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Earth System Dynamics Discussions

Interactive comment on "MEP solution for a minimal climate model: success and limitation of a variational problem" *by* S. Pascale et al.

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I would like to thank the Anonymous Referee #1 for his comments which will certainly lead to improve the manuscript. Most of them are sensible and are associated with a lack of clarity in the paper. I am not going to deal with them in this comment because I will take care of them in the revised version of the paper.

My intention here is just to discuss a few points raised by the referee in his interactive comment which may look to be more controversial and try to clarify them before the revision of the paper is completed.

R- "Another concern of importance to me is the questionable relevance of the experiment presented in section 6. As far as I understand the MEP conjecture, it provides

a way to compute without empirical parametrisation some quantities (fluxes in most cases) associated to degrees of freedom whose macroscopic equations of motion are "unknown". Therefore I am not convinced that it makes sense to apply MEP optimisation to quantities pertaining to radiative transfer, since the laws of radiative transfer are in fact "known".

A- I agree, the laws of radiative transfer are well known but the distribution of the optical properties within the atmosphere are not known a priori as they are determined by the climatology. Let us note also that in general this T-tau relation is very complex as it depends on several diverse variables i.e. the climatic distribution of water vapour, temperature, clouds, aerosols, etc which may not be related in a simple way. Therefore we are not trying to use MEP in order to by-pass the radiative transfer laws (which in the model are given, although in a simplified way, see equations (10)-(12)), rather to check whether MEP can determine not only a realistic temperature distribution but also (indirectly) a realistic distribution of water vapour and clouds. Also one may think that a situation like the one presented in Section 6 may concern the case of a planet with a complete different atmospheric composition, in which we really know nothing about the longwave transmissivity and its relationship with the temperature. However the unrealistic results point out that this is not a well-defined set for applying MEP, as suggested by Referee #2.

R- "To illustrate this point, recall that albeit the major contribution to the global entropy production of the Earth is due to radiative processes, one only maximises the contribution associated to turbulent heat fluxes in the "standard" MEP procedure".

A- I agree, but in fact no one has tried to maximise the entropy production due to emission-absorption of radiation, just the material entropy production. Again, although the transmissivity field may be associated with radiation, it is entirely a "material" field, i.e. determined by the "climatic fluid".

R- "Page 408,L13: "Nevertheless we may say that if the actual model solution is one of maximum entropy production..." I disagree with this sentence: if the longwave transmissivity varies with T the MEP state for this model has no reason to coincide with the one obtained with prescribed longwave transmissivities. The only a priori statement one can make is that you expect the MEP state in the first case to be more realistic than for prescribed transmissivity".

A- This is a good remark. I think that our point should be stated more carefully. To rationalize our argument, imagine a (T,tau) plane where each point represents (symbolically) a steady state with a certain T and tau distribution. Material entropy production will be defined on such a plane as it depends, in our model, only by T and tau, and it may have an absolute maximum. The model (FAMOUS) might not find the absolute maximum in (T,tau) because of the physical constraints. Its trajectory in (T,tau) might not pass through the maximum, and then a line of constant tau (experiment described in Section 5 with fixed tau distribution) through the model solution might find a point with a higher EP at a different T from the model. So the argument demonstrates only that the model is not in an *unconstrained* entropy maximum, which is weaker, but clearer in the end. Because the relationship between T and tau is very complex and climatically determined, it cannot be expect to know it in advance. Thus in the end this part demonstrates that you cannot apply MEP without knowing the constraints, and so you cannot apply it at all if you do not know a priori what constraints apply, which is usually the case with a system you do not thoroughly understand already.

R- "Page 413, L8: "We note that MEP does not give us a temperature field...." Due to the fact that the optical properties of the atmosphere are fixed, one cannot honestly expect that the temperature field be consistent with these values, independently of the MEP conjecture. In fact the temperature field corresponding to radiative equilibrium is not consistent with these longwave transmissivities either. Neither would be any other model than FAMOUS with fixed transmissivity...Hence I do not believe you can draw conclusions as to the eventual validity of the MEP conjecture based on such grounds.

A- It is true that, rigourously, the only temperature field which is absolutely consistent

with the time mean field of transmissivity would be only the one obtained from a FAMOUS run with fixed transmissivity. Therefore here the point raised by the referee is if it is acceptable to ignore the daily and seasonal cycle in the MEP formulation and then compare it with the output of a model that instead simulates these cycles. In general this is not, given the nonlinearity of the equations. However we argue that the amplitude of the seasonal and daily variability is small if compared with the annual means. Moreover, based on the experience, we know that until the '80s the only models used were energy balance models defined in terms of mean values which compared fairly well with the real climatology. So we believe that, in these cases, some conclusions can be drawn but having in mind the limits of the model.

Interactive comment on Earth Syst. Dynam. Discuss., 2, 393, 2011.