Earth Syst. Dynam. Discuss., 2, C51–C55, 2011
www.earth-syst-dynam-discuss.net/2/C51/2011/

Earth System Dynamics Discussions

Interactive comment on "Climate change, in the framework of the constructal law" *by* M. Clausse et al.

Anonymous Referee #1

Received and published: 11 April 2011

The aim of the manuscript "Climate change, the framework of the constructal law" by Clausse et al. is to apply Bejan's constructal law to a climate box-model, in a climate change scenario, in order to provide a simple understanding of it by showing its predictive power. Although this is an interesting and intriguing idea and the subject fits very nicely in ESD, the paper is sometimes a bit sloppy and superficial and needs to be a bit more precise; also some of the issues which the authors raise in the manuscript have to be discussed a bit more carefully before I can recommend it for publication.

1 General remarks and main points

1) In the first line of the abstract the authors say that this is an approach "alternative to the complex models of Earth...". However we can say the it really is alternative if we can use it to obtain information which is as accurate as the one provided by other models (e.g.GCM). It seems to me that, at this level, the constructal law (CL in the following) gives only a qualitative order-of-magnitude answer, but more research and experimental confirmation is needed (at least in the atmospheric and climate sciences) before we can say it is "alternative";

2) As a consequence of point 1), the authors could comment about the predictive power of the CL in climate science: do they think that it may be used for more precise predictions or will it be confined to box-models only? May it be used to constrain GCMs or improve them? may it be used to assess them? Is it possible to test it in stricter way? In other words it would valuable to discusse the (possible) contributions that the CL might give for improving and understanding, not only in a qualitative way, the climate change;

3) Often in the manuscript it is claimed that the CL gives a reasonable predictions of the configuration of the mean meridional flow (i.e. the limits of the Hadley and Ferrel cells). This is achieved by invoking the maximisation of the meridional heat flow (e.g. eq.(27)). However the meridional heat transport is not due only the the meridional heat flow (i.e. the Hadley cell) but also to the transient eddies (baroclinic waves) which at the midlatitudes provides the main mechanism of meridional transport of enthalpy and moisture (see e.g. "Physics of Climate" by Peixoto and Oort). This is never mentioned in the paper but it needs some discussion. Do the authors think that the CL may give a hint about that as well?

4) Section 5 deals with a continuous one dimensional model of climate. It is a natural development of the two-box model used in the previous sections. Because the CL results for this continuous model can be easily compared with GCMs or observation, it

ESDD
2, C51–C55, 2011

is, in my opinion, very important to formulate it (in terms of the CL) in the most accurate and unambiguous way. However this section is quite hard to read and therefore its structure has to be improved before the paper may be accepted.

First, in eq. (33) to (37) the paramter f appears, which has been defined as a function of θ (eq. 5) but here it considered as a constant;

Second, are the parameters ρ and γ function of the latitude (as in reality for ρ) as well or assumed constant?

Third, it is clealry wrong the statement "symmetry requires $dq/d\theta = 0$ at the equator", as shown by the authors also in Fig.7 ($dq/d\theta \neq 0$ at the equator);

Fourth, the formulation of the Constructal law, which for a two-box model is comprehensible ($q = q(x)$ to be maximised, that is to find the engine configuration x_{max} which maximises q), is in this section rather obscure and must be stated in a more precise way (in particular the derivation of eq. (38)). Also the statement "At every latitude θ the heat flow (Eq. 34) depends on the albedo ρ, greenhouse factor γ and temperature T...." is conusing because eq. (34) just shows that q is defined through an integral, therefore $q(\theta)$ depends on the value of T, ρ and γ at all the latitudes over which we integrate; also the statement "The CL requires maximum heat flow at all latitudes, therefore maximising $q(\theta)$" is not clear: how do you maximise q and respect to what? It should be respect to a configuration but how is now a configuration involved now?

2 Specific remarks and minor points

1) it would be nice the derive in an appendix or shortly in the main text equation (5) for the Earth-Sun factor as it may be not self-evident for many readers;

2) T_S in climate literature and textbooks refers usually to the surface temperature whereas here it refers to the mean brightness temperature of the Sun. This may be

ESDD
2, C51–C55, 2011

potentially confusiong and a different symbol would avoid it;

3) line 10-11 in Sect.2. It is not understood the importance of mentioning T_∞: either it is discussed properly or removed (as it is not fundamental for the rest of the paper);

4) Sect.3. In the definition of the dimensionless formulation: do the scales used (T_{scale}, t_{scale}) have a physical meaning? How are they chosen??

5) line 10 Sect.4. The equations should be (23)-(25) and (27);

6) line 17 Sec.4. The difference between 57 and 33 degree is not small, some comments about it would be needed;

7) page 251: what is the reason why $\delta\rho$ and $\delta\gamma$ are set to (0.002,0.011) and (0.002,0.005) in case (A) and (B) respectively? A very brief physical justification would be needed;

8) line 7-8 page 252. It is unclear how values found in Sec 4.1 are compared to Hensen's results. What does it mean "temp. increase of 1.2 K for an increase of 0.6 K"? Here it is importan to be as clear as possible, as comparison with GCM results is crucial to test how good the CL is;

9) Sect. 4.3. Is not also the Ramp function a continuous one (its derivatis is not, but the function it is)?

10)line 20-25 page 253: there may be some mistakes with the years (2000, 120??). Also it should be $\gamma = 0.411 + ...$ and not $\gamma = 0.4$ in order to have a continuous function;

11) line 24 page 254: why is there not a factor π in the area perpendicular to the sunrays (the infinitesimal area of the Earth surface to be projected is $2\pi R^2 \cos\theta d\theta$)?

12) first paragraph pag. 256: that is a trivial consequence of the fact that in the tropical region more shortwave radiation is absorbed than emitted through longwaves (and the other way around in the polar regions);

ESDD
2, C51–C55, 2011

Interactive Comment

13) eq. (34). This definition of q implicitly assumes that $q(0) = 0$ (which in general may not be the case); a better definition is obtained when the integral is taken between $\pi/2$ and θ (zero transport at one of the poles), as in Peixoto and Oort "Physics of Climate".

Interactive comment on Earth Syst. Dynam. Discuss., 2, 241, 2011.