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Denis Rancourt <denis.rancourt@gmail.com>

Some more comments on your climate sensitivity essay

Denis Rancourt <denis.rancourt@gmail.com> **Sun, Jan 8, 2012 at 3:29 PM**
To: "Raymond P." <rtp1@geosci.uchicago.edu>

Hello Ray,

I would like to continue our discussion about my paper (June 3, 2011, typos corrected in December 3,2011 update): "Radiation physics constraints on global warming: CO2 increase has little effect"
http://www.archive.org/details/RadiationPhysicsConstraintsOnGlobalWarmingCo2IncreaseHasLittleEffect

I hope you have some time for this. I will post this exchange.

I have collected below our previous emails to date about my article.

I respond to you latest (July 2, 2011, attached below) points as follows.

POINT-1:

You state (whole 2nd paragraph): "A very big flaw in the climate sensitivity essay is that you are unaware of both history and current practice. ..."

Reply: This does not point to a flaw in my calculations or physical assumptions but only justifies and qualifies the use of simple models.

POINT-2:

You state (from 3rd paragraph): "However, single-layer or two-layer models of the atmosphere are almost never used anymore, because they don't buy much simplification over what you can do with simple representations of OLR and surface fluxes based on multilayer models, but do incur very serious inaccuracies."

Reply: You are projecting a hope or belief here. You need to cite a study that demonstrates and quantifies the "serious inaccuracies" of a double-layer model relative to a same-physics multilayer model. My calculation shows that single and double layer models give comparable and adequate results to evaluate sensitivities and average global features.

POINT-3:

You state (from 3rd paragraph): "Further, there are real limitations to the use of a smooth representation of the absorption function, given the role of line overlap, especially at low pressures where spectral lines are thin. I do use the envelope model to make conceptual points regarding the source of the logarithmic behavior of OLR as a function of GHG concentration, but for quantitative calculations, one really does need a statistical representation of the effect of the line structure."

Reply: Again, if you want to argue that more complexity is needed in the model assumptions to arrive at the stated purposes of the model then the onus is on you to demonstrate that the extra complexity is needed over using average or smooth values. It's too easy to point at all the complexities that could be added. The only relevant question however is "Is the model sufficiently realistic in its simplifying assumption to achieve its stated objectives?" As you know, I can provide several arguments that your proposal of the particular needed complexity (structure of the broad intrinsic absorption band) is superfluous.

POINT-4:

Your 4th paragraph...

Reply: Again, you are not identifying an error in my calculations or physical assumptions. You agree that my model obtains a correct result but claim this arrises from "multiple errors canceling", without identifying a single such error or how it might be "cancelling". The onus is on you to describe the alleged error(s) in my simple model that uses correct physics and obtains correct results.

POINT-5:

From your 5th paragraph: "Regarding the specifics of your model, two particular errors are that you have computed the vertical temperature profile using pure radiative equilibrium, which ignores the important effect of convection."

Reply: My model does not attempt to get the actual (real) vertical atmosphere temperature profile. That should be more than obvious to you. One does not require all the complex features needed for a realistic vertical atmosphere temperature profile to calculate average planet gross radiation balance and sensitivies. You have already admitted the utility of simple models in the latter regard. Again, if you want to argue that atmosphere structure is needed here then the onus is on you to demonstrate that need.

POINT-6:

From your 5th paragraph, about the Gaussian model envelope for the CO2 IR absorption profile...

Reply: You are really belaboring this point without any evidence or cited work showing the need for a different-than-Gaussian overall envelope (or tails), for the purpose of the simple model. Again, you find no error of physics and are only waving your hands that more or different is needed. But the onus is on you to show that it is needed.

POINT-7:

Your 6th paragraph: "You also ignore the fact that, even apart from water vapor or cloud feedbacks, the mere existence of water vapor or clouds increases climate sensitivity by altering the equivalent radiating pressure level, even if water vapor or clouds did not change with temperature. This effect is included in the IPCC climate sensitivity, and is implicit in the calculation of the Planck feedback in Bony et al, and in Soden and Held."

Reply: You are stating that I am missing a physical mechanism ("water vapor or clouds increas[ing] climate sensitivity by altering the equivalent radiating pressure level, even if water vapor or clouds did not change with temperature") that is such that my model significantly underestimates the calculated sensitivities. Your assertion is contrary to the fact that I obtain essentially the same results for post-industrial and doubling of CO2 as all accepted (and more complex) models. However, I would be willing to consider your point more if you could please explain it in terms of physical processes. In particular, the phrase "altering the equivalent radiating pressure level" is ambiguous?

POINT-8:

You state (from your 7th and last paragraph): "There are various other errors in your calculation, but these are the main ones that come to mind."

Reply: I think you are saying that there are other errors but that you are not naming and that do not come to mind. There cannot be "other" errors until you find at least one error.

The rest of your last paragraph (as with many of your comments) is not relevant to the matter of finding errors in my paper.

CONCLUSION:

You are a recognized establishment-science expert in these simple models. You have not identified any errors in my model. I think you will agree that when a simple model contains no physics errors and no calculation errors and obtains correct results without over stretching (without seeking results that the model is not structurally able to legitimately obtain), then if you wish to argue that the model needs more complexity the onus is on you to

demonstrate that the added complexity is required rather than being superfluous.

Best,
-denis

---------- Forwarded message ----------
From: Denis Rancourt <denis.rancourt@gmail.com>
Date: Sat, Jul 2, 2011 at 7:56 AM
Subject: Re: Some more comments on your climate sensitivity essay
To: "Raymond P." <rtp1@geosci.uchicago.edu>

Hi Ray,

Thank you for continuing the dialogue.
I will be happy to continue exchanging about this with you.

At the moment, I have other demanding and immediate concerns: [...]

I will get back to our exchange as soon as I can.

best,
-denis

On Sat, Jul 2, 2011 at 12:04 AM, Raymond P. <rtp1@geosci.uchicago.edu> wrote:

> Hi, Denis,
>
> Here are a few more comments. I'm interested in this dialog mainly because it was interesting to me that you understood and corrected your error regarding Kirchoff's law and albedo when I pointed it out (even though you didn't go further and question whether there were other equally big blunders that compromised your conclusions). This had never happened to me before when pointing out flaws in reasoning of those who deny that climate change is a serious problem. So it was an interesting experience, and it would be interesting to see how far we can take this and where it will end up (time permitting).
>
> A very big flaw in the climate sensitivity essay is that you are unaware of both history and current practice. While GCM's get most of the public attention, in fact most of the theory of climate change was developed using the kind of simplified calculations that you are using. What's more they are still a big part of the toolkit used in understanding what more complex models are doing, and (as I said in our interview) as a first attack on completely novel problems that don't need or benefit from the full power of a GCM. For example, in the NRC Climate Stabilization Targets report (available free as pdf from the NAS press) the section on climate sensitivity includes some estimates of climate sensitivity based on grey-equivalent radiating temperature, which are even simplier than your calculation. And as I mentioned, my whole Planetary Climate book is based on the idea that big ideas come from deep analysis of simple models.
>
> However, single-layer or two-layer models of the atmosphere are almost never used anymore, because they don't buy much simplification over what you can do with simple representations of OLR and surface fluxes based on multilayer models, but do incur very serious inaccuracies. Further, there are real limitations to the use of a smooth representation of the absorption function, given the role of line overlap, especially at low pressures where spectral lines are thin. I do use the envelope model to make conceptual points regarding the source of the logarithmic behavior of OLR as a function of GHG concentration, but for quantitative calculations, one really does need a statistical representation of the

effect of the line structure.

The coarse vertical resolution is not catastrophically bad if you are just doing CO2 or methane alone, but if you try to simultaneously include water vapor, it goes really bad; that was one of the main problems with Arrhenius' one-layer calculation. In fact, in your calculation, the no-feedback climate sensitivity is, as one commentor on the blog pointed out, somewhat above the value IPCC obtains with more accurate radiative transfer. This is actually a case of multiple errors canceling, much as heppened in the calculation of Arrhenius. But your calculation of the no-feedback response is not too terribly far off, at least for a dry atmosphere where the two-layer model can be made crudely correct. Your main problem is that you cavalierly dismiss water vapor feedback without seeming to have understood anything about the very arduous work that has gone into the understanding of this subject.

Regarding the specifics of your model, two particular errors are that you have computed the vertical temperature profile using pure radiative equilibrium, which ignores the important effect of convection. Further, you shouldn't be using a Gaussian envelope. You seem to be trying to base this on the line shape, but you have that way wrong -- the lines in the troposphere and most of the stratosphere are collisionally broadened, and have a Lorentz shape, not a Gaussian tail. But even that isn't the relevant thing, since if one wants to crudely represent the absorption lines as some equivalent continuum, and exponential rather than Gaussian or Lorentz tail gives a better fit to the true behavior. (That's what one gets for a one-term exponential sum approximation to the full line-resolved calculation).

You also ignore the fact that, even apart from water vapor or cloud feedbacks, the mere existence of water vapor or clouds increases climate sensitivity by altering the equivalent radiating pressure level, even if water vapor or clouds did not change with temperature. This effect is included in the IPCC climate sensitivity, and is implicit in the calculation of the Planck feedback in Bony et al, and in Soden and Held.

There are various other errors in your calculation, but these are the main ones that come to mind. I think there is very little doubt left about the water vapor feedback, though cloud feedbacks are subject to very considerable uncertainties; however the important thing about that is the fact that no sound scientific argument has yet been able to rule out the risk of high climate sensitivity due to cloud feedbacks, and indeed paleoclimate evidence (notably the PETM and Pliocene) seem to argue for high climate sensitivity.

--Ray

---------- Forwarded message ----------
From: Denis Rancourt <denis.rancourt@gmail.com>
Date: Sat, Jun 4, 2011 at 11:44 AM
Subject: Re: climate
To: "Raymond P." <rtp1@geosci.uchicago.edu>

Hi Ray,

I see you found a bit of time for this.

You state: "you used a gaussian absorption profile in your attempt to deal with the wavelength dependence of

absorption,

but that leads to errors because it underestimates absorption in the tails. Moreover, the use of any crude envelope (even the exponential one I favor) underestimates the CO2 radiative forcing because there is considerable line structure within the envelope, and new absorption is added in the spaces between lines"

One has to look and see if a Gaussian best fit to the 15 um cross section band would give significantly wrong saturation-degree estimates. There is no reason to assume this a priori. It is a reasonable approach. A Lorentzian line (with exaggerated wings) gives the same saturation width variation answer. Therefore, you have the onus to demonstrate the validity of your critique. Give me a reference where this has been examined by calculation. Your line structure objection is not correct on a prima facie basis because there are as many troughs as peaks in the "structure". Please check your intellectual honesty on this point.

You state: "But it's absolutely correct that a one-layer atmosphere is insufficient to treat the saturation."

Saturation does occur correctly in a given single uniform slab of absorbing material. You are saying that one needs several slabs (layers) each having its own temperature-density-etc. in a radiative transfer approach in order to correctly estimate CO2 surface temperature scenarios. I find that a double-slab (radiative-transfer) model works well and gives the same scenario predictions as several other simple models (including single-layer models) and as the 1D-dynamic and GCMs reviewed by the IPCC...

Now you are not pointing out errors in physics but rather expressing your criticisms regarding how simple models best represent (or misrepresent) the essential features of reality. My (Kirchoff-corrected) simple models follow physical laws and make reasonable simplifying assumptions (e.g., Gaussian profile, two layers...) and obtain correct (accepted) predictions. At this point, we are therefore beyond posturing and hand waving. You need to send me to a paper or calculation that shows any key aspect of my physical argument to be wrong.

Regarding your broad "utter nonsense" statements about carbon fluxes, sorry I can only respond to actual specific criticisms. If you show me to be wrong I will correct it. I have demonstrated this with the Kirchoff Law error.

best,
-denis

On Sat, Jun 4, 2011 at 11:54 AM, Raymond P. <rtp1@geosci.uchicago.edu> wrote:

> But it's absolutely correct that a one-layer atmosphere is insufficient to treat the saturation. And if I recall correctly,
> you used a gaussian absorption profile in your attempt to deal with the wavelength dependence of absorption, but that leads to errors because it underestimates absorption in the tails. Moreover, the use of any crude envelope (even the exponential one I favor) underestimates the CO2 radiative forcing because there is considerable line structure within the envelope, and new absorption is added in the spaces between lines.
>
> At the point where I had encountered so many serious and elementary errors in your treatment, I just lost interest in reading further. It was an exercise in futility. But I hope you continue trying to learn this stuff. Your early writings on the carbon cycle and accumulation of anthropogenic CO2 were also utter nonsense. Have you ever corrected your misconceptions on that and retracted that nonsense? I am a bit suspicious because nothing you ever learn seems to change your opinions of the consequences, so I begin to think you have a preconceived notion of the conclusion you want to reach (politically) and nothing will budge you from that.
>
> Still, I look forward to our conversation on the radio show.

> On Jun 4, 2011, at 10:45 AM, Denis Rancourt wrote:

> > You then incorrectly stated about my revised (second, May 12, 2011) post that I was not handling IR optical thickness (saturation) correctly and you have insisted that only a multi-layer atmosphere is realistic enough to

produce correct estimates of CO2 surface temperature scenarios.

---------- Forwarded message ----------
From: Denis Rancourt <denis.rancourt@gmail.com>
Date: Sat, Jun 4, 2011 at 10:45 AM
Subject: Re: climate
To: "Raymond P." <rtp1@geosci.uchicago.edu>

Hi Ray,

At least this way you have a record of my understanding of the physics before we do our radio interview of June 30th.

I appreciate your offer to read it when you have time and look forward to any feedback.

As far as I know the only physics error you have ever correctly pointed out to me is the Kirchoff Law error of my first posted (May 9, 2011) attempt.

You then incorrectly stated about my revised (second, May 12, 2011) post that I was not handling IR optical thickness (saturation) correctly and you have insisted that only a multi-layer atmosphere is realistic enough to produce correct estimates of CO2 surface temperature scenarios.

best,
-denis

On Sat, Jun 4, 2011 at 11:23 AM, Raymond P. <rtp1@geosci.uchicago.edu> wrote:

> Dennis, if you think you have anything to say about this that will bear
> scrutiny, you ought to submit your calculation to a peer-reviewed
> journal, such as Journal of Climate, or perhaps one of the new
> open-review journals. If I find time I will try to help you educate
> yourself on these matters, since from the previous things you've written
> it's clear you still have a long way to go, but things are pretty busy
> and none of us have time to correct everything out in the blogosphere,
> since there is a lot of other work to be done. So please do not take
> silence as indication of agreement with your calculation.
> --Ray
> On Jun 3, 2011, at 9:54 PM, Denis Rancourt wrote:
>
> Dear colleagues,
>
> I posted this today:
> http://climateguy.blogspot.com/2011/06/radiation-physics-constraints-on-global.html
>
> I don't think there are any errors in my calculations.
>
> Please don't bother with the political commentary parts but if you find any error in the physics do let me know what you believe it to be.
>
> Sincerely,
> Denis Rancourt
>
>

---------- Forwarded message ----------
From: Denis Rancourt <denis.rancourt@gmail.com>
Date: Fri, Jun 3, 2011 at 9:54 PM
Subject: Re: climate
To: "Raymond P." <rtp1@geosci.uchicago.edu>
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>, mann@psu.edu, ammann@ucar.edu, Rasmus Benestad <rasmus.benestad@met.no>, Ray Bradley <rbradley@geo.umass.edu>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Stefan Rahmstorf <rahmstorf@pik-potsdam.de>, Eric Steig <steig@uw.edu>, Eric Steig <steig@u.washington.edu>, David Archer <d-archer@uchicago.edu>, garidel@marine.rutgers.edu, Jim Bouldin <jrbouldin@ucdavis.edu>, William Connolley <>

Dear colleagues,

I posted this today:
http://climateguy.blogspot.com/2011/06/radiation-physics-constraints-on-global.html

I don't think there are any errors in my calculations.

Please don't bother with the political commentary parts but if you find any error in the physics do let me know what you believe it to be.

Sincerely,
Denis Rancourt