlinear than their larger wavelength stationary or westward propagating cousins, in part because they are destabilized by their interactions with the surface temperature field, and they roll up into vortices. One can think of the atmosphere as bathed in a spectrum of these waves of different scales, with a time average over more than a few weeks selecting out those wavelengths that are more or less stationary.

For those comfortable with the concept of group velocity, you can compute the x-component of the group velocity by differentiating the dispersion relation with respect to x:

$$G_x = \partial\omega/\partial k_x = U - \beta/(k_x^2 + k_y^2) + 2\beta k_x^2/(k_x^2 + k_y^2)^2 \tag{57.2}$$

implying that

$$G_x = c + 2\beta k_x^2/(k_x^2 + k_y^2)^2 \tag{57.3}$$

So the zonal group velocity is always eastward with respect to the phase speed and, in particular, stationary waves always have eastward group velocities — a striking property of Rossby waves. This explains why the propagation of the wavefront in the animation is eastward and why there is a large wavelike response over North America to the rearrangement of convection in the tropical Pacific associated with ENSO. If you compute the ray paths of the stationary waves emanating from the tropic for this special case in which $U \propto \cos(\theta)$ (θ is latitude) it turns out that they are great circles. All great circles passing through the source meet again at the antipodal point, which you can sort of see in the animation. This setup is nice pedagogically because of the simplicity of the ray paths. These ray paths can be more complicated in more realistic settings, but they often retain a great circle-ish aspect. (I have added a linear damping with 10 day e-folding so that the waves don't have enough time to go into the other hemisphere and then return to the source and interfere with themselves –there is substantial dissipation in the atmosphere and, in any case, this kind of back and forth trajectory between the hemispheres is not relevant when the waves are propagating on more realistic zonal flows.)

So what is β in the Rossby wave dispersion relation? It is the northward gradient of the radial component of the vorticity. In the special case of solid body rotation, you can show that the radial component of the vorticity is $2\Omega\sin(\theta)$, also known as the Coriolis parameter, f, which increases monotonically from the south pole to the north pole. (Ω is the rotation rate and θ is latitude once again.) So the northward gradient, $\beta = 2\Omega\cos(\theta)/a$, where a is the radius of the sphere, is positive everywhere. Plugging in some values for Ω, a, U you should get a Rossby stationary wavelength consistent with the results described above. If the flow is approximately in solid body rotation (remember that the surface of the Earth in an inertial reference frame is moving eastward at over 460 m/s at the equator) this northward vorticity gradient will be dominated by the contribution from the solid body rotation of the planet and the flow will support Rossby waves by a beautiful mechanism that I would like to return to in another post. In the meantime, here are some pictures to look at.

By the way, there are still a lot of open questions of climate relevance that one can begin addressing in this simple setting of two-dimensional flow on a sphere.

58 Addicted to Global Mean Temperature

[Originally posted March 31 2015]

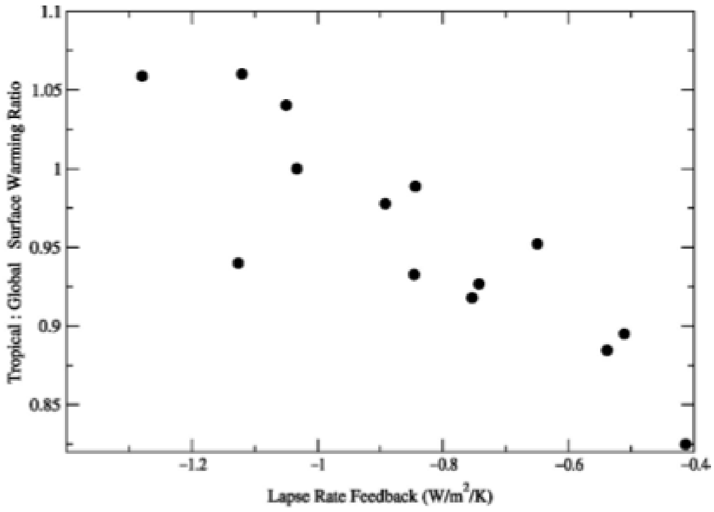


Figure 58.1: Traditional "lapse rate feedback" in CMIP3 models, over the 21st century in the A1B scenario, plotted against the degree of polar amplification of surface warming in those models (tropical – 30S-30N divided by global mean warming). From Soden and Held 2006.

"Everything should be made as simple as possible, but not simpler." There is evidently no record of Einstein having actually used these words , and a quote of his that may be the source of this aphorism has a somewhat different resonance to my ear. In any case, I want to argue here that thinking about the global mean temperature in isolation or working with simple globally averaged box models that ignore the spatial structure of the response is very often "too simple". I am reiterating some points made in earlier posts, especially 5, 7, and 44, but maybe it is useful to gather these together for emphasis.

Consider two regions A and B which together cover the globe. Suppose that we have excellent observations of the mean temperature of A over time and relatively few of B. Let's also consider the admittedly extreme case with negligible internal variability and CO_2 the only external agent causing change. Now assume that some new observations of the evolution of temperatures in B are obtained, resulting in larger trends in B and therefore in the global mean as well. The result is an increase in the estimate of climate sensitivity (transient climate response to be precise) since this quantity is traditionally defined using the global mean temperature. Which is OK, but someone living in A might read of this upward revision of climate sensitivity and mistakenly conclude that the projected response to CO2 in A has increased. Of course, given this setup what the new observations are telling us is that the response to CO_2 has a different pattern than what we had thought, not that the response to CO_2 is everywhere larger than previously estimated. This scenario is meant to be reminiscent of some of the reaction to the recent work of Cowtan and Way 2014. Putting aside the question of the quantitative implications of that particular study for estimates of the transient climate response, I think this is an example of how the emphasis on the global mean in isolation can be misleading.

Suppose more realistically that there is substantial internal variability, plus other forcing agents, as well as uncertainty in the pattern of the response to CO_2 to deal with. Then it is possible that observations in B could modify estimates of the change in A attributable to CO2, depending on how the covariability in A and B intersects with what we know and don't know about these various factors. The effect on the attributable A response might be positive or negative however.

Or consider the connection between global mean surface temperatures and the Earth's energy balance. This has become a hot topic, with a number of perspectives on this emerging, some of which I have talked about in previous posts. In the simplest box model, perturbations to the global mean energy flux at the top of the atmosphere (TOA) — or what is essentially the same thing on the time scales of interest, perturbations to the heat uptake H by the oceans — are assumed to be a simple function of the radiative forcing F and perturbations in global mean surface temperature T, ie $H = F - \beta T$. But, among other issues, different spatial patterns of warming with the same global mean can produce different spatially integrated responses in the TOA energy flux. You might be able to get away with the simplest of models, dealing only with global means, when the spatial structure of the temperature response is self-similar: $\delta T \propto f(x, y)g(t)$. But you cannot expect it to be accurate in general.

In models, the effective strength of the radiative restoring is stronger for perturbations in tropical temperatures than for perturbations in high latitude temperatures. In addition, temperature responses are less polar amplified in the initial as compared to the final stages of the approach to a new equilibrium with elevated CO_2. So equilibrium climate sensitivity is increased beyond what you would expect from fitting heat uptake, forcing, and temperature responses during the initial stage — when the stronger radiative restoring at lower latitudes plays a bigger role. This is sometimes referred to as the difference between "effective climate sensitivity" and equilibrium climate sensitivity. But beyond this distinction in the global mean response, there is the tendency to miss the point that this enhanced climate sensitivity due to the structure of the slow response has larger consequences for polar than for equatorial regions.

Trying to think about these issues while focusing on the global mean in isolation tempts people to think about nonlinearity to explain this behavior, whereas the explanation seems to be primarily that the spatial structure of the linear response is a function of frequency.

As another example, one approach to thinking about the recent hiatus is to focus on the energy balance of the Earth, asking where the energy has gone. But suppose we are looking at some superposition of forced and internal variability (a safe assumption). Both affect the global mean surface temperature and both affect the global mean TOA energy balance (and heat uptake by the oceans), but not necessarily with the same restoring strength β. The forced response and internal variability can have very different spatial structures after all. You can't go back and forth from global mean energy balance to global mean temperature that easily.

Additionally, the hiatus is mostly reflecting temperature evolution in northern hemisphere winter, where there has been a cooling trend over the past one or two decades (Cohen et al 2012) The global and annual mean receives so much emphasis that the important constraint this seasonal and spatial structure imposes on explanations for the temperature evolution is often ignored. Take for instance the idea that an increase in heat uptake in the south Atlantic is important for closure of the energy budget in recent years (Chen and Tung 2014). Suppose you could rerun the climate over the past couple of decades and command the South Atlantic not to increase its heat uptake (this is what models are for) — how would surface temperatures respond? I could be wrong, but I suspect that most of the warming would be in the southern hemisphere, with much of the excess heat radiated away in the southern hemisphere as well, with minimal impact on temperatures in northern winter. If this is the way to close the Earth's energy budget, it does not strike me as plausible that there is a tight connection to the recent hiatus. (I hasten to add that there are other reasons to want to close the

Earth's energy budget.)

As another example, consider the accumulated emission perspective on long-term climate change after emissions cease, in which slow carbon uptake over centuries compensates approximately for the slow equilibration of the climate to the evolving CO_2 levels. The Southern Ocean plays a leading role for both carbon and heat uptake. And from a global perspective these are competing to change the same global mean temperature. But CO_2 is well mixed in the atmosphere on time scales longer than a year or two, so any uptake of carbon affects both hemispheres with roughly equal radiative forcing. But uptake of heat in the Southern Oceans affects the southern more strongly than the northern hemisphere. This distinction can get lost when discussing this accumulated emission perspective.

Finally, I've included a figure illustrating the spread in the strength of the lapse rate feedback in GCMs at the top of this post. This term measures how much the global mean TOA flux is modified by the fact that temperature changes aloft are not the same as at the surface, holding water vapor and clouds fixed. It is negative in models because it is dominated by the tropics where temperature changes are larger aloft than at the surface. The resulting change in the TOA flux is normalized by the global mean surface temperature change because this is a term in a feedback analysis that focuses on explaining the value of β in the simplest global mean energy balance model. One gets a big spread across models in the strength of this term, which can be (mis)interpreted as evidence that the models differ a lot in the physics determining the vertical structure of the response. But as the figure makes clear much of this spread is due to differences in the polar amplification of the surface warming — since most of this feedback is coming from the tropics it scales with the tropical, not the global mean, surface change. This is not to say that the remaining spread is not interesting, but the normalization by the global mean temperature change evidently disguises a major source of the difference across models.

I make these points in large part as self-criticism. The simple global mean perspective is addictive and I am sure that I'll succumb again sooner rather than later.

59 How (not) to Evaluate Climate Models

[Originally posted May 13 2015]

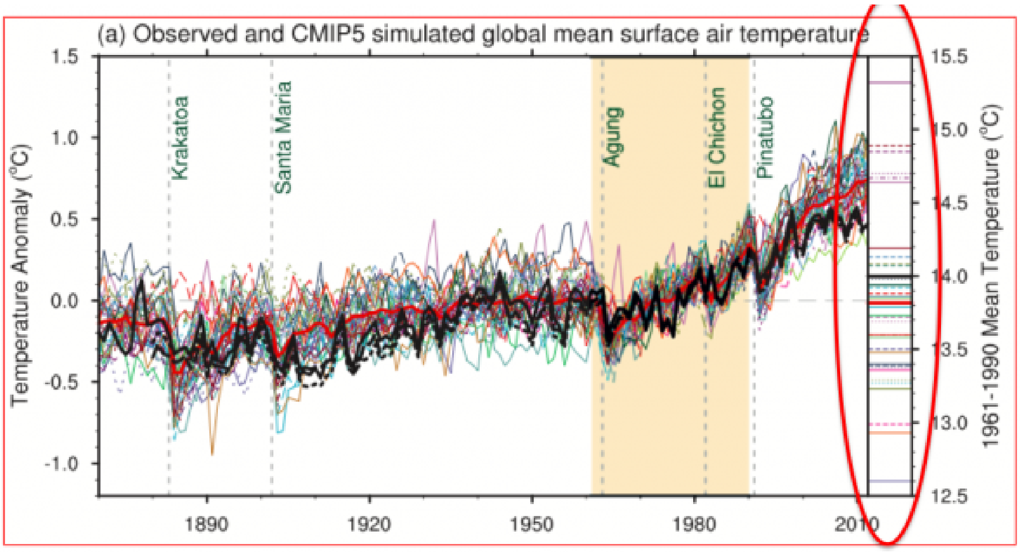


Figure 59.1: Global mean surface temperatures simulated by a set of climate models, shown as anomalies from the time mean over a reference period 1961-1990. Observations (HADCRUT4) in black; ensemble mean in red. On the right (circled) are the mean temperatures in the reference period.

Ch.9 of the AR5-WG1 report, "Evaluating Climate Models" is, in my opinion, the most difficult to write of any chapter in that report. You can think of hundreds if not thousands of interesting ways of comparing modern climate models to observations, but which of these is the most relevant for judging the quality of a projection for a particular aspect of climate change over the next century? This is an important research problem. Consider this figure, which shows the familiar simulated changes in global mean surface temperature over the past 150 years, in a set of models deposited in the CMIP5 archive, as anomalies from the model's own temperature during some reference period (shaded). But the figure also shows in the narrow panel on the right side, circled in red, the models' mean temperatures during that reference period.

People tend to be disappointed when they see this — some models are better than others but the biases in the model's global mean temperature are typically comparable to the 20th century warming and in some cases larger. If we are interested in projections of global mean warming over the coming century, or in the attribution of this past warming, should we trust these models at all, given these biases?

I would claim that it cannot be a valid requirement in general that the bias in some quantity needs to be small compared to the change that we are trying to predict or understand. Suppose we are interested in the forced response of global mean temperature to an increase of 10% or just 1% in CO2 rather than a 100% increase. Do biases in the models that we use for this purpose have to be 10 or 100 times smaller in order to trust their responses to these smaller perturbations? This makes no sense to me. I happen to think that these responses are quite linear over this range, in which case the size of the perturbation obviously has little relevance. But I am hard pressed to imagine any picture in which the bias in global mean temperature would have to be smaller than 0.01C to justify using a model to study global mean temperature responses. (The difficulty of studying very small responses in the presence of internally generated variability is a different issue entirely — if you were really interested in the response to such a small perturbation in a model for theoretical reasons, perhaps to test for linearity, you would have to generate a very large ensemble of simulations to average out the internal variability.)

On the other hand, consider sea ice extent. If your simulation is way off, it's going to be hard to simulate the retreat of sea ice quantitatively — interactions between sea ice and the ocean circulation are likely seriously distorted due to the complexity of the ocean basin geometry. Plus, too extensive sea ice, say, would put it in regions of more incident solar flux, affecting the strength of albedo feedback. Sea ice issues are likely to be nonlinear in the sense that the mean state that you are perturbing matters a lot.

Why don't models do better on the mean temperature? I think it is fair to say that all climate models have parameters in their cloud/convection schemes which which they tune their energy balance. This optimization step is necessary because cloud simulations simply are not good enough to get an energy balance to less than 1 W/m^2 from first principles. Is it that some modeling groups are not very good at tuning their models?

One possibility is that there are tradeoffs between optimizing global mean temperature and some other aspect of the simulation. Imagine that a model has a bias in its pole-to-equator temperature gradient and that it is easier to adjust the models mean temperature up and down, with some parameter in the cloud scheme perhaps, than to correct this bias in the gradient. The result might be a choice between optimizing the global mean temperature and the sea ice extent. How would you weigh the importance of the bias in sea ice extent vs the bias in global mean temperature? I would probably give more weight to the ice because that is where the sensitivity to the mean state is likely to be stronger.

But this kind of explicit tradeoff is probably not the dominant reason for the bias in global mean temperature in most models. It is more likely that the models cloud schemes have been tuned to get a good temperature using relatively short runs of the model and then when one does longer multi-century integrations the model drifts — and it may be too expensive to iterate the model using these long integrations. So you live with the bias resulting from this slow drift.

Rather than the simulation of the climatology, why not use simulated trends in some quantity of interest as the metric with which to judge the credibility of model projections of that same quantity? If you are confident that the observed trend can be attributed to known forcings this is fine, but the familiar issue with uncertain aerosol forcing and uncertain contribution from internal variability makes this problematic for the global mean temperature, and the same issues arise for other quantities. The more credible and quantiative the attribution claim, the more valuable observed trends are for model evaluation.

Different views on the relative importance of different metrics are partly responsible for divergence between models. Are you better off with an optimized simulation of top-of-atmosphere spatial patterns of incoming and outgoing radiative fluxes, or of precipitation patterns? What if a proposed change in a model improves Amazon precipitation but causes African rainfall to deteriorate? If you are interested in how ENSO may evolves in the future under different emission scenarios, is it better to use a metric based on the quality of ENSO in simulations of the past century, or is it better to to use the same model to make seasonal forecasts of ENSO and use the skill of those forecasts as a metric? (Of course seasonal forecast skill is important in its own right — but its value as a metric compared to other possible metrics for a model of climate change is less self-evident.) If our models were close enough to nature, it would not matter which metric we used to push them even closer because all metrics would give a consistent picture. That different metrics agree or disagree on which of two versions of a model is better is itself a hint of how far one is from a fully satisfactory simulation.

Rather than defining metrics in a subjective way, basically guessing which metrics are most important, you can ask which metrics matter for a particular projection (ie of Sahel rainfall). If I sort models using some metric, some way of comparing the model to observations, does this also discriminate between model projections (i.e. between a dry Sahel in the future or a wet Sahel)? If so, and if I believe that this connection is physical, I can use it to sharpen my projection using that model ensemble. If there is no correlation between the metric and the projections, even if you were convinced that the metric was relevant, there would be no direct way of using it together with that ensemble of model results to improve the projection. This approach, sometimes referred to as looking for *emergent constraints*, strikes me as the most promising for the design of useful metrics. Returning to the figure at the top, should you use the value of a model's mean bias to weight that model's contribution, within a model ensemble, to future projections of global mean temperature? I don't thinks so. The bias is not strongly correlated with the projected temperature change, as documented in Ch.9 (Fig. 9.42) of the WG1 AR4 IPCC report. .

60 The Qaulity of the Large-Scale Flow Simulated in GCMs

[originally posted June 7 2015] Given the problems that our global climate

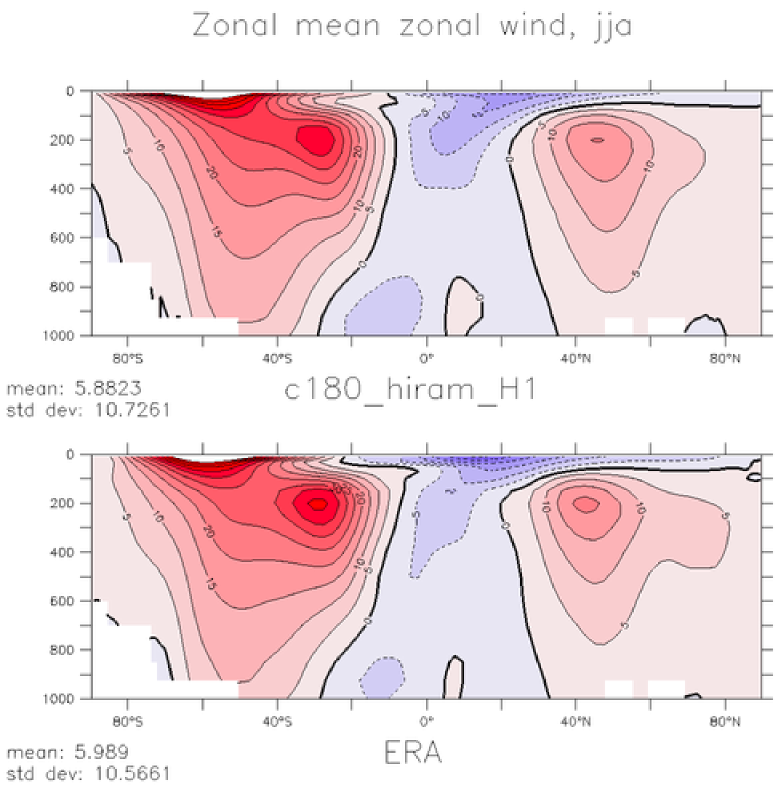


Figure 60.1:

models have in simulating the global mean energy balance of the Earth, some readers may have a hard time understanding why many of us in climate science devote so much attention to these models. A big part of the explanation is the quality of the large-scale atmospheric circulation that they provide. To my mind this is without doubt one of the great triumphs of computer simulation in all of science.

The figure above is meant to give you a feeling for this quality. It shows the zonal (eastward) component of the wind as a function of latitude and pressure, averaged in time and around latitude circles. This is an atmosphere/land model running over observed ocean temperatures with roughly 50km horizontal resolution. The model results at the top (dec-jan-feb on the left and june-july-aug on the right) are compared with the observational estimate below them. The observations are provided by a reanalysis product(more on reanalysis below). The contour interval is 5m/s; westerlies (eastward flow) are red, easterlies are blue. Features of interest are the location of the transition from westerlies to easterlies at the surface in the subtropics, and the relative positions of the the subtropical jet at 200mb, the lower tropospheric westerlies and the polar stratospheric jet in winter (the latter is barely visible near the upper boundary of the plot when using pressure as a vertical coordinate).

Fig.60.2 below is also of the zonal component of the wind averaged over the same two seasons, but now on the 200mb surface, close to the subtropical jet maximum near the tropopause. Features of interest here include the orientations of the Pacific and Atlantic jets in the northern winter (models often have difficulty capturing the degree of NE-SW tilt of the Atlantic jet) the secondary westerly maxima over the northern tropical oceans in northern summer (time-mean signatures of the tropical upper tropospheric troughs — TUTs) and the split in the jet over New Zealand in southern winter. The contour interval here is 10m/s.

This circulation cannot be maintained without realistic simulation of the heat and momentum fluxes due to the dominant eastward propagating midlatitude storms familiar from weather maps. These fluxes depend not just on the magnitude of these eddies but also the covariability of the eastward component of the wind (u), the northward component of the wind (v), and the temperature (T). Focusing on the winter only, Fig.60.3 shows maps of the northward eddy heat flux, the covariance between v and T, and the eddy northward flux of eastward momentum, the covariance between u and v. The latter, in particular, turns out to be fundamental to the maintenance of the surface winds and can be challenging to capture quantitatively.

The plots in Fig. 60.3 show these fluxes only for eddies with periods between roughly 2 and 7 days (fluxes due to lower frequencies are also significant but have different dynamics and structures.) Each flux is shown at a pressure level close to where it is the largest: the eddy heat flux is largest in the lower troposphere, while the momentum fluxes peak near the tropopause. The storm tracks, marked by the maxima in the down-gradient

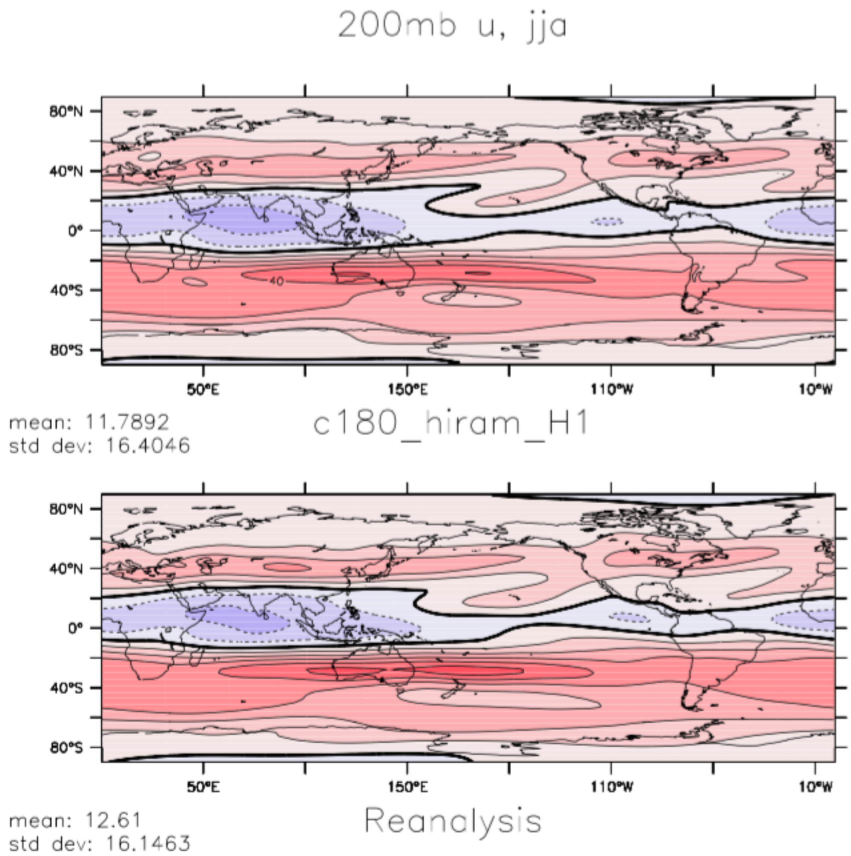


Figure 60.2:

poleward eddy heat fluxes in the lower troposphere, are accompanied by a dipolar structure of the momentum fluxes in the upper troposphere, with meridional convergence of eastward momentum into the latitude of the storm track. The eddies responsible for these fluxes have scales of 1,000 km and greater. This is what we mean by the term large-scale flow in this context.

I am using reanalysis as the observational standard for these fields, an idea that takes some getting used to. Weather prediction centers need initial conditions with which to start their forecasts. They get these by combining information from past forecasts with new data from balloons, satellites, and aircraft. These are referred to as analyses. If you took a record of all of these analyses as your best guess for the state of the atmosphere over time, it would suffer from two inhomogeneities — one due to changes in data sources and another due to changes in the underlying model that

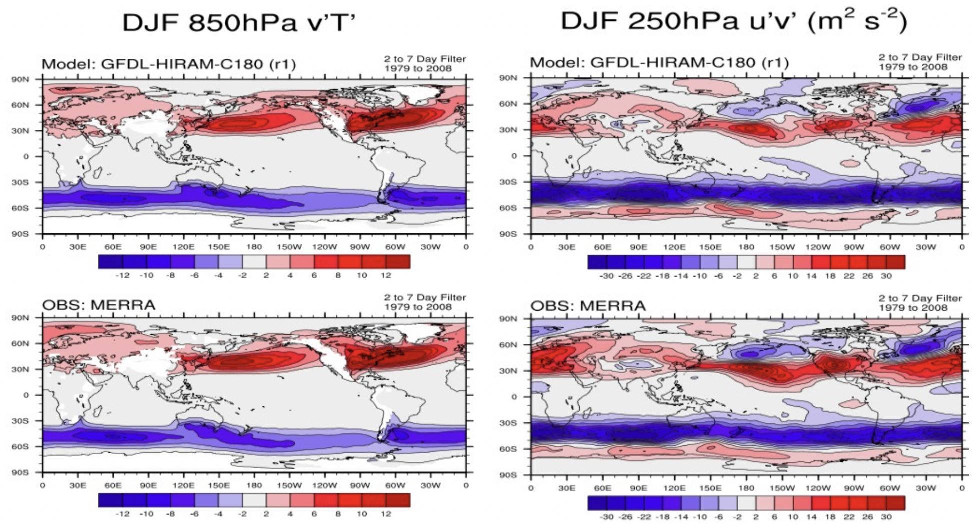


Figure 60.3:

the data is being assimilated into. Reanalyses remove the second of these inhomogeneities by assimilating an entire historical data stream into a fixed (modern) version of the model. They still retain the inhomogeneity due to changing data sources over time. Where data is plentiful the model provides a dynamically consistent multivariate space-time interpolation procedure. Where data is sparse, one is obviously relying on the model more.

The multivariate nature of the interpolation is critical. As an important example, horizontal gradients in temperature are very closely tied to vertical gradients in the horizontal wind field (for large-scale flow outside of the deep tropics). it makes little sense to look for an optimal estimate of the wind field at some time and place without taking advantage of temperature data. The underlying model and the data assimilation procedure handle this and less obvious constraints naturally. Importantly, the model can propagate information from data rich regions into data poor regions if this propagation of information is fast enough compared to the time scale at which errors grow. For climatological circulation fields such as the ones that I have shown here reanalyses provide our best estimates of the state of the atmosphere. For the northern hemisphere outside of the tropics these estimates are very good — I suspect that they provide the most accurate description of any turbulent flow in all of science. For the tropics and for the southern hemisphere the differences between reanalyses can be large enough that estimating model biases requires more care.

I am claiming that the comparison to reanalyses is a good measure of the quality of our simulations for these kinds of fields. (You need to distinguish estimates of the mean climate described here from estimates of trends, which are much harder.) If you accept this then I think you will agree that the quality seen in the free-running model (with prescribed SSTs) is impressive (which does not mean that some biases are not significant, for regional climates especially). This quality is worth keeping in mind when reading a claim that atmospheric models as currently formulated are missing some fundamentally important mechanism or that the numerical algorithms being used are woefully inadequate.

I would also claim that these turbulent midlatitude eddies are in fact easier to simulate than the turbulence in a pipe or wind tunnel in a laboratory. This claim is based on the fact the atmospheric flow on these scales is quasi-two-dimensional. The flow is not actually 2D — the horizontal flow in the upper troposphere is very different from the flow in the lower troposphere for example — but unlike familiar 3D turbulence that cascades energy very rapidly from large to small scales, the atmosphere shares the feature of turbulence in 2D flows in which the energy at large horizontal scales stays on large scales, the natural movement in fact being to even larger scales. In the atmosphere, energy is removed from these large scales where the flow rubs against the surface, transferring energy to the 3D turbulence in the planetary boundary layer and then to scales at which viscous dissipation acts. Because there is a large separation in scale between the large-scale eddies and the little eddies in the boundary layer, this loss of energy can be modeled reasonably well with guidance from detailed observations of boundary layer turbulence. While both numerical weather prediction and climate simulations are difficult, if not for this key distinction in the way that energy moves between scales in 2D and 3D they would be far more difficult if not totally impractical.

I have been focusing on some things that our atmospheric models are good at. It is often a challenge to decide the relative importance, for any aspect of climate change, of the parts of the model that are fully convincing and those that are works in progress, such as the global cloud field or specific regional details (you might or might not care that a global model produces a climate in central England more appropriate for Scotland). You can err on the side of inappropriately dismissing model results; this is often the result of being unaware of what these models are and of what they do simulate with considerable skill and of our understanding of where the weak points are. But you can also err on the side of uncritical acceptance of model results; this can result from being seduced by the beauty of the simulations and possibly by a prior research path that was built on utilizing model strengths and avoiding their weaknesses (speaking of myself here).

The animation in post 2 is produced by precisely the model that I have used for all of the figures in this post. I find this animation inspiring. That we can generate such beautiful and accurate simulations from a few basic equations is still startling to me. I have to keep reminding myself that there are important limitations to what these models can do.

A final comment: For those who have looked at the CMIP archives and seen bigger biases than described here, keep in mind that I am describing an AMIP simulation — with prescribed SSTs. The circulation will deteriorate depending on the pattern and amplitude of the SST biases that develop in a coupled model. Also this model has roughly 50km horizontal resolution, substantially finer than most of the atmospheric models in the CMIP archives. These biases often improve gradually with increasing resolution. And there are other fields that are more sensitive to the sub-grid scale closures for moist convection, especially in the tropics.

61 Tropical Tropospheric Warming Revisited - Part III

[Originally posted August 3 2015]

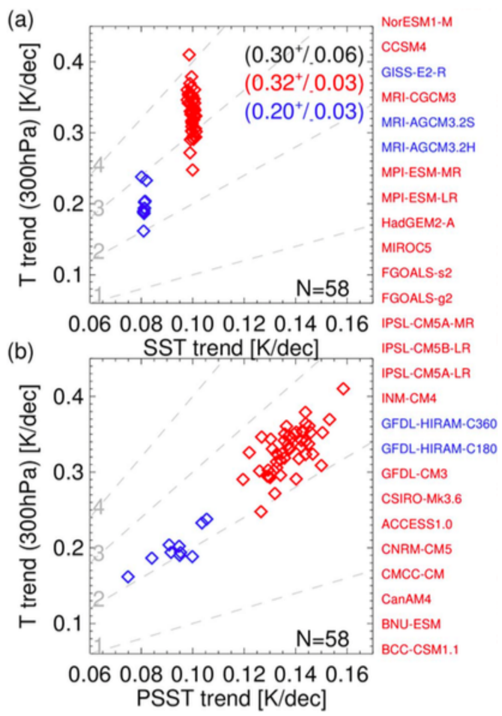


Figure 61.1: Upper tropospheric warming trends in AMIP (prescribed sea surface temperature) simulations at 300mb over the period 1984-2008 averaged from 20S-20N, in the CMIP5 archive. Red and blue correspond to models that used two different SST data sets. SST on the x-axis in the upper panel is the mean SST over the tropical oceans. PSST in the lower panel is the mean of SST over the tropical oceans weighted by the pattern of precipitation simulated in each model.

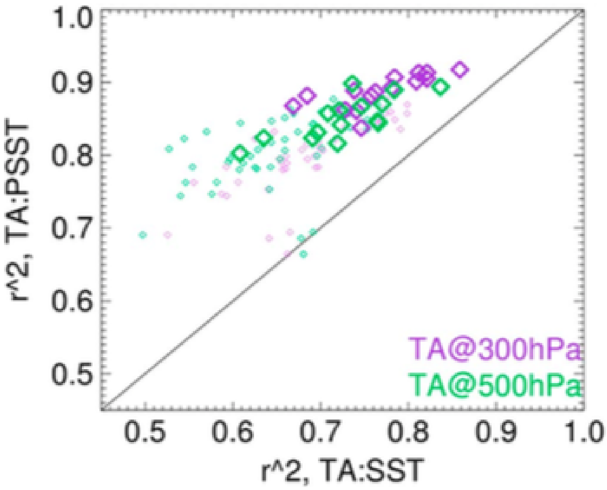
Let's turn once again to the problem of the vertical structure of warming trends in the tropical troposphere. (Warning: this post contains no comparison with data — the point is to try to clarify how to think about the GCM simulations.) In two recent posts, 54 and 55, I discussed a paper, Flannaghan et al 2014, in which my colleagues and I focused on the simulations in the CMIP archives in which sea surface temperatures (SSTs) are prescribed following observations ("AMIP" simulations). There is interesting spread in the upper tropospheric warming trends even in these AMIP simulations, as indicated in the figure above, taken from a new paper Fueglistaler et al 2015. Each dot corresponds to an AMIP run from a different model. In the upper panel, the 300mb temperature trends are plotted against the trends in the prescribed tropical SSTs.

It turns out that not all of these models use the same estimate of the observed SSTs. The two different data sets used have modestly different trends in the tropical mean SSTs, and those simulations that use the data set with the larger trend produce larger upper level trends as well, not surprisingly. But it was surprising to us how big the difference was, given the amplification expected from the usual moist adiabat argument. As described in post 55, we can evidently explain this difference by looking at the SSTs weighted by the precipitation when taking the tropical average (We call this quantity PSST here.) The upper and lower troposphere in the tropics are strongly coupled only in the regions of deep convection. In the simplest picture, the rest of the tropics adjusts its temperatures to the moist adabat set by these convecting regions. We use precipitation as a marker for these regions. The difference between PSST in these two sets of simulations is bigger and more consistent with the difference in upper level trends.

In the new paper we look further at the differences across the simulations that use the same SSTs. As seen in the figure, using PSST also helps explain the spread in upper level warming among these simulations. The result is a pretty consistent ratio, about 2.5, between the 300mb temperature trend (this is roughly the level where this trend is largest in the models) and the surface trends in PSST across all of the simulations.

This means that the spread across the simulations with identical SSTs is explained in large part by the differences in the simulated precipitation patterns, resulting in different trends in PSST. This spread is partly due to differences between models and partly just due to differences in the precipitation in different realizations of the same model resulting from internal atmospheric variability. Both of these sources of spread seem to contribute.

If we remove the linear trends from each simulation and then correlate the month-to-month variations in the upper level tropical mean temperatures with the surface, we find consistently larger correlations with the mean tropical PSST than with the unweighted SST itself (see below for a plot of the explained variance using PSST vs the explained variance using the tropical mean SST– the big symbols correspond to ensemble means for individual models; the little symbols to single realizations; purple refers to 300mb and green to 500mb temperatures — the figure is from Fueglistaler et al 2015. once again). This corroborates the idea that PSST is a good indicator of the information passed from the lower to upper troposphere. I was a bit surprised by this result, given an earlier paper that I was involved in that suggested that SST and PSST were interchangeable when thinking about the warming response to El Nino.



If we understand what underlies the spread across models it should be easier to to study differences between models and observations. This work suggests that observational estimates of PSST are valuable in this context.

PSST is defined using ocean temperatures only. It is important to exclude surface land temperatures when relating upper level and surface temperature trends. PSST is a surrogate for the entropy of near-surface air in the convecting regions. This entropy depends on humidity as well as temperature, but over the oceans it seems that we can avoid thinking about this because relative humidity does not change much. But over land this is not the case. It would make sense to include land in this kind of analysis if you know the entropy of the convecting regions, but you can't replace the entropy with the temperature. Confronting model results with observations would then require knowledge of humidity trends over land.

Since there is quite a bit of convection over tropical land, it is surprising to me that using only ocean temperatures works as well as it does in these two figures. Perhaps this is related to the idea that entropy changes over convecting land regions are constrained to be comparable to entropy changes over the convecting oceans. (See post 56.)

62 Poleward Atmospheric Energy transport

[Originally posted September 9 2015]

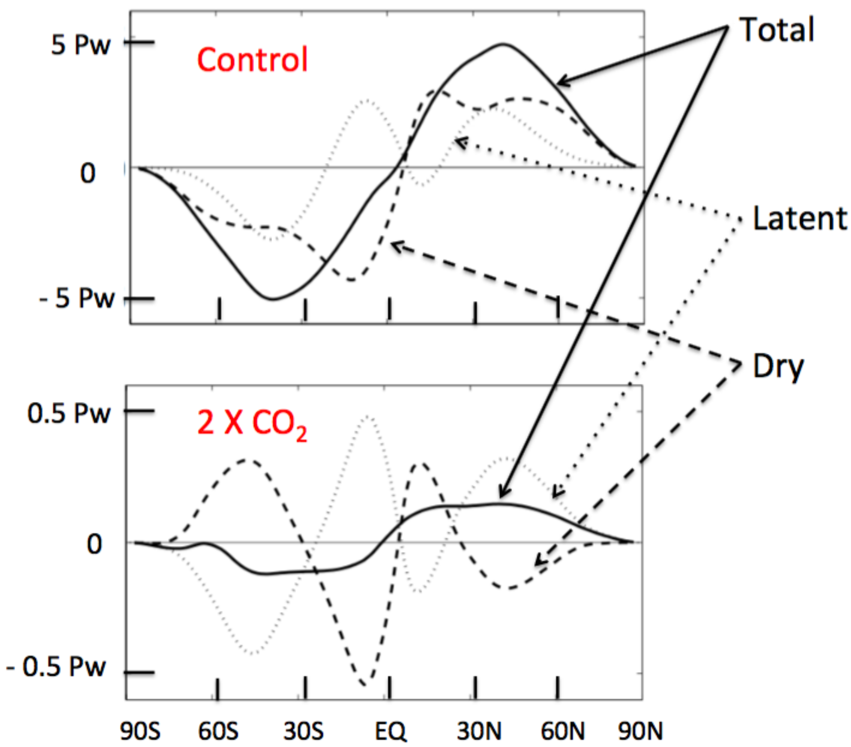


Figure 62.1: Upper panel: annual mean northward atmospheric energy transport as a function of latitude averaged over the control simulations in CMIP3. The total flux is shown as well as the decomposition into the latent flux and the dry static energy flux. Lower panel: Response of these fluxes to doubled CO_2 (2 x CO2 minus control) in slab-ocean models with fixed oceanic heat transport. Fluxes in petawatts. Courtesy of Yen-Ting Hwang.

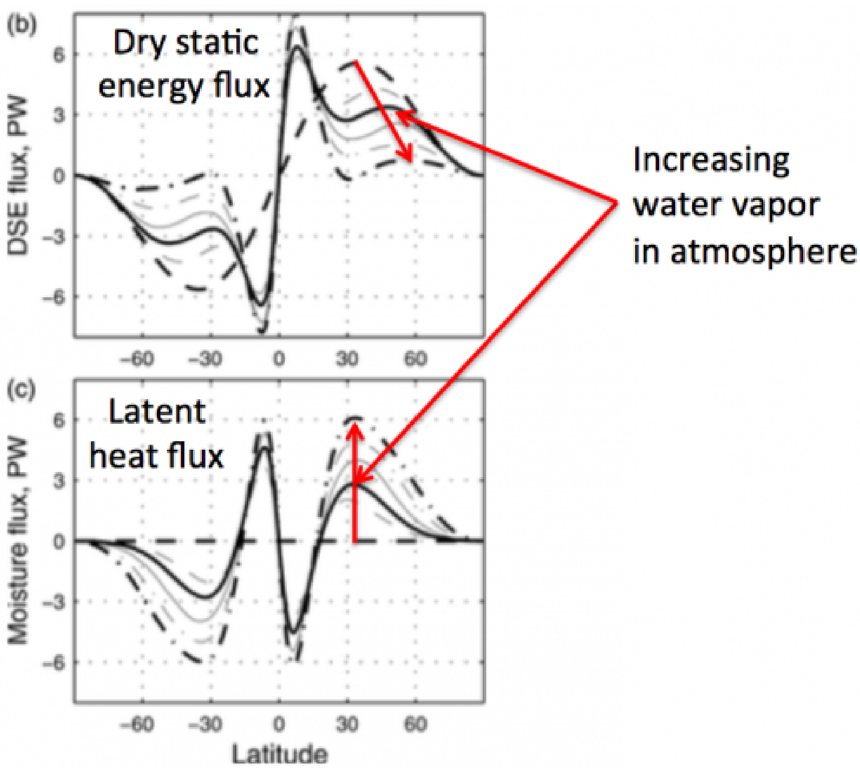
A warming atmosphere typically results in larger horizontal moisture transports. In addition to the implications for the hydrological cycle and oceanic salinity discussed in previous posts, this increased moisture transport also has implications for energy transport. If energy is used to evaporate water at point A and the vapor is transported to point B where it condenses, releasing the heat of condensation, energy has been transported from A to B. This latent heat transport is a large component of the total atmospheric energy transport. The thin dotted line in the top panel above is the northward latent heat transport averaged across the CMIP3 models. Outside of the tropics, eddies are mixing water vapor downgradient, resulting in a poleward transport. Close to the equator, the Hadley circulation dominates, with its equatorward flow near the surface that carries water vapor from the subtropics to the tropical rain belts (the compensating poleward flow near the tropopause carries very little water vapor in comparison). This tropical branch with equatorward vapor transport is clearer in the Southern Hemisphere in this plot, due I think to more continents and larger seasonal cycle of tropical rainfall in the north, producing less separation in the annual mean between the tropical and midlatitude rainbelts.

To get the total flux we add the remaining "dry" component, the flux of dry static energy, to this flux of latent energy. (I'll refer to the flux of dry static energy as the "dry" flux here. Some background on atmospheric energy fluxes can be found on pages 50-51 here.) This dry flux (dashed line) is directed polewards everywhere but tends to have two maxima, one in low latitudes due to poleward flux in the Hadley cell, where this transport counteracts the latent heat flux — and another in midlatitudes dominated by the transport of sensible heat by midlatitude storms. The total atmospheric flux is very simple in contrast, varying smoothly in latitude and directed from equator to pole in each hemisphere, with a midlatitude maximum. The sense you get is that the total flux is a simpler object to think about than its components. I am going to focus on the extratropical part of these fluxes in the following.

In global warming simulations, the extratropical poleward latent flux increases, more or less as expected from an assumption of fixed relative humidity and fixed wind fields. The total poleward flux also increases, but the increase is smaller than the increase in latent flux. Without going into the details of how the response of the total flux might be controlled, it is evidently constrained in ways that the latent and sensible fluxes individually are not, so it cannot keep up with the increasing latent flux. The upshot is that the poleward dry flux outside of the tropics decreases with warming. The lower panel shows the response of "slab ocean" models in the CMIP3 archive to a doubling of CO2. (Hwang et al 2011 correct some errors in a similar plot in Held and Soden 2006. This plot is slightly different from the figure in Hwang et al. where the individual model responses are divided by their global mean surface warming. I thank Yen-Ting Hwang for redrawing this figure for this post.) In these slab ocean models the atmosphere is coupled to a shallow stagnant layer of water. Prescribed fluxes of heat into or out of the lower boundary of this slab account for the horizontal heat transport by the oceans. Because the slab is thin it equilibrates much more quickly than a full ocean model. These equilibrated slab ocean simulations are a bit easier to think about than transient warming simulations with full ocean models in which there are changes in oceanic heat storage and transport. But in fact the analogous figure for a typical 21st century scenario is very similar, perhaps with a bit less compensation between the moist and dry components.

This extratropical dry flux is dominate by the eddy flux of sensible heat, proportional to [v'T'] where primes denote deviations form the zonal mean and the brackets an average over time, height, and longitude. The reduction in [v'T'] is potentially important for changes in weather — it must be associated either with a reduction in the typical temperature perturbations T' or in the north-south winds v' (or, conceivably, in the correlation between the two). This eddy flux is often thought of as having a diffusive flavor, with the eddies generating a downgradient turbulent diffusion of heat. One explanation that you sometimes see for the reduction in eddy sensible heat flux with warming is polar amplification, with a reduced north-south temperature gradient resulting in lower flux, given more or or less the same turbulent diffusivity. But the argument given here, if you can call it that (we haven't really explained why the total poleward flux doesn't increase as rapidly with warming as the extratropical latent flux), does not seem to have anything to do with polar amplification, at least explicitly. Is the explanation based on polar amplification misleading? I'll just leave this question hanging for now.

Something else interesting happens due to this compensation between extratropical latent and dry fluxes. It is easier to see this effect if you increase the water vapor by an even larger amount than in these global warming scenarios. My colleagues and I have looked at this in a more idealized setting (Frierson et al 2007). This is an atmospheric model with water vapor, evaporation and precipitation and latent heat transport, coupled to a slab ocean. But there are no clouds and we assume a grey atmosphere with radiative fluxes a function of temperature only, so water vapor does not interact with the radiative transfer. The model climate is symmetric about longitude circles and about the equator. The model still has the complexity of deep moist convection in the tropics and storms modified by latent heat release in midlatitudes. We could call this model the mouse of climate models, in the spirit of the fruit fly model discussed in earlier posts. Rather than increase the temperature to see what happens when the water vapor increases, we just increase the water vapor more directly by increasing the saturation vapor pressure. I think this is a nice framework for addressing the kinds of questions exemplified by the discussion above, and I don't think we understand this model very well. This figure from



Frierson et al, shows the changes in the dry flux and the latent flux for very large changes in water vapor. We even go to the dry limit (the dashed line) with no water vapor in the atmosphere. It turns out, interestingly enough, that this model generates a more precise compensation between the changes in latent and dry fluxes than more comprehensive GCMs. But the point I want to focus on here is the poleward movement of the maximum in dry flux with increasing water vapor. The extratropical poleward latent flux peaks at a lower latitude that the dry flux. So when it increases, the compensating decrease in the dry flux is larger on the equatorward side of the dry flux maximum, causing a poleward shift in this maximum.

In many kinds of climate models we find that the atmospheric circulation and the associated climate regimes are shifted polewards with warming on average — in particular the latitude of the maximum kinetic energy in midlatitude storms shifts polewards. Can this shift be understood in more or less the same way as the shift in the dry flux? An increase in water vapor makes midlatitude storms more efficient at transporting energy since the latent heat flux adds constructively to the dry flux, but this increase in efficiency is greater on the equatorward side of the maximum in storm activity. So maybe we can think of the storms shifting towards the poles because that is where there is more work left to do, relatively speaking. There are an impressive number of competing ways of thinking about the poleward shift in circulation with warming. Maybe this one deserves a closer look.

63 How Unusual is the Recent Evolution of the Tropical Pacific

[Originally posted October 3 2015]

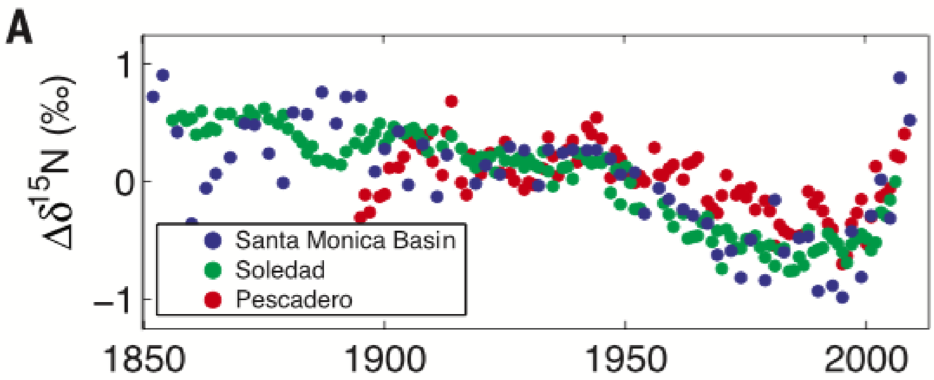


Figure 63.1: A proxy for the strength of the trade winds in the North Pacific: nitrogen isotope records from three sediment cores off the west coast of North America (blue = $33°N$, green = $25°N$, red = $24°N$). More ^{15}N is interpreted as stronger trades. From Deutsch et al 2014.

The warming of the globe over the last couple of decades has been slower than the forced warming predicted by most GCMs, due to some combination of internal variability, incorrectly simulated climate responses to the changes in forcing agents, and incorrect assumptions about the forcing agents themselves. A number of studies have implicated the tropical Pacific as playing a central role in this discrepancy, specifically a la Nina-like trend — with eastern equatorial Pacific cooling and strengthening trade winds. If you intervene in a climate model by imposing the observed near-surface ocean temperatures in the eastern equatorial Pacific (Kosaka and Xie 2013) or by imposing the observed surface equatorial Pacific wind fields (England et al 2014; Delworth et al 2015), the rest of the simulation falls into place– not just the global mean temperatures but the spatial pattern of temperature trends over the past two decades — as well as the California drought. The wind and ocean surface temperatures are tightly coupled on annual and longer time scales in this region, so these different studies tell a consistent story. The implication is that explanations for the discrepancy in global warming rate need to simultaneously explain this La Nina-like trend to be convincing. Based on its importance in recent decades, it is tempting to assume that the tropical Pacific has played an important role in modulating the rate of global warming throughout the 20th century. But if the nitrogen isotope record on the eastern margins of the subtropical north Pacific shown above is a good proxy for trade wind strength, it is interesting that it isn't dominated by quasi-periodic multi-decadal variability. Instead it looks like a long term trend towards weaker trades until the last 20 years or so — a trend that happens to be roughly consistent with the forced response of the tropical winds to greenhouse warming in most models — which is then interrupted by an event that is unique in the context of the last 150 years. (This is admittedly less clear for the red record in the figure than for the green and blue.)

In regions of the ocean where oxygen is scarce (oxygen minimum zones (OMZs)) bacteria utilize nitrate as a substitute, taking up lighter N preferentially, leaving the water enriched in ^{15}N. The more extensive or intense the OMZ, the more enriched the water is in this heavier isotope. This water makes its way up closer to the surface where most of the biological activity is and most of the falling detritus that reaches the ocean floor is produced, so the sediments become enriched in ^{15}N as the OMZ's expand. It is the surface productivity and the utilization of oxygen as this falling detritus decomposes that generates the OMZs in the first place, so much of the variability in the OMZs is itself thought to be produced by variations in the productivity in the surface water. The productivity variations in turn are produced in large part by variations in the upwelling of nutrient rich waters. So more upwelling results in heavier N in the sediments. Finally, on these slow time scales you expect the variations in upwelling to be tied to large scale variations in the winds over the tropical and subtropical Pacific. If these variations in the wind stress are sufficiently coherent spatially maybe we can think of this proxy as a measure of the overall strength of the trades, with stronger trade winds resulting in more upwelling on the eastern boundary of the basin and increasing ^{15}N in the sediments. (The circulation and biology provide a natural low-pass filter, so you don't see much of an imprint of internanual ENSO-driven variability.)

I am far from an expert on the ways in which this argument can break down or how this proxy is related to others, such as coral records. But this picture of the evolution of the tropical Pacific winds is consistent with direct surface pressure measurements. On time scales of seasons and longer, surface pressure is a very smooth field in the tropics, and the pressure difference across the basin in low latitudes is strongly correlated with the strength of the equatorial winds. Deutsch et al also provide the following figure, based on HadSLP2 (Allan and Ansell 2006) and ERA40 (Uppala et al 2005) (yellow and cyan respectively), of the near equatorial difference in sea level pressure between the Indian Ocean/ western Pacific and the central/eastern Pacific. The black line is a 10 year running mean and an average over the two datasets where they overlap.

The 15N proxy and the direct estimates of the equatorial east-west surface pressure gradient give the same qualitative picture, supporting the idea that the wind field variations are coherent and large scale enough that we can think of the proxy as a trade wind index. Of course the proxy then opens up the possibility for extending the record farther back in time.

I was involved in a paper with GFDL colleagues (Vecchi et al 2006) in which we were looking at the slow downward pressure gradient trend as of 2005, before the extended strengthening trend in recent years became such a dominant part of the record. Zhang and Song 2006 independently focused on the same downward trend at the same time. We felt that we could attribute this trend to increasing greenhouse gas forcing, given that models simulate a trend of this sign and magnitude in their forced response. We also felt that this weakening could be related to the overall weakening of the convective mass exchange in the tropical atmosphere between the surface boundary layer and the deeper troposphere, a weakening that is expected to accompany warming (post 52). Understandably, the extension of this record since 2005 has led others to cast doubt on this attribution.

Given the emphasis on quasi-periodic variability in the Pacific, the ^{15}N figure from Deutsch et al caught my eye. But my comfort level in talking about proxies is pretty low (and as a result I have pestered more colleagues than usual while writing this post), and perhaps I am reading too much into it. How unique an event is the recent strengthening of the trades? The coincidence between the slow weakening trend that preceded it and the response expected from increasing greenhouse gases suggests that there may be something distinctive going on (post 45 expands on this possibility a bit). Or is this just a rather big instance of run-of-the-mill background variability?

64 Disequilibrium and the AMOC

[Originally posted November 21 2015]

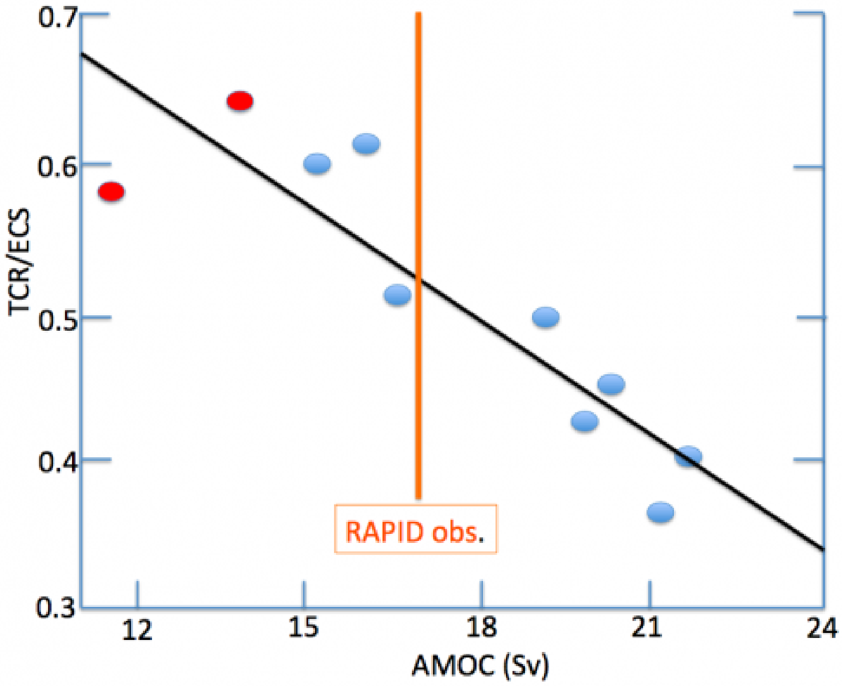


Figure 64.1: The strength of the Atlantic Meridional Overturning Circulation (AMOC) at 26N , in units of Sverdrups ($10^6 m^3/s$), plotted against the ratio of the Transient Climate Response to the Equilibrium Climate Sensitivity (TCR/ECS) in a set of coupled atmosphere-ocean climate models developed at GFDL over the past 15 years. Redrawn from Winton et al 2014. Also shown is the strength of the AMOC as observed by the RAPID array at 26N, from McCarthy et al 2015.

An important piece of information about the climate's response to CO_2 (and the other long-lived greenhouse gases) is the degree to which the forced response manages to equilibrate to the CO2 concentration as it increases.

Estimates of the degree of disequilibrium affect the attribution of the warming to date as well our ability to anticipate the longer term response to the anthropogenic CO_2 pulse. Most of the oceanic heat uptake that determines the degree of disequilibrium occurs either in the Southern Ocean or in the Atlantic Ocean where it is associated with an overturning circulation consisting of less dense waters moving northward near the surface throughout the Atlantic with sinking in the subpolar North Atlantic and a return flow of denser water at depth. I'm focusing here on the latter, the Atlantic Meridional Overturning Circulation, or AMOC. The case for the strength of the AMOC playing an important role in setting the rate of heat uptake by the oceans and the degree of disequilibrium in global mean surface temperature is made in particular by Winton et al 2014 and Kostov et al 2013, who describe two rather different perspectives on why you should expect a relationship between these two quantities.

The Atlantic overturning circulation is difficult to simulate in climate models. Dense waters forming in high northern and southern latitudes compete to fill the deep ocean. The coastal geometry and bathymetry of the sub-polar North Atlantic, where the sinking branch of the AMOC occurs, is complex and often not well-resolved in models used for multi-century simulation. Performing some "geoengineering" of the bathymetry to allow a reasonable pathway for the deep outflow from the Nordic Seas is not uncommon — the dense waters that form need to pass over sharp ridges and through narrow channels. Even if the bathymetry is sufficiently realistic, avoiding too much entrainment of less dense waters into the outflowing dense water (or too little entrainment) is a difficult challenge for ocean models (see Legg 2009). So we shouldn't be surprised if climate models produce a range of strengths for the AMOC This is illustrated in the figure above, which shows the strength of the Atlantic overturning as measured by the mass flux at 25N in a set of climate models that have been used for research on climate variability and change as well as seasonal-to-decadal prediction at GFDL in recent years. The observational constraint here is from the RAPID array as described by McCarthy et al 2015, whose best estimate for the mean over 2004-2012 is 17.2 Sverdrups. I haven't put an error bar on this figure (McCarthy et al 2015 quote a 0.9 Sv uncertainty for annual means) There is a nominal downward trend in the RAPID data over this time period, and the model results are obtained from pre-industrial control simulations so this is not exactly an apples-to-apples comparison.

Getting a realistic value for the strength of AMOC in a climate model, and it variability, is important for a variety of reasons. (Multi-decadal variability of the AMOC is not robustly simulated across climate models either.) As ocean models move to higher resolution, one would hope that the simulations of the strength of the AMOC and its variability would improve systematically. The red dots in the figure refer to two relatively new models in which the ocean resolution is considerably finer than in the other models. These models are producing relatively weak AMOCs. So this is an ongoing issue (stay tuned — there is a lot of related work underway).

The y-axis in the figure is the ratio TCR/ECS. The Transient Climate Response (TCR) is obtained by looking at the global mean warming at the surface that occurs at the time of doubling when CO2 is increased at 1%/year. The Equilibrium Climate Response (ECS) is the global mean warming after equilibration with a doubling of CO2. The latter is obtained by integrating from a while and then extrapolating carefully. (For those following these things, these are estimates of the models true equilibrium sensitivity, not what is sometimes referred to as the effective climate sensitivity, which is typically smaller. It is also worth keeping in mind that all of these models ignore the responses of the Greenland and Antarctic ice sheets to warming, which would enhance the ECS further if included.) As I have discussed in other posts a model's TCR is a good guide to how much warming it generates in response to the increase in greenhouse gases from pre-industrial times to the present — you simply need to multiply the TCR by the ratio of the radiative forcing due to all well-mixed greenhouse gases (WMGG) at present to that due to doubling of CO2 (about 0.8). You can then divide this by the ratio of TCR to ECS, to get an estimate of the equilibrium sensitivity consistent with this WMGG-attributed warming. If your estimate of TCR is 1.5K, the implication is that the warming due to the WMGGs up to the present is about 1.5 x 0.8 = 1.2K, (with the implication that aerosol forcing or something else has reduced this to the observed value). A ratio of TCR/ECS of 0.6, say, would give an ECS of 2.5K.

Why would stronger AMOC accompany a smaller ratio of TCR/ECS across models? The argument in Winton et al is less straightforward than you might guess at first. There has been a lot of focus on the possibility of substantial reduction in AMOC strength as the climate warms on decadal-to-century time scales. The Summary for Policymakers of the IPCC WG1/AR5 report assures us that "it is very unlikely that the AMOC will undergo an abrupt transition or collapse in the 21st century for the scenarios considered" (but is less dismissive of this possibility beyond 2100). However, nearly all models simulate a gradual weakening of the AMOC with warming. Interestingly the AMOC typically recovers eventually as the model climate equilibrates –some models even predict an eventual strengthening, depending in part at least on the competition between the densities in the subpolar North Atlantic and the waters in the Southern Ocean. People have looked at the initial weakening across models and found that the models that have stronger AMOC's in their control simulations typically have larger reductions in AMOC in 1%/year warming simulations — ie, there is somewhat less spread across the models when you look at the fractional reduction, not too surprisingly.

But AMOC warms the climate on average. You might think that a circulation transporting heat from the southern to northern hemisphere would warm the north and cool the south more or less equally, but because of the asymmetry of the land-ocean configuration, and feedback from northern ice and snow among other things, the northern warming is much larger, resulting in global mean warming with increasing AMOC. (For example, Knight et al 2005 find 0.05K global warming per Sv of AMOC, and 0.09K/Sv in the Northern Hemisphere, in the low frequency variability of their model.) So a decreasing AMOC retards global warming. (As long as the AMOC strength recovers, this retardation is temporary and does not affect the ECS substantially.) Models that have larger AMOCs to begin with simulate larger reduction in this strength with forced global warming, resulting in greater retardation of the warming, providing one perspective on the results in the figure.

A simpler perspective, described in Kostov et al 2013, is that stronger AMOCs are also deeper (think of denser waters formed at the surface in the subpolar N. Atlantic as sinking deeper as well as driving a stronger overturning). So there is a larger effective oceanic heat capacity involved in the heat uptake by this circulation, increasing the disequilibrium in global warming simulations. This argument does not depend on the change in AMOC in response to the warming but just relates to the control models AMOC structure, unlike Winton et al 2014. I encourage interested readers to read these papers and make up their own minds between these two perspectives. But independent of the specifics, it does seem that simulating a realistic AMOC is important for the degree of disequilibrium in model simulations of global warming.

(I had a number of helpful discussions with Mike Winton while writing this.)

65 Small Earth, Deep Atmosphere, and Hypohydrostatic Models

[Originally posted December 15 2015]

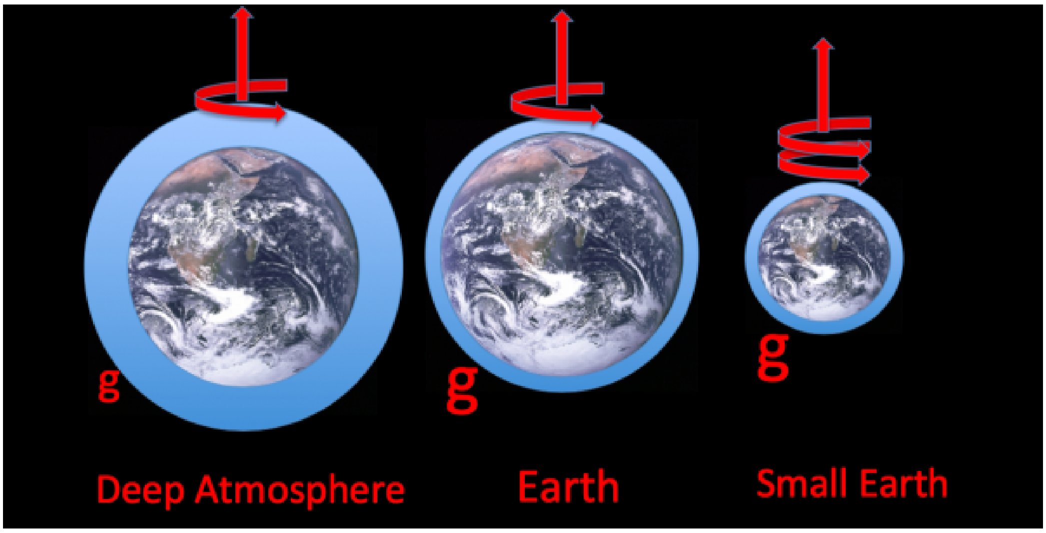


Figure 65.1:

A key problem in atmospheric modeling is the large separation in horizontal scales between the circulations that contain the bulk of the kinetic energy and dominate the horizontal transport of heat, momentum, and moisture, and the much smaller convective eddies that provide much of the vertical transport, especially in the tropics. We talk about the aspect ratio of a flow, the ratio of its characteristic vertical scale to its horizontal scale. The large-scale eddies dominating the horizontal transport have very small aspect ratios. These eddies (extratropical storms and even tropical cyclones) are effectively pancakes. In contrast, the small-scale convective motions have aspect ratios of order one. You have to get the horizontal grid size down well below the vertical scale of the atmosphere — the tropopause height or the scale height — to begin to resolve these small-scale motions. They are not resolved in current global climate models, a fact that colors all of climate modeling. We try to develop theories (closure schemes) for the vertical fluxes by unresolved eddies, but that's hard and success is limited. So groups around the world are developing global models with horizontal resolution of a few kilometers. (The animation in post 19 is produced by a model of this resolution but in a very small domain a few hundred kilometers on a side.) While these high resolution models don't resolve all of the vertical transports, global models with horizontal grid size of 1km or so will clearly help a lot. But we are still pretty far from being able to utilize global models with such high resolution as a flexible tool in climate research. They are too slow on available computing resources. So we look for shortcuts.

One important idea consists of embedding a high resolution model into each grid cell of the global model. The trick is to try to get away with far fewer grid points in each high resolution embedded model than would be needed to cover the whole grid at this resolution. Often the small scale models are 2D (x and z) rather than 3D (x, y, and z). The problem is how best to design these small high-res models and how to account for the two-way interactions with the large-scale model. This multi-grid approach is often referred to as superparameterization. It is still far more computationally intensive than a typical global climate model. This multi-scale approach has a lot of promise, but I would like to discuss another, more esoteric, idea here.

The problem is that the Earth is big. So why not just make it smaller? Reducing the planetary radius by a factor of 10, say, would decrease the number of grid points needed to resolve flow with aspect ratio =1 by 100. Rather than thinking about approaching the desired model by gradually decreasing grid size, maybe we could approach the desired model by gradually increasing the radius of the planet as computational resources allowed, with greater confidence in our simulations of vertical transports at each step along the way.

One obvious problem with this approach is that we don't have observations of such a planet to compare with our model! In any case the meteorology of such a planet would be very different from our own. Everything we know about atmospheric dynamics indicates that by reducing the radius by a factor of 10, changing nothing else, we would end up being dominated by an equator-to-pole Hadley circulation. It would be a totally different atmosphere. That doesn't mean that it is entirely without interest, but we can do better.

If we increase the rotation rate by the same factor α by which we decrease the radius we are much better off. One way to see this is to think about a ring of air starting at rest with respect to the surface at the equator, which then conserves its angular momentum as it moves polewards near the tropopause. If Ω is the rotation rate and a is the planetary radius, then it is not hard to show that when it reaches latitude θ the zonal velocity in the ring will be eastward with respect to the surface, with magnitude $\Omega a \sin^2(\theta)/\cos(\theta)$. If we keep the product Ωa fixed we end up with the same wind speeds at a given latitude. This flow becomes unstable through a process known as baroclnic instability that depends on this wind speed (actually, the difference between the wind speed near the tropopause and the much slower winds adjacent to the surface). Fixing Ω a means that we will have the right kind of instability. For those familiar with baroclinic instability theory, the length scale of the instability will be reduced by a factor of α due to the increase in rotation rate, thereby keeping the ratio of this instability scale to the planetary radius unchanged as well.

But now we have another problem. Even if the wind speeds and eddy scales are OK, the characteristic time scale of the dynamics, the ratio of the length scale of the eddies (or the planetary radius) to the wind speed, will be reduced. So the competition between the dynamics and the radiative fluxes and surface frictional effects that controls the circulation will be altered. But one can try to fix this also by artificially increasing the radiative fluxes and frictional stresses by the factor α. At this point, it feels like we are going down a rabbit hole with no end in sight, but this is the bottom of the hole. The result is referred to as the DARE approach (diabatic acceleration and rescaling) by Kuang et al 2005.

It is interesting to think about the fruit fly model of post 28 in this context. This model has the advantage that the radiation and surface friction take the form of linear damping with prescribed time scales. So in the DARE approach it is unambiguous how to decrease these time scales. You can then show that the rescaling of radius, rotation rate, and the radiative and frictional relaxation time scales is equivalent to increasing the aspect ratio of the atmosphere. The large-scale component of the flow remains more or less unaffected because its aspect ratio is so small that it is hardly affected by an increase in this aspect ratio (as long as you don't use too large a factor α) .

If you actually use the code that produced the figures in post 28 and do this rescaling you find that nothing changes at all! The resulting model is identical to the model that you start with. The reason for this is that the model is hydrostatic. If you have no intention of integrating your model with a grid fine enough to resolve aspect ratio one flows, you might as well drop all terms that are negligible when the aspect ratio is very small. When you do this systematically, you end up with a hydrostatic model in which the aspect ratio no longer appears in the equations. Nearly all climate models today are hydrostatic. Since the DARE rescaling amounts to increasing the aspect ratio, this has no effect on hydrostatic models. Some of us prefer the terminology hypohydrostatic to describe this modeling approach, since you are making the flow less hydrostatic when you increase the aspect ratio of the atmosphere.

Rather than starting out by thinking of making the Earth smaller, we can instead make the atmosphere deeper by reducing the acceleration of gravity, g, since the scale height of the atmosphere is inversely proportional to g. The small-scale convection then occurs on a larger scale since the horizontal scale of these convective cells is controlled by their vertical extent. In the fruit fly model, reducing g by the factor α, which we might call deep atmosphere rescaling, is exactly equivalent to and a lot simpler than the DARE rescaling. There is no need to change rotation rate or any damping time scales. In a hydrostatic fruit fly model, the solution is unaffected by changing g. (To avoid confusion, or maybe create it, in a full hydrostatic GCM changing g will affect the solution because this will create unwanted changes in the radiative and frictional damping times, but there are also ways of getting around this.)

Is it useful to approach the desired global nonhydrostatic high-resolution limit by this approach, gradually reducing α as computer power allows? The goal is to improve on the standard models with uncertain closure schemes for the unresolved convective scales. You can think of this as a kind of convective parameterization. You distort the convective eddies by making them bigger (think of the deep atmosphere perspective) and then just let the fluid dynamics take over. I think this question is still open, with some encouraging and some discouraging results to date. See here and here. If it improves climate models that will be great, but at a minimum I think it helps us to think more clearly about the hydrostatic approximation and which parameters control the shape of the atmospheric circulation.

(Conversations with Steve Garner on this topic have helped me a lot.)

66 Clouds are Hard

[Originally posted February 16]

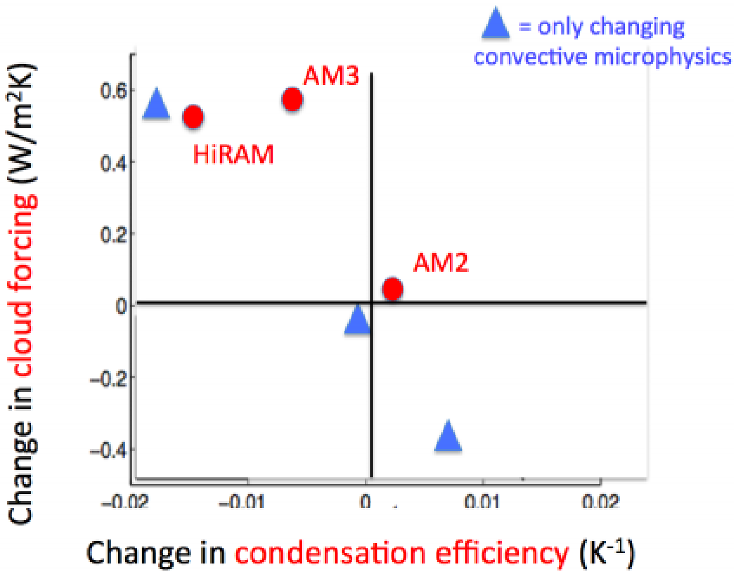


Figure 66.1: A measure of cloud feedback (vertical axis) plotted against a property of the sub-grid closure for moist convection (horizontal axis) for 3 comprehensive atmospheric models developed at GFDL (red dots) and 3 versions of a prototype of a new atmospheric model, the 3 versions differing only in the treatment of "microphysics" within the sub-grid convective closure (blue triangles). Redrawn from Zhao et al 2016.

The current generation of global atmospheric models in use for climate studies around the world do some things remarkably well, as I've tried to argue in several earlier posts. But it is well known that they struggle with a part of the system that is critical for climate sensitivity: simulating the Earth's cloud cover and how it responds to warming. They also struggle with simulating those aspects of the tropical climate that are sensitive to the moist convection that occurs on small horizontal scales — the effects of this small-scale convection need to be incorporated into climate models with uncertain "sub-grid closures". Unfortunately, the treatment of moist convection affects cloud feedbacks.

At GFDL we have built a variety of atmospheric models over the past 10-15 years, but they are almost all closely related to 3 distinct models AM2, AM3, and HiRAM. The 3 models differ especially in their sub-grid closures for moist convection. AM3 and HiRAM generate larger positive cloud feedbacks than AM2. We are in the process of trying to construct a new, hopefully improved, atmospheric model ("AM4") so we are naturally interested in understanding the key distinction between our earlier efforts that resulted in differing cloud feedbacks. This is not straightforward since cloud feedbacks are properties of the model that emerge from the interactions between a number of model components. Ming Zhao took the lead in this detective work, reporting in Zhao 2014 that the different cloud feedbacks among these 3 models were primarily related to effects on the short wave feedback of the assumptions concerning microphysics (the micron and smaller scale physics controlling precipitation processes) in the plume models underlying the sub-grid scheme for moist convection. It was mostly low and mid level clouds in the tropics that mattered as opposed to upper tropospheric cirrus.

When you change one part of a model, like the sub-grid convective microphysics, you often have to change another aspect of the model to retain a top-of-atmosphere (TOA) energy balance sufficiently realistic to justify coupling it to ocean and ice models and using it in climate change studies. The sensitivity of the resulting model is often a consequence of the change in model formulation needed to rebalance the model as well as the original modification motivating the change. A more recent paper (Zhao et al 2016) describes a study with AM4 in which we only change the model's sub-grid convective microphysics in ways that maintain balance in the TOA energy fluxes, requiring no changes in other aspects of the model. The figure shows the results from simulations from the atmosphere/land model, in which the sea surface temperatures are prescribed at observed values in a control simulation, then raised by 2K (holding CO_2 fixed) and then looking at the change in TOA radiative fluxes. The larger the increase in the net flux out of the system in response to the 2K surface warming, the stronger the radiative restoring force and the smaller the climate sensitivity. (This is sometimes referred to as the Cess senstiviity in honor of Bob Cess who proposed this setup to quantify radiative feedbacks in atmospheric models (Cess et al 1989).)

Here we isolate the effects of clouds on the Cess sensitivity by computing the change in cloud forcing, which is now often referred to as the cloud radiative effect. As the model is running, we compute the radiative fluxes twice, both with and without clouds, using only the calculation with clouds to interact with the rest of the model. The difference between the TOA fluxes with and without clouds is the cloud forcing — clouds have a net cooling effect on the climate so the cloud forcing is negative. We then repeat this calculation with the +2K perturbation and decompose the change in the net TOA flux into a clear sky part and a part due to the change in the cloud forcing. A positive change in cloud forcing (less negative cloud forcing in the warmer climate) is a warming effect and increases climate sensitivity.

(This change in cloud forcing is not quantitatively a good approximation to the cloud feedback in that it does not vanish when "clouds are unchanged" (e.g Soden et al 2004), but the difference is mostly a constant offset so the two are closely correlated. This can be confusing so might be worth returning to some other time.)

The sub-grid convective closure in these models is a source of cloud condensate on the grid-scale. The horizontal axis in the figure is a non-dimensional measure of the efficiency of this process: the amount of condensate produced per unit precipitation. Cloud can also be directly produced in these models on the grid scale — for example, in extratropical storms. Here we are only looking at the cloud produced by the sub-grid closure. After combining these changes in cloud forcing with the clear sky TOA values, the ratio in the (Cess) sensitivity between the low end and high end models here turns out to be about 1.7– a bit more than half of the factor of 3 uncertainty in sensitivity often quoted. These changes in convective condensation efficiency are small, ranging from – 2% to +1% per degree warming, illustrating the power of clouds to change the energy balance and suggestive of the difficulty in constraining this metric with observations..

Why do we need sub-grid convective closures in our climate models? The atmosphere in the tropics is typically conditionally unstable — parcels of air are stable to ascent if unsaturated but are often unstable if lifted beyond the point at which they become saturated. Within a 100km2 grid box, say, there is a lot of sub-grid turmoil that can create unstable parcel ascent in some fraction of the box. If you do not parameterize the effects of this sub-grid creation of buoyant plumes, you have to wait for the grid box to become saturated and then contend with the often violent, unrealistic convection that would occur in the model on the grid scale. For some time I have felt that we should take models with no sub-grid convective closure more seriously; this simplifies the model a lot and it is interesting to isolate what the sub-grid convection scheme is doing to the simulations. There is more effort in this direction recently (e.g. Webb et al 2015) But there is no claim that at typical GCM resolutions we can avoid sub-grid convective parameterization without degrading the quality of the simulation. It seems that we are stuck with this layer of complexity until we adequately resolve the convection itself, or find some trick like superparameterization to get us to these global "cloud resolving" models more quickly (see post 65).

These closure schemes are often based on a picture of plumes in which the upward motion is assumed to be concentrated and that entrain environmental air, precipitate out some water, and detrain vapor and condensate to the grid-scale as they ascend. Assumptions concerning the turbulence controlling the entrainment and detrainment get a lot of attention — you typically find that many aspects of the tropical simulations, not just cloud feedbacks but many aspects of tropical meteorology, are sensitive to these assumptions about the cloud macrophysics. Before beginning this study, based on the previous literature I suspected that these entrainment assumptions were the source of the differences between our models, but that was not the case — it was the microphysical assumptions instead. You need a microphysical picture to decide how much you precipitate and how much condensate (cloud) remains suspended in the atmosphere. The effects on tropical meteorology of these convective microphysical assumptions are more subtle than those concerning cloud macrophysics. but they still affect cloud feedbacks.

In one class of models, the microphysical picture within the convective sub-grid closure effectively provides a threshold condensate density above which precipitation becomes very efficient. As the climate warms, for a given upward mass flux in a plume there is more water vapor and more condensation but a limit to how much condensate you can suspend and carry around. The result is less cloud condensate produced per unit precipitation. HiRAM and AM3 have this threshold flavor, and the simulation marked by the triangle near the top of the figure is configured to behave in this way as well. Alternatively, the microphysical picture might be such as to make the condensate production more or less proportional to the precipitation as temperatures increase, keeping convective precipitation efficiency about the same. AM2 behaves like this as do the models that produce the two lower blue triangles (these differ in the parameter setting within this scheme).

Can we choose between the different blue triangles based on observations? Direct cloud scale observations of these small changes in the condensation efficiency with temperature are difficult. An alternative is to search for indirect constraints based on emergent properties of the simulation (e.g. Klein and Hall 2015). The problem is that, while it may be possible to find some properties of the climate simulation that look better in one of these models than the others, the biases in other parts of the model affecting the same metric can make it hard to make a convincing case that you have constrained cloud feedback. At this point, we are not convinced that we have emergent constraints that clearly favor one version of this proto-AM4 model over the others. We are uncomfortable having the freedom to engineer climate sensitivity to this degree. You can always try to use the magnitude of the warming over the past century itself to constrain cloud feedback, but this gets convolved with estimates of aerosol forcing and internal variability. Ideally we would like to constrain cloud feedbacks in other ways so as to bring these other constraints to bear on the attribution of the observed warming.

67 More on Tropical Cyclones and the ITCZ in Aquaplanet Models

[Originally posted March 12 2016]

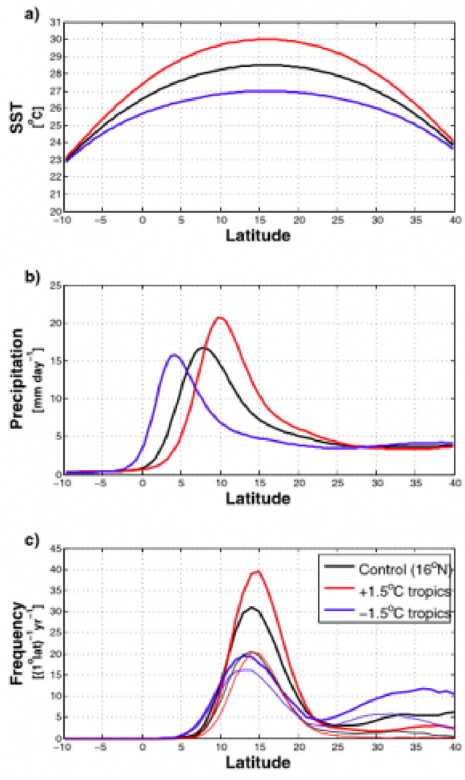


Figure 67.1: Tropical cyclone statistics in the global aquaplanet model of Ballinger et al 2015 varying the strength of the surface temperature gradient in the tropics. a) sea surface temperatures; b) precipitation; c) heavy lines: frequency of formation of tropical cyclones; light lines: frequency of formation of tropical cyclones that reach hurricane strength.

There are a lot of open questions regarding how the frequency of tropical cyclones (TCs) is controlled. I'm excited about the fact that our global models are getting progressively better at simulating TC statistics, as I tried to show in post 2. As the quality of global simulations improves it opens up the possibility of manipulating these models in different ways to isolate the factors that alter TC statistics. One approach is to just take a model with realistic boundary conditions (seasonal cycle, continents, orography) and simplify these boundary conditions. A standard simplification is an aquaplanet model in which the surface is uniform and "ocean" covered", ie water-saturated, typically with no seasonal cycle. With this idealization the model climate, all statistics of the flow — temperatures, precipitation, clouds, radiative fluxes — are functions of latitude (and height) only, and not longitude or time of year, making for a much simpler system to analyze. These aquaplanet simulations are sometimes run with prescribed sea surface temperatures (SSTs) and sometimes with prescribed heat flux through the surface (usually realized by running the atmosphere over a "slab ocean" s saturated surface with some heat capacity, and specifying an "oceanic heat flux" into or out of the slab. With prescribed SSTs, the net energy flux through the surface is part of the solution; in the slab model, the SSTs are part of the solution.

I've discussed one aquaplanet study of TCs using a slab ocean, Merlis et al 2013, in post 42. We have another paper, Ballinger et al, 2015 using prescribed SSTs. I'll focus here on the particular result shown above from Ballinger et al. Starting with an SST distribution that is a a function of latitude only and warmer in the Northern than the Southern Hemisphere, we then flatten the SSTs in the tropics as shown in the top panel, keeping the latitude of the maximum SST unmodified (16N in this case). The SSTs outside of the latitudes shown in the figure are also unmodified (this is a global model). Does the number of TCs increase or decrease as the tropical SSTs are flattened?

This particular sensitivity study is of interest in part because some paeloclimates are thought to have had weaker tropical SST gradients. In this paleo context, there have been some arguments that these weak tropical SST gradient climates might have had more tropical storms. Another motivation for looking at this case is the way in which the latitude of the peak tropical rainfall (middle panel) changes when we flatten or sharpen the SSTs. (This maximum is referred to as the ITCZ — the intertropical convergence zone –the north-south flow of the low level air, carrying a lot of water, is converging most strongly at this latitude.) The ITCZ moves northward (towards the maximum in the SSTs) as the SST maximum is sharpened. There is quite a bit of recent literature on the topic of how the

ITCZ position is controlled, a lot of it using aquaplanet models (see post 37) This is obviously an important topic, but I won't try to discuss it here, except to re-emphasize the point that, in this model at least, the latitude of the SST maximum is not in itself determining the ITCZ position.

Quantifying the number of TCs that form in these simulations depends to some extent on the criteria used to identify TCs. The criteria used here are fairly standard: a localized low level vorticity maximum above a threshold, a "warm core", and near-surface winds of a a given strength lasting for a least a couple of days. Identifying coherent structures of a particular type in fluid flows in not entirely straightforward, but we are pretty confident that the qualitative results don't depend on the details of these criteria. The thicker lines in the lower panel show the frequency of TC formation as a function of the latitude at which they first satisfy the wind speed criterion. No storms form in the cooler Southern Hemisphere. The latitude at which most of the TCs emerge hardly moves at all as the SSTs are changed in these runs.

The frequency of TC formation decreases as the SSTs are flattened. (There is a secondary mode of formation in the subtropics around 30N; these are vortices that spin off from extratropical frontal circulations penetrating into the subtropics –we did not focus on these in this paper.) The thin lines (a bit hard to see) are the frequency of TC formation counting only those storms that reach hurricane strength at some point in their lifetime. The number of these strong storms does not change much. So the average strength of the TCs increases as the SSTs are flattened. In the flattest case, nearly all TCs reach hurricane strength. As discussed in the paper, these changes are dominated by changes in the SST gradients, not the changes in the tropical temperatures themselves. If you increase the SSTs uniformly the frequency of TC formation decreases in this model, the opposite of the result when you increase tropical SSTs in the manner shown in the figure.

The key to the changes in the number of TCs seems to be closely related to the latitude of the ITCZ, just as in Merlis et al. The following plot from Ballinger et al shows this relationship for the simulations discussed above and also for simulations in which the latitude of the maximum SST and the global mean temperature are changed. The figure also shows the slab ocean results of Merlis et al (black squares). It seems that to understand why the flattening of the SSTs decreases TC numbers in this model we need to understand why the ITCZ moves to lower latitudes. IOt is tempting to think of TCs in this setup as emerging from an instability of the ITCZ, an instability that cannot generate vortices easily when the ITCZ is too close to the equator due to the weakness of the horizontal component of the Coriolis force. The challenge is to translate this picture into a more

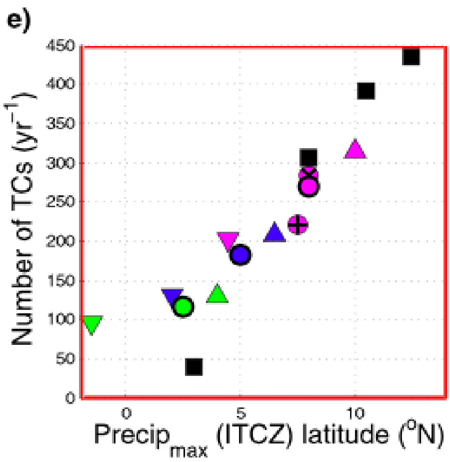


Figure 67.2:

quantitative theory for the frequency of TC formation. It is interesting that the TC genesis region does not move with the ITCZ; we suspect that this is because the disturbances generated by the instability are just too weak to show up in this metric until they enter the a more favorable region for vortex spin-up. The reason for the increase in average storm intensity with the flattening of SSTs is more obscure but seems related to the fact that storms last longer when the tropical SSTs are relatively flat and so have more time to develop to their mature intensities before migrating to higher latitudes.

Often a more idealized setup such as this provides a cleaner way of understanding differences between models. It will be interesting to see how robust these kinds of results are across models and how they change as the resolution of the models is improved. These simulations use a 50km grid, which is intuitively far from adequate for tropical cyclone simulations, a point that I am sure would be seconded by most tropical cyclone specialists. (But I place a lot of weight on the result that he version of this 50km model with realistic boundary conditions, counterintuitively perhaps, produces an impressive simulation of TC frequency statistics, as discussed in post 2.)

How can this kind of idealized model ever be compared to observations? Among other things, the ITCZ position presumably plays a more important role here than in reality where there are other paths to cyclogenesis not closely tied to a well-defined ITCZ. I think the key is to think of this idealized version as part of a hierarchy that includes the version with realistic boundary conditions. You confront the realistically configured model with observations and use the idealized model to help understand this more comprehensive model and, hopefully, the atmosphere as well, and to cleanly expose the source of differences between attempts at realistic simulation.

68 Superrotation, Idealized Models, and GCMs

[Originally posted April 13 2006]

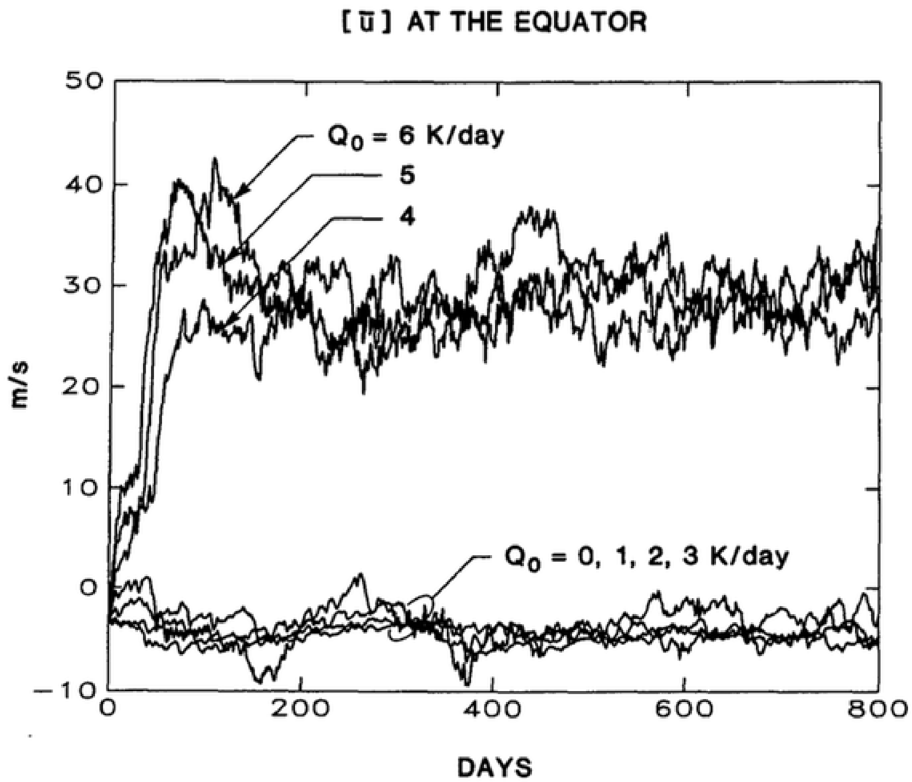


Figure 68.1: The average around the equator of the eastward wind in the upper tropospheric layer of the idealized atmospheric model of Suarez and Duffy 1992, for several different values of the strength of an imposed tropical heat source.

GCMs often play a conservative role as a counterpoint to speculation or idealized modeling regarding "tipping points" or abrupt climate change, in favor of gradual, more linear climate response to external forcing. Think of abrupt collapse of the AMOC (Atlantic Overturning Circulation) or the "death spiral" of Arctic sea ice. These high-end models may not always be correct of course, but I think they provide an appropriate null hypothesis that must be critically examined in the light of observational constraints, possible missing physics, etc. I'll use the problem of equatorial superrotation to illustrate this point — also taking this opportunity to introduce this relatively obscure subject.

The figure is from a paper by Suarez and Duffy 1992 using an idealized two-level global model of the atmosphere. Two-level implies that the horizontal winds are defined at only two vertical levels, which we can think of as the lower and the upper troposphere. The interaction between an upper tropospheric and a lower tropospheric flow is the classic starting point for thinking about the fluid dynamics that produces midlatitude storms. The tropical Hadley circulation also seems, at least naively, to be representable to a first approximation with two levels. The first non-trivial climate models — models that simulate weather and that can be integrated over long enough periods to obtain the statistics of that weather — were two-level models, initially confined to mid-latitudes (Phillips 1956) and then moving to a global geometry (Smagorinsky 1963). It is tempting to continue to use two-level models to try to encapsulate our understanding of aspects of atmospheric climate, despite the fact that GCMs long ago moved to much finer vertical resolution. Suarez and Duffy use a dry version of such a model, starting with a control climate that is independent of longitude and then adding a longitude-dependent heat source in the tropics. As the magnitude of this heat source is increased, the model undergoes an abrupt transition to a very different climate. Is the atmosphere capable of making this kind of profound abrupt transition?

In the control climate of this model, the east-wind winds at the equator in the upper troposphere are (realistically) close to zero after averaging around the equator; in the new climate generated when the tropical heating is strong enough, the eastward winds at the equator increase dramatically. This alternative climate is referred as superrotating. Eastward midlatitude jets can be generated by simply taking a ring of air that is stationary with respect to the surface at the equator and moving it polewards, towards the axis of rotation, while conserving its angular momentum, but you can't generate eastward winds at the equator that way. The interesting case is equatorial superrotation — equatorial winds in the direction of and faster than the rotation of the surface at the equator. Equatorial superrotation (which I'll abbreviate as just "superrotation"), is seen in many planetary atmospheres. Is the abruptness of this transition to a superrotating state realistic?

And does this issue have any conceivable connection to global warming? The simplest way to make a case for potential relevance is to point to simulations of the very warm Eocene (especially that of Cabellero and Huber 2010). These do produce strong equatorial superrotation, and they show some acceleration towards larger superrotation with increasing warming, albeit at much warmer temperatures than are relevant for the next century — but the transition is not nearly as abrupt as in the figure above. Most importantly, there is only a barely significant hint of an increase in westerlies near the tropical tropopause in GCM projections for the next century (Figure 12.19. in Ch. 12 of the AR5/WG1 report.).

I discuss this issue in this lecture from 1999, where I talk about some of the underlying mechanisms that favor superrotation and that might create an abrupt transition. One important ingredient is the excitation of Rossby waves (see post 57) that propagate out of the tropics into midlatitudes. If the tropical Rossby wave source increases — this is what increasing the heat source in Suarez and Duffy does — there should be a tendency towards superrotation. If the tropical wave source increases with warming, then warming should produce some superrortation (holding everything else fixed). Abruptness then seems to depend in large part on the fate of Rossby waves excited in midlatitudes that propagate into the tropics. But these arguments are sufficiently qualitative that they could be applied to full GCMs as well as the two-level model.

The Suarez-Duffy paper did not attract much attention. I feel a bit guilty for not having done more on this problem in recent years. I am not even sure how robust the two-level result is to the details of the model formulation. But I suspect that we might learn something significant about the atmosphere by understanding better the susceptibility to this abrupt transition in idealized atmospheric models, including this dry two-level model. And this understanding might in turn be relevant to how we judge the credibility of this aspect of our high-end simulations.

I would put a very low number on the probability of a big surprise in the response of equatorial winds to warming in the coming century. But it would not be infinitesimal. This superrotation problem still troubles me because there are aspects of GCMs that are uncertain that might be relevant to this issue. For example, the vertical redistribution of horizontal momentum by small-scale moist convection in the tropics could affect the tendency towards superrotation — and this momentum exchange could be sensitive to the spatial organization of convection (into squall lines etc) that is not well resolved in current models. In addition, Cabellero and Huber and others point to Madden-Julian Oscillation (MJO)-like phenomena as playing a role in the transition to superrotation, yet the MJO, an intensely studied mode of variability in the tropics on a 30-50 day time scale, is not simulated very robustly in current GCMs. But I would need much stronger arguments than anything I currently have to override (the thankfully rather boring) GCM simulations and push this issue as important for anthropogenic climate change.

In this case and in some others like it on potential tipping points, such as AMOC collapse, GCMs do seem to provide a conservative null hypothesis. Even though I may have an argument or idealized model suggesting that some abrupt climate response might be possible, if our GCMs — by which I mean the best simulators of the climate that the world can come up with — do not show this behavior then I cannot just decide to prefer my theory to the GCM simulations. I need a compelling argument that the GCMs do not include some key ingredient or are inconsistent with observations in a way that is relevant to the response in question.

69 Modest Proposal Regarding TCR

[Originally posted May 23 2016]

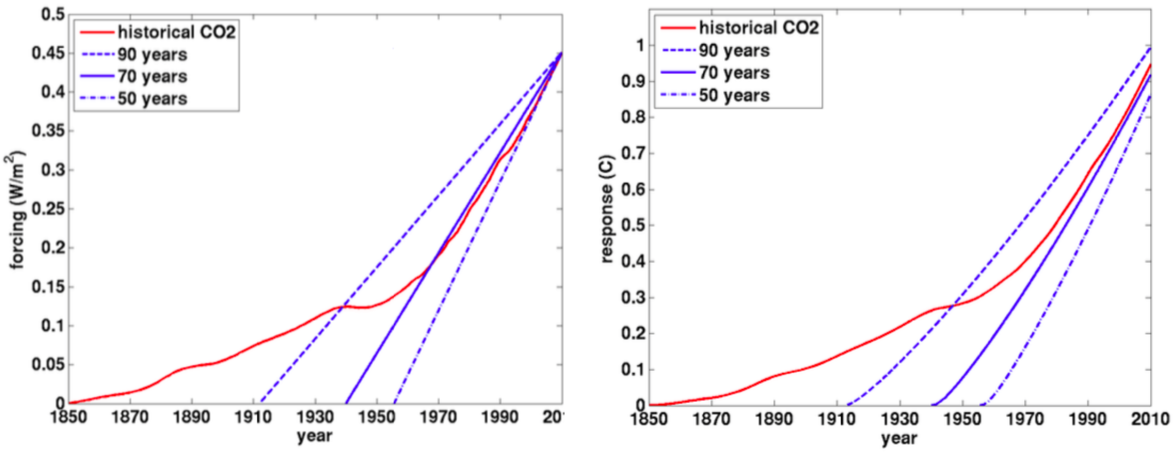


Figure 69.1: The radative forcing (left) and global mean temperature response (right) using a simple GCM emulator, for the historical CO2 forcing (red) and for the linearly increasing forcing consistent with the simulations used to define the transient climate response (blue), for 3 different ramp-up time scales, the 70 year time scale (solid blue) corresponding to the standard definition.

The terminology surrounding climate sensitivity can be confusing. People talk about equilibrium sensitivity, Charney sensitivity, Earth system sensitivity, effective sensitivity, transient climate response (TCR), etc, making it a challenge to communicate with the public, and sometimes even with ourselves, on this important issue. I am going to focus on the TCR here (yet again). The TCR of a model is determined by what appears to be a rather arbitrary calculation: starting with the climate in equilibrium, increase CO_2 at 1% per year until doubling (about 70 years). The global mean warming of the near surface air temperature at the time of doubling is the TCR. In a realistic model with internal variability, you need to do this multiple times and then average to knock down the noise so as to isolate the forced response if you are trying to be precise. If limited to one or two realizations, you average over years 60-80 or use some kind of low-pass filter to help isolate the forced response. Sometimes the TCR is explicitly defined as the warming averaged over years 60-80. Although I have written several posts emphasizing the importance of the TCR in this series, I would like to argue for a de-emphasis of the TCR in favor of another quantity (admittedly very closely related – hence the modesty of this proposal.)

If you talk to someone about the TCR you have to explain why this idealized 1%/yr scenario is of interest. From my perspective, the importance of the TCR stems from its close relationship to the warming from the mid-19th century to the present that can be attributed to the CO2 increase. There is a growing literature on estimating TCR from observations, using the instrumental temperature record over this time frame. But these studies are not direct estimates of TCR; they are estimates of the warming attributable to the CO2 increase which are then converted to TCR by assuming that the warming is proportional to the CO_2 radiative forcing. If forcing due to a doubling of CO_2 is $\mathcal{F}_{2X}$ and the forcing due to the observed increase in CO_2 over the period $T \equiv (T1 : T2)$ is $\mathcal{F}(T)$ then

$$TCR = WACO_2(T)/\xi$$

Here I have defined $WACO_2(T)$ as the global mean Warming Attributable to CO_2 over the time interval T and I have set

$$\xi = \mathcal{F}(T)/\mathcal{F}_{2X}.$$

For the rest of this Essay, I'll just assume that $T = (1850, 2010)$. For this period, ξ is about 0.45.

The past warming attributable to CO_2 is itself important as a constraint on models used to project this warming into the future. Whether your model is a simple extrapolation or an energy balance model or a full GCM that simulates climate by simulating weather, you obviously want the model you are using to be consistent with the past warming.

Estimating $WACO_2(T)$ from observations over the past century or so is far from straightforward, due primarily to the uncertainty in the cooling due to anthropogenic aerosols, but also due to the presence of other forcing agents, including other well mixed greenhouse gases, as well as internal variability, But what's the point of converting someone's estimate of the range of values of $WACO_2$ consistent with observations into the corresponding range of TCR values? The point is simply that the latter has become a standard for the comparison of GCM responses, so the range of TCR estimates from models is readily available. But this does not seem like a very good reason to try to communicate the importance of the TCR value rather than the more obviously relevant $WACO_2(T)$.

How good is the proportionality assumption $TCR = \xi WACO_2(T)$? And if it is good, why? For concreteness I'll use a very simple three time-scale fit to the response of a particular GCM to an instantaneous doubling of CO2. The model is GFDL's CM3 and the fit is described in Winton et al 2013. The response takes the form

$$T(t) = \sum_{i=1}^{3} \alpha_i [1 - \exp(-t/\tau_i)] \tag{69.1}$$

with $[\alpha_1, \alpha_2, \alpha_3] = [1.5, 1.3, 1.8]K$ and $[\tau_1, \tau_2, \tau_3] = [3, 60, 1000]$ years. I have rounded off the time scales a bit. Since this model is linear you can scale this response to that for an infinitesimal increase and then add up the responses to the forcing over time for any CO_2(t). (See the discussion of the response to volcanic forcing in Essay 50,)

I carried it along for these calculations,, but the very long millennial time scale present in the GCM has negligible effect on WACO2 or TCR , so this is really a two-time scale model for our purposes. And you may have noticed that this is a a rather sensitive model. But keep in mind that it is linear, so if you multiply all of the α's by the same factor you change the amplitude of all responses, including WACO2 and TCR, by this same factor.

[In the calculations to follow, I'm assuming that the radiative forcing due to CO2 is exactly logarithmic in CO_2 concentration, so 1% increase/yr is a linear increase in radiative forcing.]

The red line in the figure on the left above shows the CO2 radiative forcing from 1850 to 2010 from GISS. The solid blue line shows the linear increase in forcing over 70 years that ends up at the same value of forcing at 2010 as the red line. This is the forcing due to a 1%/year increase multiplied by ξ — or, equivalently, it is the forcing due to a $\xi\%/yr$ increase for 70 years. Also shown with the blue dashed lines are the linear forcing trajectories that reach the same point in 2010 but increasing the 70 year interval to 90 years or decreasing it to 50. The 70 year linear increase at $\xi\%/yr$ is evidently a pretty good fit after 1960. It's not relevant whether a 1%/yr increase is larger than the increase in CO2 forcing since we are assuming linearity and normalizing the TCR anyway. The key is that a linear fit to the recent period of rapid increase in CO_2 forcing requires roughly 70 years starting from zero.

The figure on the right shows the responses of the three-time scale model to these forcing trajectories. The standard (70yr) TCR after normalization underestimates the $WACO_2$ (the red curve) by about 3%, which is basically negligible given the the uncertainties in TCR that we are concerned about. My eyeball estimate of the error, given the forcing that is missed by this linear approximation before 1950, keeping in mind the 60 year intermediate e-folding time in this model, would have been a bit more than this, so I have checked this result a couple of times — which does not guarantee that I did not make a mistake, of course, (It seems that the error made by missing the response to the increases in CO_2 in the first half of the 20th century is canceled in part by the fact that the linear fit in the more recent period is not perfect.) Even if you make the sub-optimal choices of 50 or 90 years for the ramp-up, the errors are only of the order of 10%.

So the approximation $WACO_2 = \xi TCR$ looks good, at least for this particular response function. If you want to modify the model to create a larger difference, you will have to decrease the relative importance of the fast response that occurs on time scales shorter than the time scales of the CO2 evolution itself and put more weight on the longer time scales. Using discrete response times is not the only way of emulating a GCM's response function. Diffusive models have a long history in this regard. But as long as the fast response is as large a part of the response to centennial-scale forcing as it is in GCMs (see Geoffroy et al 2013) you won't get very much of a discrepancy.

We could de-emphasize the 1% year simulation in favor of just simulating the response to the historical CO2 increase. This simulation is performed routinely by some groups, but for the CMIP projects, including the upcoming CMIP6, it is the response to the historical evolution of all of the well-mixed greenhouse gases (WMGGs) that is typically requested, without breaking out the CO2 contribution. This raises another issue — the validity of assuming that you can get the response to CO2 from the response to the full set of WMGGs by simply normalizing by the ratio of the radiative forcings. Given questions about how best to define radiative forcing (a good topic for another post), this adds an unnecessary layer if you is are primarily interested in a model's response to CO_2.

Rather than focusing on TCR itself, especially when discussing this topic outside of scientific circles, we should think of it as just a standard way of estimating WACO2 for a model, a technique that could be improved if desired. Perhaps what we need is a good acronym for the warming attributable to CO_2. $WACO_2$ seems less than ideal.

70 Spherical Rotating Radiative-Convective Equilibrium

[Originally posted June 3 2016]

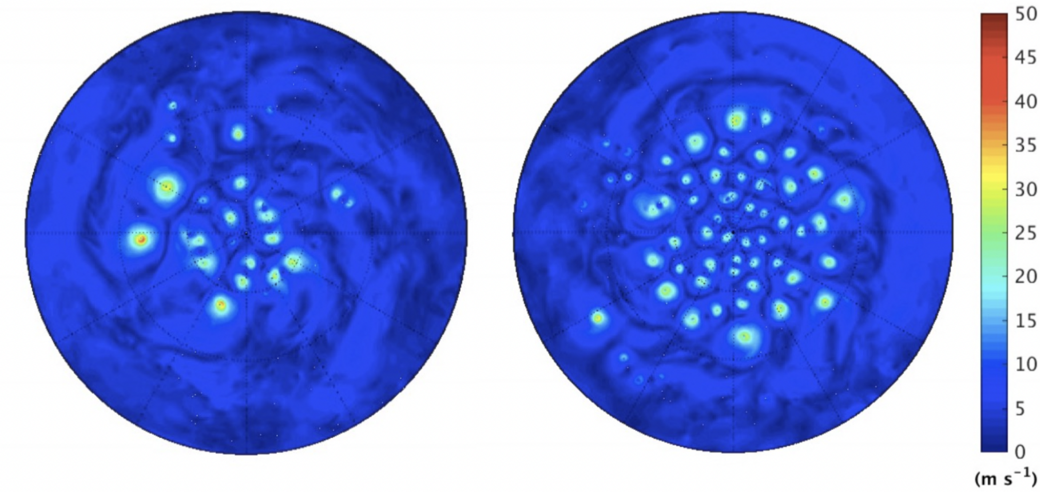


Figure 70.1: Snapshots of near surface wind speed in simulations of spherical rotating radiative convective equilibrium, as described in Merlis et al 2016. A full hemisphere is shown, the outer boundary being the equator. The surface temperature is prescribed at 307K in the left panel and 297K in the right panel

Continuing a recurrent theme in these posts, I'll describe yet another idealized framework for thinking about the climatology of tropical cyclone (TC) formation. In this setup, we assume a rotating sphere with a homogeneous surface — water-saturated; no continents; surface temperatures prescribed to be uniform over the entire sphere. There are no seasonal or diurnal cycles in the insolation (and we prescribe a spatially uniform ozone distribution as well). So the only source of spatial inhomogeneity is the rotating sphere. For our thin rapidly rotating atmospheric spherical shell, the key inhomogeneity is the latitude dependence of the strength of the horizontal component of the Coriolis force, the "Coriolis parameter", $f \equiv 2\Omega \sin(\theta)$ where θ is the latitude. The amplitude of f increases from the equator, where it vanishes, to the poles. This latitudinal gradient in f is critical for TC evolution. In the Northern Hemisphere, the increase of f with latitude causes cyclonic vortices to drift north and west with respect to the larger scale flow in which they're embedded, a process known as β-drift. (β is the conventional symbol used to represent the northward gradient of f.)

Being spatially inhomogeneous this model is potentially a lot more complicated than the homogeneous model described in Post 43 that uses a a doubly-periodic "f-plane" geometry, ie with a constant strength of the Coriolis force f. The intriguing result in the latter setup, but also the problem with that setup, is that the domain fills up with tropical cyclones. These TCs are so long-lived and interact so weakly that there is rarely room for another storm to form. The f-plane framework is fascinating to me but it seems more suited to studying mature storm structure rather than new storm formation (genesis). On the sphere β-drift carries these storms polewards, clearing out the tropical regions where the storms develop and making plenty of room for new storms.

Compared to the more standard aquaplanet simulations described in essays 42 and 67, this setup has the simplification of surface temperatures that are uniform in latitude as well as longitude. When you prescribe more realistic surface temperatures with an equator-to-pole gradient, you get large east-west winds and instabilities resulting in midlatitude storms. These aquaplanet frameworks may be ideal for studying the interactions between TCs and extratropical storms, but it is also nice to get rid of these storms altogether to see how the TC's behave on their own, which is just what the uniform sea surface temperatures do.

There was some earlier work with more or less this same setup, but focusing on the mean precipitation rather than TC statistics — such as Kirtman and Schneider 2000, who describe a model in which the tropical precipitation still gathers itself into a sharp intertropical convergence zone at the equator despite the absence of any spatial inhomogeneity in the lower boundary condition or the incident solar flux. The theory for this kind of organization of the mean precipitation is itself of interest, but this earlier work did not discuss TC simulation. Tim Merlis, Wenyu Zhou, Ming Zhao and I were discussing this framework — it has the nice feature that anyone with a GCM can configure it easily — but I was personally rather discouraged by the complexity coming from the inhomgeneity of both the mean climate and the TCs. However, more recently Shi and Bretherton 2014 returned to this setup but focusing on TCs, with a lot of interesting results that caught our interest. So we tried our hand at this framework in Merlis et al 2016. We used somewhat higher horizontal resolution (about 50 km) and focused in particular on the response to increasing surface temperatures.

Most importantly from my perspective is that this model and the homogeneous f-plane model of Zhou et al 2014 and Essay 43; the aquaplanet model with SSTs a function of latitude of Ballinger et al 2015 and Essay 67, the "slab-ocean" aqua-planet of Merlis et al 2013 and Essay 42, and the atmospheric model with realistic boundary conditions of Zhao et al 2009 and Essay 2 **all use exactly the same atmospheric model**. They differ only in their boundary conditions, at the surface and incident solar flux at the top of the model, and in the domain geometry. The model with realistic boundary conditions does a pretty good job on the global number of TCs, their seasonal cycle, the response of TCs to ENSO, etc. Having this realistic point of contact is important to help justify the idealized configurations — ie, if they help us understand the realistically configured model they may help us understand nature. I am struck by the variety in these simulations, which I think gives us a lot to think about when trying to understand the TC climate more fundamentally.

The figure at the top shows snapshots (of the near-surface wind speed for simulations with two different surface temperatures, 297K and 307K. The basic picture is of TC genesis in low latitudes , after which the storms migrate poleward and congregate into a swarm of cyclonic vortices around the pole. The situation in the polar cap resembles the f-plane simulation of Essay 43. This evolution has gone on for some time before the month shown in the animation, so we think the distribution of vortices has equilibrated. New storms entering the polar cap crowd are compensated by some merger or destruction of vortices already congregated there, so this cap of vortices does not seem to be growing, which is nice since that leaves the generation region more realistically free of crowding by pre-existing vortices.

In the colder case, there are obviously a lot more vortices with less space between them, and the crowd of vortices surrounding the pole has grown — to the extent that any more growth could interfere with the generation process in low latitudes. It is not only the number of vortices but the generation rate is also much higher in the case with colder temperature, by roughly a factor of 4. Admittedly, this is a big change in temperature but even when you look at the reduction in genesis frequency per degree warming it is quite a bit larger than the reduction that you see in this model when configured with realistic boundary conditions. (You may recall that the aqua-planet slab ocean model described in Essay 42 actually has the frequency of TC formation increasing with increasing surface temperature.)

I think this may be a good setup for looking at how different global model formulations affect TC genesis frequency — and the temperature dependence of this frequency — as long as the β–drift is successful at cleaning out the development region. The expansion of the polar cloud of cyclones with decreasing temperature is itself interesting but a problematic distraction if you want to focus on genesis in a realistic setting of relatively little interference from pre-existing TCs. One could add damping (increased surface friction) to the polar regions to try to destroy these storms and prevent their accumulation and avoid this interference.

71 Forcing, Feedback, and Clouds

[originally posted September 11 2016]

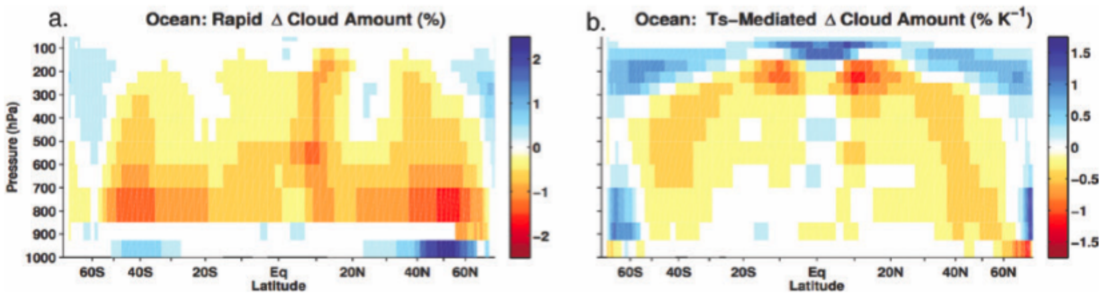


Figure 71.1: Percent change in zonally-averaged cloud cover over the oceans as a function of latitude and height in response to an instantaneous quadrupling of CO_2, decomposed into two parts: (a) a fast adjustment that occurs before surface temperatures have warmed appreciably, and (b) a part that scales linearly with the warming of surface temperature as the system adjusts to the increase in CO_2. From Zelinka et al 2013 (as reproduced in Sherwood et al 2015).

The forcing-feedback language used in discussing climate change is familiar but is evolving in interesting directions. I discussed some of the arbitrariness in the decomposition of the feedback into components in Essays 24 and 25. (I'm still serious about discarding the traditional concept of "water vapor feedback" by the way). Here I'll focus instead on the distinction between forcing and feedback.

In the simplest picture, "forcing" is the result of a radiative transfer calculation that does not depend on the climate response — hold everything else fixed (temperature, water vapor, clouds, etc) and change only the forcing agent (ie CO_2); the radiative forcing is the resulting change in flux at the tropopause. But we also speak of "forcing" and "feedback" when emulating GCMs with simple energy balance models. Among other things this helps us isolate the source of differences among GCMs and between GCMs and observations. But these two ideas — forcing as a purely radiative computation, and forcing as a parameter in an emulator of GCM responses — are not fully consistent.

The logic of a "stratospheric adjustment" to forcing has been clear since the work of Manabe and collaborators in the 60's. Forced stratospheric temperature changes are to first approximation independent of surface/tropospheric changes and adjust to a perturbation in the forcing agent much more rapidly than the latter (less than a few months). Their effect on the radiative flux into the troposphere does not scale with the surface temperature change. So it makes sense, especially if interested in time scales long compared to the stratospheric adjustment, to put the consequences of these stratospheric temperature changes on the forcing side of the equation. This has a modest effect for CO_2 but it can be more important for other forcing agents, such as ozone.

More recently is has become apparent that fast responses to changes in the CO_2 are not confined to the stratosphere in GCMs, but occur in the troposphere as well. Some of these are due to rapid warming of the land surface, but the most interesting are changes in the cloud field over the oceans that occur even when ocean surface temperatures are fixed, due in large part to changes in radiative cooling within the troposphere. See Gregory and Webb 2008. Aerosols produce other complications through their effects on the cloud field, but let's stick to CO_2 for simplicity.

When emulating GCMs with simple forcing-feedback models, it's informative to start with the simplest switch-on simulation: take a control run and increase CO_2 instantaneously, then hold it steady and watch the system equilibrate. The figure at the top shows the change in the cloud distribution over the oceans in an instantaneous quadrupling CO2 simulation, broken up into a fast adjustment and a feedback part. Only the latter part scales linearly with temperature change during the equilibration process. The feedback part in the right panel is the % change in cloud cover per degree K warming; the left hand adjustment part is the change for 4X increase in CO_2. (These are averaged over several GCMs). If the forced warming after 70 years due to the instantaneous 4X increase is 3K say, you multiply the right panel by 3 and add it to the left panel to get the total change. The presumption is that the fast cloud response scales more or less with the traditional radiative forcing, ie, it is proportional to the change in $\log(CO_2)$. The effect of these fast cloud changes on the net radiative flux at the top of the atmosphere (N) is not negligible.

Gregory et al 2004 have advocated for the use of a fit to the $N - T$ relation in the switch-on case (where the radiative forcing is independent of time after the switch on) to define the CO_2 forcing F by extrapolating N to

$T = 0$. The key point is to extrapolate using the $N - T$ evolution only after the fast adjustments have played out. (There is some sensitivity to how this is done, hinting that the distinction between adjustments and feedbacks is not totally sharp.) F will then contain the fast tropospheric cloud response and other adjustments. It will not be understandable in terms of a radiative computation in isolation. If you generate a big enough ensemble you can knock down the noise enough to extrapolate all the way back to $T = 0$ and isolate the pure radiative, or instantaneous, forcing, eliminating the stratospheric as well as tropospheric (and land) fast adjustments, but this will not be the best F for fitting the GCM on the longer time scales of interest.

An alternative method for isolating the fast response, utilized systematically by Hansen and colleagues, is to take an atmosphere-land model and then look at the response to an increase in CO_2, keeping sea surface temperatures and ice extent fixed. By fixing some things (the slow physics) and letting other things adjust (the fast physics) you are effectively defining what you mean by fast adjustments — the atmosphere and land are fast, the surface ocean and sea ice slow. There can be some differences between these two ways of getting at these fast adjustments, but I won't try to discuss those here. The left hand panel in the figure above was actually obtained with this alternative approach.

It seems that the cloud response problem has become more complicated, since it now consists of two distinct parts with different physics. You could argue that the fast adjustment is simpler than the feedback component, however. The details of the feedback component can be influenced by the patterns of the ocean surface temperature and sea ice changes which, in turn, involve slower physical mechanisms beyond the relatively fast atmospheric adjustment of the cloud fields once surface conditions are given. This suggests that uncertainty in the fast response might be reduced more quickly as cloud models improve than uncertainty in the feedback component. Maybe we should be grateful that there is a part of the cloud response that is not dependent on the added uncertainties in slow ocean/ice physics.

But can we hope to constrain the fast cloud response to CO_2 from observations? I don't think so — not in any direct way at least . As I have emphasized in a number of previous posts, in GCM responses of the climate response to CO_2 over the past century, and that expected over the next century, it is a useful first approximation to assume that N is the heat uptake by the oceans and is proportional to the temperature response T: $N = \gamma T$. The implication is that $T = F/\beta + \gamma)$ so T and F are proportional. This is not valid on the short time scales characterizing volcanic forcing,nor on the long time scales required for the oceans to equilibrate and for N

to approach 0. But on intermediate time scales, say 20-100 years, the assumption that forcing and the restoring flux and temperature increase together is supported by GCMs. Now suppose the change in the cloud distribution C can be divided into fast and slow parts as described above, with the fast part proportional to the radiative forcing and the slow part to the temperature perturbation: $C = CF + CS = \mu F + \kappa T$. But in the intermediate regime, F and T are proportional, so the ratio between the two parts will be unchanged in time (assuming that μ, κ, β, and γ are all constant in time) and observations of trends in clouds will not provide any way of separating the two parts. The bottom line is that we can readily separate these two pieces of the cloud response given an abrupt change in CO_2, but we don't have one of those to study. (And there is no reason to believe that the cloud response to volcanic forcing bears any simple relation to the fast response to a CO2 increase.)

72 Odd Recent Evolution of the QBO

[Originally posted October 8 2016]

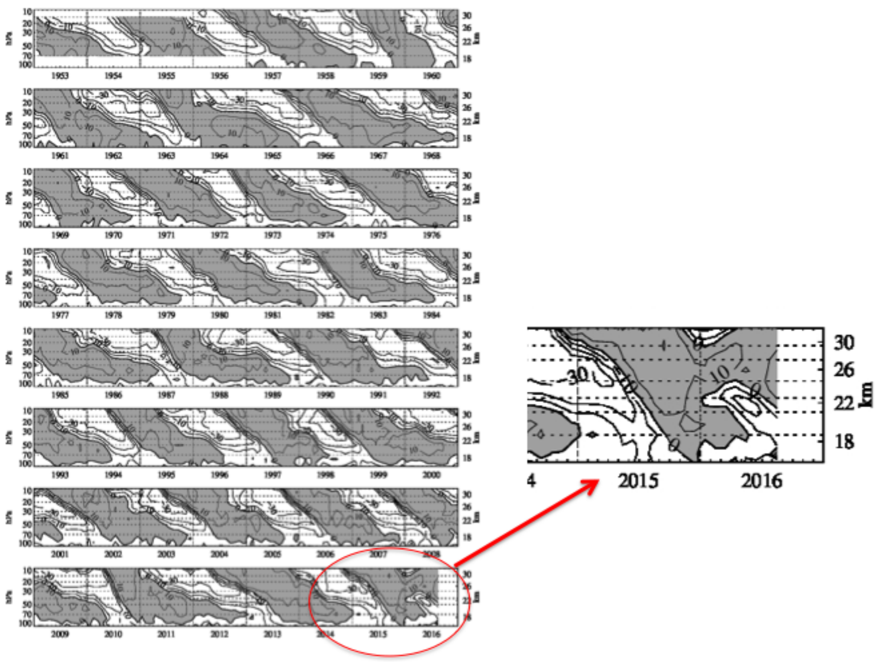


Figure 72.1: Monthly mean equatorial zonal winds in the stratosphere as a function of time and height. Eastward winds are shaded. 10m/s contour interval. This figure is updated monthly here, thanks to Marcus Kunze. (Original version created by Christian Marquardt (Marquardt, C. (1998): Die tropische QBO und dynamische Prozesse in der Stratosphäre. PhD Thesis, Met. Abh. FU-Berlin, Serie A, Band 9/Heft 4, Verlag Dietrich Reimer Berlin, 260 S.) I have highlighted the last two years.

The quasi-biennial oscillation (QBO) in the equatorial stratosphere is one of the more remarkable phenomena in our atmosphere. In a region of about 15 degrees north and south of the equator, the east-west winds change directions, from about 20 m/s eastward to 30m/s westward and back again with roughly a 27 month period. As seen in the height/time plot shown above, these alternating winds first appear with a given sign at upper levels and then descend more or less regularly before stalling and decaying near the tropopause. There is generally one eastward wind layer and one westward layer at any given time: a new layer of eastward winds appears at upper levels once the previous eastward layer near the tropopause has been squeezed away by descending westward winds. Baldwin et al. 2001 is a classic review of both observational and theoretical aspects of the QBO.

In its most recent evolution, the QBO has exhibited some strange behavior, as seen in the plot shown above — in the past year westward winds unexpectedly appeared at about 40 mb, interrupting the eastward winds in their familiar steady descent. This unusual behavior, evidently with no good analog over the period of our observations of the QBO, is discussed in two recent papers: Newman et al 2016 and Osprey et al 2016.

(Note on confusing terminology: meteorologists typically speak of easterly and westerly flow, emphasizing where the air is coming from, with easterly = westward and westerly = eastward. I am using the the eastward-westward terminology in this post.)

The theory for the QBO is one of the triumphs of atmospheric fluid dynamics. The starting point is two papers by Lindzen and Holton 1968 and 1972. The theory describes the evolution of a system that consists of two interacting components, a zonally symmetric jet, and vertically propagating waves generated in the troposphere by tropical moist convection, which then propagate into the stratosphere. The theory falls into the class that we call "wave-mean flow interaction theory"– the waves are assumed to be linear; the only nonlinearity is the interaction of the waves with the mean flow (the zonal winds). I usually recommend Plumb 1977 to students as the best point of entry into the theory, the essence of which goes something like this:

Some of the waves propagating upwards through the stratosphere have phase speeds that are eastward and some have phase speeds that are westward with respect to the ground. These waves have temperature perturbations associated with them and are radiatively damped as they propagate upwards through the stratosphere. The two keys to the theory, neither of which is very intuitive but follow straightforwardly from the linear wave dynamics, are:

1) Where a wave is dissipated, the zonal mean flow is accelerated towards the phase speed of the wave, and

2) The closer the zonal wind to the phase speed of the wave, the more

slowly the wave propagates vertically. In the presence of damping, this means that the waves do not propagate as far, confining their effect on the zonal mean flow to lower levels in the atmosphere.

Think of two waves with identical amplitudes at the tropopause, one with westward phase speed of 20m/s and one with eastward phase speed 20m/s, propagating upwards. And think of the zonal winds as being zero initially. Each wave accelerates the zonal winds towards it own phase speed over the region over which it is dissipated, but these two forces balance initially — in this rather artificial symmetric state. Now suppose you break this symmetry by making the zonal winds slightly eastward say. The wave with eastward phase speed now has slower vertical propagation and transfers eastward momentum to the winds in a shallower layer. The wave with westward phase speed propagates further into the stratosphere and deposits momentum through a deeper layer. The result is an eastward push to the winds in the lower layers and a westward push in the upper layers. You can iterate this kind of argument to follow the evolution as these new zonal winds modify the wave propagation and dissipation, to show that these winds will strengthen, saturating at the phase speed of the waves, while propagating downwards.

An important detail in generating oscillatory behavior is the necessity for a mechanism that mixes away the zonal winds near the tropopause — if there are eastward winds near the lower boundary of the stratosphere, while westward winds are descending and confining these eastwards winds to a shallower and shallower layer, a mechanism for destroying this shallow jet is needed to allow eastward propagating waves to bust through and propagate deeply to generate a new eastward wind layer at upper levels. See Plumb's paper linked above if interested in pursuing this.

The QBO evolves very slowly by atmospheric standards. For this picture to make sense the equatorial stratosphere has to be very well protected from waves and mixing initiated from midlatitudes. The wintertime stratosphere in particular is full of Rossby waves and turbulence that have the potential to disrupt the stately progression of the QBO if they can mange to penetrate close to the equator and mix things up. It seems plausible that the unusual evolution of the winds in this past year was the result of this protection breaking down, allowing some extratropical influence to penetrate to the equator. It is hard to construct an explanation of the zonal wind evolution over the past year, with westward winds appearing sandwiched between eastward layers,if you confine yourself to the classic picture of vertical redistribution of momentum.

Was the strong El Nino in part responsible for the unusual QBO behavior this past year? Was this behavior predictable, say, a few months in advance? Could it be telling us something about subtle trends in the stratospheric circulation that allow more extratropical influence on the equatorial winds?

To summarize:

> The stratospheric QBO missed a beat last year.
> Is this the end of civilization as we know it?
> Or is it simply that the stratospheric QBO missed a beat?

(with apologies to Alan Bennett)

73 Tuning to the Global Mean Temperature Record

[Originally posted November 28 2016]

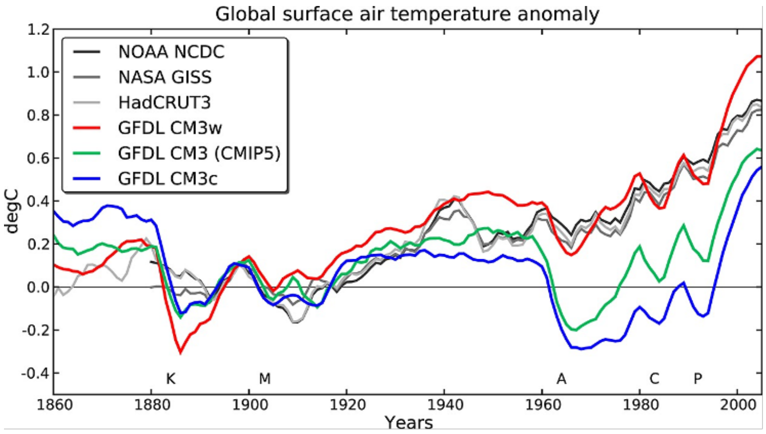


Figure 73.1: Global men surface air temperature evolution (with 5 year running mean) in 3 versions of GFDL's CM3 Donner et al 2011 compared to observations. Each model result is an average of 5 realizations. Anomalies are computed relative to the average over 1881-1920. From Golaz et al 2013.

I view the goal of climate modeling as the development of multi-purpose climate simulators. The same model generating the global mean temperature in this figure is also used to simulate the response of tropospheric winds to the Antarctic stratospheric ozone hole, for example. But as we all know, some aspects of the simulations in our current models are robust while others are sensitive to model uncertainties and may be tunable to some extent within the context of a particular model. If you have a simple model that you are fitting to some data, there is no problem in describing in detail how you decided on the model, the free parameters, the fitting procedure, the data used, etc. it can be more of a challenge to make the development path of a climate simulator fully transparent.

A question that gets a lot of attention is whether you should try to tune your model to be consistent with the evolution of global mean temperatures (GMT) over the past century, or if you should withhold that particular iconic data set during model development, justifying its use as a measure of model quality.

My impression is that it is the advent of models incorporating indirect aerosol effects (the effects aerosols have on climate through their modification of clouds) that has really brought this question to the forefront. The evolution of GMT is controlled by a combination of climate sensitivity, radiative forcing, and internal variability. I don't know of any way to robustly increase or decrease a model's internal variability on decadal and longer time scales. But in a given model you often find ways of altering the model's climate sensitivity through the sub-grid convection and cloud schemes that affect cloud feedback, but you have to tread carefully because the cloud simulation exerts a powerful control on the atmospheric circulation, top-of-atmosphere (TOA) and surface radiative flux patterns, the tropical precipitation distribution, etc. But including aerosol indirect effects on radiative forcing has made it easier to generate a greater variety of 20th century simulations without affecting other aspects of the climate simulation as strongly.

The figure at the top shows simulations from three versions of the CM3 coupled atmosphere-ocean model developed at GFDL in 2011. This was the lab's first attempt to incorporate the indirect effects of aerosols in a climate model. The three models are the result of varying a single parameter that controls the amount of cloud water required for the onset of coalescence in the models microphysics scheme, which in turn controls the water content of clouds. (Other parts of the model need to be adjusted to retrieve a good global mean TOA energy balance but are not the main drivers of this behavior.)

One of these versions clearly provides a better fit than the others. These differences are primarily due to aerosol radiative forcing, not climate sensitivity. The model that is most consistent with the observed evolution has the smallest aerosol forcing. (The figure shows 5-member ensemble means; individual realizations do not change the basic picture.) As described in Suzuki et al 2013, the value of the parameter that provides the best fit is not the one preferred by comparing directly to cloud observations; the settings that result in larger aerosol forcing seem more justifiable at face value. There is nothing mysterious about this. It's an example of the familiar tension resulting from the imperfections of any model and the need to weight different performance metrics relative to each other.

So how much should we weight the importance of the simulation of the historical warming relative to other observational constraints? Personally, I think it deserves a lot of weight. I prefer if possible to study models that provide a viable hypothesis for 20th century temperature change, pushing against other observational constraints as a necessary expedient. You might be able to arrange climate modelers along an expediency spectrum depending on how they weight low level constraints vis-a-vis more holistic aspects of the climate simulation.

In general, the quality of the 20th century trend has to be considered somehow along with other metrics deemed to be of significance. How much weight would need to be placed on these trends to say that they were "tuned"? Imagine a best case scenario in which weights are made explicit and the optimization is performed by an explicit algorithm. You would presumably have to give up on binary tuned/no tuned categories and know a lot of details about the optimization procedure if you really cared to quantify this. But this best case scenario is rarely relevant; why this is so is an interesting question that could be the topic of another post.

"Tuning" to the 20th century temperature trends is itself ambiguous. In particular, initial stages of atmospheric model development often take place without coupling to an ocean model, running instead over observed sea surface temperatures (SSTs) and sea ice extent. It is fairly standard to compute the Cess sensitivity (CS), increasing SSTs uniformly by some amount, 2K is common, and looking at the increase in the net radiation at the TOA, per degree. This computation gives you a first inkling of the model's climate sensitivity. Another standard computation is to hold the SSTs and sea ice fixed and change all radiative forcing agents from present day to pre-industrial values. Looking again at the change in the TOA energy balance, we call this change the effective radiative forcing or radiative flux perturbation RFP. Dividing RFP by CS gives you a scale for the temperature change from pre-industrial to the present day. It's not quantitative for several reasons (ocean heat uptake, the dependence of radiative feedbacks on the spatial structure of the SST changes, etc). But it is a reasonable expectation (whether it is always true is a separate question) that if you make a change in the atmospheric model that affects RFP/CS substantially, your overall warming from pre-industrial-to-present in the fully coupled model will change in the same direction. Suppose a modeling group is doing this routinely and sees that a proposed change in the model atmosphere modifies RFP/CS in a way that would likely push a coupled model in the wrong direction, and as a result this change is not accepted. Is this "tuning" to past GMT evolution, even though the model has not actually been used to simulate this evolution explicitly?

You could go further and talk about tuning to "emergent constraints" for climate sensitivity, observational metrics that are correlated with climate sensitivity when looking across model ensembles. Is it "tuning" of the 20th century temperature record if your decisions are justified on the basis of these emergent constraints alone and not the GMT evolution with a fully coupled model? Your answer might depend on whether you find this literature on emergent constraints convincing or not.

But irrespective of all these details, the key point, I think, is that bottom-up, first principles modeling coupled with observational constraints other than the observed GMT evolution still leave room to generate a substantial spread in aerosol forcing and climate sensitivity. So what does it mean if a group manages to get a good simulation without tuning? Were they lucky? Have they made a case for reduced uncertainty? Is being satisfied with a first attempt and not exploring the consequences of this uncertainty a form of implicit tuning?

I was interviewed recently for a news article on climate model tuning, which said that I claimed that: nearly every model has been calibrated precisely to the 20th century climate records—otherwise it would have ended up in the trash. "It's fair to say all models have tuned it," says Isaac Held. The word "precisely" changes the flavor of this sentence a lot, raising the spectre of overfitting. (I have no memory of using that word.) But I don't doubt that I did say the part inside the quotes. I am not very good at provided sound bites. Consistent with this post, a more accurate and long-winded sound bite would have been something like: in light of the continuing uncertainty in aerosol forcing and climate sensitivity, I think it's reasonable to assume that there has been some tuning, implicit if not explicit, in models that fit the GMT evolution well.

So is it worthwhile digging into the model development process and trying to quantify the explicit component of the tuning? I am all for transparency, and it is potentially useful as a reference to have the development path laid out in detail as best one can. But implicit tuning, which has the potential for coming into play when the target data set is as well known as the GMT evolution, is harder to quantify. In addition, just speaking for myself (as always in this blog), life is short, and it's not easy finding actionable intelligence in the details of a development path except where the process has clearly isolated an interesting dependency; some aspect of the simulation depending on the model formulation in an unappreciated way; in which case that dependency would probably need to be analyzed in detail in a stand-alone study that ideally made a case for robustness, without being mixed with the more contingent aspects of the model development trajectory.

Thanks to Chris Golaz, Larry Horowitz, Leo Donner, Ming Zhao, and Mike Winton for discussions on this topic.