Earth Syst. Dynam. Discuss.,
doi:10.5194/esd-2015-84-RC2, 2016

Earth System Dynamics Discussions

Interactive comment on "A conceptual model of oceanic heat transport in the Snowball Earth scenario" *by* D. Comeau et al.

Anonymous Referee #2

Received and published: 9 April 2016

I love a good box climate model, and the authors' approach to simplifying complex Snowball climate dynamics down to its fundamentals is admirable. However, the authors have made two serious errors in their analysis, one conceptual and one mathematical:

1) No model of global oceanic heat transport can neglect atmospheric heat transport: the latter is about six times as large as the former in the modern climate.

2) The authors' integrals calculating top-of-atmosphere heat fluxes and geothermal heat fluxes in equations 3-6 are missing a factor of 2 pi.

In a surprising coincidence, these errors cancel each other out, leading to reasonable values for the model's oceanic heat transport and temperature gradient. See the discussion below.

I have two second-tier concerns:

3) I don't think the model has reached equilibrium, which calls into question the authors' results.

4) Like atmospheric heat transport, atmospheric water transport should not be ignored.

Unfortunately, there's not a simple fix which would allow the paper to be published with revisions. Solving problem #3 is easy. Fixing the math error in #2 is simple, but would result in a model climate which is wildly different from reality, because of the conceptual error #1. Fixing that would require major changes to the model, and a total reanalysis of the results.

I rush to add that I really like the philosophy and goals of this paper, and when the authors have addressed the flaws I've outlined here, I would love to review the new manuscript.

I discuss these problems in more detail below, along with additional minor comments.

==

1) Neglect of atmospheric heat transport.

For a given surface temperature, the net heating or cooling of each latitude is fixed by the top-of-atmosphere radiation balance (Trenberth and Caron 2001, Figure 1). Any imbalance in TOA radiation in a given latitude band must be compensated by net heat transport into or out of that band (TC01, Figure 2). This could be achieved by atmosphere or ocean, but in the present-day climate, only about 1/5 of this is carried by the ocean: the atmosphere does most of the work (TC01, Fig 7).

The authors' ocean model is driven directly by TOA radiation balance integrated over the surface of their box, and the integrand is a good match to observations (TC01 Fig 1). The integral of TOA radiation over the tropical box should amount to 5 petawatts, as in observations, but unlike the real world, in their model all of this must be transported by the ocean: their ocean's heat transport must be five times greater than reality.

To achieve this, the model's ocean circulation would need to be five times more vigorous, or the tropic/polar temperature difference about five times greater, than is observed. This would make the model's base climate so divergent from reality that their Snowball experiments – which depend crucially on meridional temperature gradient – would be called into question.

However, on Page 16, they note that the model produces quite reasonable ocean circulation and temperature gradients. How can this be? Their integral of TOA radiation is too small by a factor of 2 pi, which nicely cancels out the neglect of the atmosphere in the modern-day climate (Point 2 below).

The atmospheric heat transport could be included in a box model. I would suggest the authors track surface air temperature separately from ocean temperature: determine it diagnostically by a balance between TOA radiation, atmospheric heat transport from one box to the other, and air-sea heat exchange:

TOA = F_atmos + F_airsea

Let flux between atmospheric boxes, and exchange between air and sea be governed by the same sort of mixing dynamics used for the ocean boxes, with exchange constants tuned to match observations. Crucially, there is no time derivative in this equation, so the atmosphere temperatures can be solved algebraically given the temperature of the ocean boxes in each timestep.

The oceans would be forced entirely by F_airsea in such a model, rather than the TOA fluxes: since these are weaker, the ocean circulation would be in line with observations.

It's all quite doable, but it's a different paper than the one the authors have submitted. If they wish to go in this direction, I would be delighted to review the manuscript again in the future.

2) Missing factor in integrals.

The integrals in equations 3-6 are intended to be integrals of a vertical flux (watts per square meter) over the surface area of the box, to give a total flux in watts. They are obtained by multiplying the flux at each colatitude by the area of a thin circular "ribbon" encircling the globe at that colatitude, and then adding up all such ribbons. The north-south height of the ribbon is dy = r_e*dtheta; its east-west length is the circumference of the globe at that colatitude, 2*pi*r_e*sin(theta). The 2*pi factor is missing from these equations.

As a result, the authors' radiative forcing is 6.28 times weaker than it should be!

3) Model not at equilibrium.

I was alarmed by the statement on Page 18: that the ice thickness increases continuously in the fully glaciated model. Eventually, the ice should thicken enough that geothermal heat input to the ocean balances conductive heat loss through the ice shell. Goodman et al (2003) argue that this should happen at an ice shell thickness of 1200 meters, and in their model equilibrium is achieved only after 30,000 years. It's possible the authors' model is not yet at equilbrium: they shows a thickness of just 300 m (and still thickening), achieved after a shorter run of 20,000 years. The authors' model may take even longer to reach equilibrium than Goodman (2003), because their ice boundary layer parameterization may limit the rate of heat transfer.

4) Atmospheric moisture fluxes.

Goodman (2006) finds that about 20% of the thickness of a sea glacier would be composed of ice derived from snowfall ("meteoric" rather than "marine" ice.) And that's in a hard snowball climate where precipitation is measured in mm per year! In the partially-glaciated cases considered here, the thickening and flow of the ice sheet due to snowfall probably cannot be neglected. A box-model-style mixing parameterization between atmospheric boxes, with a Clausius-Claperyon dependence on temperature could probably be used here.

===

Minor comments:

Page 4 Line 18: "The circulation itself is driven by wind stress in and near the Southern Ocean" âĂŤ need citation; note Munk and Wunsch 1998 “Abyssal Recipes II” which argues that the AMOC is driven partly by mechanical mixing from tidal action.

Page 4 Line 20: headâĂŤ> heat

Page 10: Box numbers are hard to keep straight. I'd suggest labeling the variables like this: rho_ut = upper tropics, rho_dp = deep poles, etc.

Page 14: Can D be estimated or justified from observations of modern ice shelves?

Page 16, line 6: latitudinal, not longitudinal, right?

Equation 7 appears to be valid for theta = latitude, not colatitude. Presumably the authors have fixed this mixup in their code, or they'd get box 1 colder than box 2!

Page 18: Why is the ocean circulation negative (sinking at the equator) in the Snowball simulations? In equilibrium, the sea glacier should be freezing at the pole, and melting at the equator, so brine rejection would lead to high-latitude sinking. Temperature effects should be negligible, since the ocean should be at the freezing point everywhere.

Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2015-84, 2016.