Earth System
Dynamics

Open Access

Discussions

EGU

Interactive comment on "ESD Reviews: Thermodynamic optimality in Earth sciences. The missing constraints in modeling Earth system dynamics?" *by* Martijn Westhoff et al.

Maarten Ambaum (Referee)

m.h.p.ambaum@reading.ac.uk

Received and published: 29 March 2019

Review of Thermodynamic optimality ... by Westhoff et al.

This paper is written by a broad range of authors with a high level of expertise, who are well placed to provide an overview of thermodynamic optimality principles in earth system science. It is a well written and very readable paper of appropriate length, although I did feel that at points the paper suffers from a lack of detail, so that any reader who is not completely familiar with the background material does need to refer back to the primary literature on all topics. I personally enjoyed reading the paper, and enjoyed picking up on some of the topics that I was less familiar with.

Reviews on this area have been written before, and I was looking at what this review adds above our present understanding; the paper promises quite a lot from the outset. I am afraid to say that in my opinion the paper does not deliver on these promises. In summary, the paper restates our lack of understanding about general properties of TOPs, but does not add any new insight.

The first sentence does not at all reflect my understanding of the area: thermodynamic optimality principles, as addressed in the present paper, have little solid physical foundation. My reading of the literature, including some of my own contributions, is that many people attempt to demonstrate an underlying optimality principle in some given system; that is different from people using optimality principles to estimate model parameters or fluxes that are not already known or estimated from other physically explicit models. These optimality principles are invariably used post-hoc, and not as calculating principles. Of course, the authors admit as much in the final paragraphs of the introduction.

In line 18 of the abstract we are promised that there is a correct and consistent use of the maximum power principle, which sounds far fetched to me. Maximum power is at best a hypothesis with a good amount of circumstantial evidence. However, it does not have the status of a physical theory which has a well defined application area and procedure. At best we can hope to give a geography of cases where an application of maximum power appears to give a physically realistic result. Of course, any new results or understanding in the rest of the paper could prove me wrong, but I do not think such new results or understanding were provided; in fact the paper mostly is a descriptive geography of applications of TOPs. Fun to read, but probably not adding much insight.

Your third paragraph (p.2, l. 12-20) is a case in point: this is a great example of a falling object reaching terminal velocity in the presence of friction. It is used to point to the potential of thermodynamic optimality principles. Of course, the ultimate balance at terminal velocity is between production of heat at the expense of potential energy, and

that seems to point to some possible thermodynamically optimal limit. But this does ignore the fundamental fact that, especially at higher Reynolds numbers, the primary, and limiting conversion is between potential and kinetic energy of the surrounding fluid. The balance is dynamic, not thermodynamic, evidenced by the fact that the energy conversion rate does not actually depend on the viscosity of the fluid, as long as the Reynolds number is large. The heat production is incidental; the terminal velocity is determined by a drag coefficient which is essentially a geometric property of the falling object.

Section 2.1: all basic and correct, but in section 2.2, and the rest I am surprised that there is never a reference to the Curzon-Ahlborn work on maximum power production which seems to me to address many issues of how heat is fluxed though a system using explicit models of conductive heat flux. The discussion around Eqs 3 and 4 imply a very particular physical set-up which is not at all explained in detail. For example: which flux is meant? The flux at the input terminal is typically different from that at the output terminal if mechanical work can be extracted. If the mechanical work is re-injected in the system, then we need to know at what temperature this happens. The schematic in Fig 1 implies this is re-injected at the output temperature (something that is not obvious at all: think about the thermodynamics around energy dissipation in tropical cyclones). I think you refer to this issue on p.21, l.15-21, but that was not particularly explicit. In other words, this part 2.2 does not explain much. Note also there are more explicit versions such as in Bejan's book and in my own book on Thermal Physics of the Atmosphere (Chapter 10.3) where you can find explicit expressions of "lost work" due to entropy production and of the effective temperatures of the input and output terminals. There you can also see that, for example, T_c depends on where the heat is lost from the system, so it is a geometric property of the system as much as a thermodynamic property. In that sense, T_c is not a function of J but of the whole of the fluid state. To take T_c as a function of J is a statement of belief, or approximation, but not a statement of a physical principle.

Printer-friendly version

Discussion paper

Just as an aside to p.9, l. 31, as part of a PhD project (thesis by J. Kamieniecki, University of Reading, 2019) we repeated the work by Herbert et al. (2013) and found that they do not offer the complete picture in their paper: the profiles become really rather unrealistic above the levels they plot in their paper. So the interpretation of this result is that the MEP principle did produce profiles that look somewhat realistic in a limited part of the vertical column, but as a whole are unrealistic.

You refer to our work on p. 12, l. 19-27; the description is correct. The ultimate failure of our work has to do with the fact that energy conversions were dominated by latent heat fluxes which scale more with the mean temperature of the system, rather than the temperature gradients. My suspicion is that we need to somehow exclude chemical conversions, such as described by Pauluis in several papers as the "Gibbs penalty" (such as in Kamieniecki et al., 2018, J. Atmos. Sci.).

The authors are honest in that they do not avoid the fundamental failure of TOPs, such as in Section 3.3, which in my view should be interpreted as: several applications give broadly sensible results, but they often do not survive deeper scrutiny, or broadening of application range. It looks like the initial set-up of the physical problem encodes the outcome, not the TOP itself.

In section 3.4 I must admit that I am not an expert on this literature, but I thought that non-linear chemical reactions have been widely used to explain pattern formation in nature, with an essentially thermodynamic argument: the free energy being a non-linear function of some order parameter. Apologies if this sounds a bit vague, but I would have expected that a review, addressing pattern formation, would acknowledge that part of the physics literature which, as far as I understand it, is reasonably well established.

In section 3.5, I must again admit that this is not my specialist area of expertise, but the process described in p.17, l. 1 sounds very similar to the mechanism underlying the sandpile models of Per Bak, leading to SOC, which has a substantial body of literature

around it. I may well be completely wrong here, but I would be surprised if there was no link between the SOC states and some appropriate TOP state in such models. At the end of Section 3.5, the authors, admirably, point again to an observed limitation of TOPs.

In Sec. 5.1 you discuss MP vs. MEP, and essentially argue they point in broadly the same direction. I agree with that, but it also means that, in the absence of a first principles physical theory for TOPs, we cannot decide at this stage which of the two options is the better one. It is clear that MEP has the potential to be related to MaxEnt, while MP might well be related to ideas around stationary action principles in Lagrangian or Hamiltonian descriptions of physical systems. There does not seem to be an overriding argument presented either way.

Your discussion of minimum entropy production, as in Prigogine's work, seems to miss some clarity. I would have thought that minimum entropy production is a well established outcome for a system in the presence of linear fluxes (such as in Fourier's law) and fixed boundary conditions. The big transition to the kind of systems we are studying must then be the non-linearity of the fluxes, where flux values are linked to gradients in a non-trivial way, or the boundary conditions.

Your minimum requirements in section 5.3 appear mostly self-evident extractions from past experience, and therefore would not and should not exclude other ways that TOPs might be operational in the future.

Your set of questions in section 5.4 address some of the fundamental issues, especially questions 2 and 4; these are issues that everybody working in the field has been aware of for many years; it looks like this review has mostly restated these issues, and by giving a list of applications with successes and failures has only restated the gaps in our understanding.

Your very final sentence then summarizes what I dislike about this review: "But this is only possible of the principle is applied in a transparent and correct way". This

sentence implies there is such a thing as "the" principle, and that there is such a thing as the "correct" way. Both of these are highly disputed in the cited literature and the present review does not provide evidence for a solution for either of these.

I found the paper well written, and have therefore not picked up on any typos. Here are three I did spot and did record: p. 2, l. 21: an –> a p. 20, l. 27: "a couple" means 2; you mention 6. p. 23, l. 13: fuzie –> fuzzy

---

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2019-6, 2019.