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Article

A Story and a Recommendation about the Principle of Maximum Entropy Production

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Abstract: The principle of maximum entropy production (MEP) is the subject of considerable academic study, but has yet to become remarkable for its practical applications. A tale is told of an instance in which a spin-off from consideration of an MEP-constrained climate model at least led to re-consideration of the very practical issue of water-vapour feedback in climate change. Further, and on a more-or-less unrelated matter, a recommendation is made for further research on whether there might exist a general "rule" whereby, for certain classes of complex non-linear systems, a state of maximum entropy production is equivalent to a state of minimum entropy.

Keywords: maximum entropy production; minimum entropy; water vapour feedback

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When the organizer of this special issue of *Entropy* asked me for a contribution to his volume some little while ago, I explained at the time that I have a religious objection to making a fool of myself in public, which would be the inevitable outcome if I tried to produce something erudite and deeply scientific on a subject that got beyond me at least 25 years ago. "Get lost!" I said—or words to that effect.

Thinking about it afterwards however, it occurred to me that I do have a small semi-scientific story which may be of passing historic interest. It concerns an instance in which using the concept of maximum entropy production, while not of any immediate practical value in itself (the all-too-normal situation with MEP), was at least a catalyst for investigating something of practical interest in another field.

The story concerns an attempt to use MEP as a global constraint for a climate model in which water-vapour feedback was handled with highly simplified relations derived from the results of general circulation models (GCMs). The work was described in Paltridge *et al.* [1], and among other things showed that the MEP-constrained model, when water-vapour feedback was included, gave fairly unlikely changes in cloud cover when the concentration of atmospheric carbon dioxide was doubled. To be specific, the positive water-vapour feedback of the typical GCM, when translated into the framework of the MEP climate model, ensured a reduction in cloud cover of nearly ten percentage units.

Actually the reduction in cloud was mainly the result of the model maintaining energy balance when the CO_2-induced rise of surface temperature was amplified by water vapour feedback. In that context, the MEP constraint (which in the model was the determinant of the latitudinal *distribution* of cloud and surface temperature rather than of the global averages) was not all that relevant. However there was an annoying practical consideration. The large water vapour feedback made the numerical process of determining the position of the maximum of global entropy production quite difficult because it pushed the calculations fairly close to instability.

Suffice it to say that a lot of time was wasted looking at the MEP side of the problem. Eventually, since MEP seemed to be creating more difficulties than was reasonable, it occurred to us that maybe the problem lay elsewhere. We began to wonder about the robustness of the holy writ handed down from the mountain of GCMs to the effect that water vapour feedback is strongly positive.

At this point, maximum entropy production disappears from the story. We became involved instead in the question of water vapour feedback, and the present author for one learnt a few things which he should have known a long time ago.

The first was that water vapour feedback on climatic time-scales is determined almost entirely by the response of water vapour in the middle and upper levels of the troposphere. If the concentration at those levels increases with time in concert with surface temperature T, then the feedback is positive. If it decreases, then the feedback is negative. It is virtually irrelevant whether the total water vapour amount in the atmosphere (which is dominated by the very large concentrations in the lowest levels near the ground) increases or decreases.

The second was that, for various reasons that are fairly obscure to most of us, GCMs tend to maintain at any given height a remarkably constant relative humidity as surface and atmospheric temperatures increase. This means that the *specific* humidity in the middle and upper troposphere of GCMs increases with time and with temperature as the CO_2 concentration increases, and as a consequence, water vapour feedback in GCMs is large and positive.

The third was that, if one is foolish enough to look at the global radiosonde humidity information that is fed into the NCEP (National Centres for Environmental Protection) data assimilation model and regurgitated as a coherent set of humidity data on a regular grid, one finds that middle and upper tropospheric humidity appears to have decreased fairly steadily over the last few decades. "Appears" is the operative word here, since of course there are lots of known and potential errors with balloon-borne radiosonde data. Satellite humidity data have their own peculiar problems, and also cannot be trusted to reveal long-term trends. For what it is worth, and for at least some latitudes, satellite information suggests an increasing humidity in the middle and upper levels.

The bottom line is that one of the two sources of observational data on upper level humidity suggests that, at face value, water vapour feedback over climatic time scales might be negative. The practical outcome of which ought to be, because it bears on a matter of fundamental concern to climate change, that past radiosonde data be re-examined in considerable depth. The object would be to establish what, if any, aspects of the face-value humidity trends can be retrieved from the "noise" of the potential measurement errors.

Purely as an aside, the path to publication of the face-value trends of the NCEP humidity data [2] was fraught with considerable difficulty. One of the reviewers became almost hysterical about the dangers associated with reporting this sort of result in a refereed journal. Which is an interesting reaction when one comes to think about it.

In the present context, the story is merely a poor example of a case where working with MEP might turn out to have been practically useful—albeit in a rather second-hand and roundabout way.

The concept of maximum entropy production is rather difficult to visualise as a tool for *understanding* the behaviour of complex non-linear systems. It may be a good description of an actual result—for instance that a complex system seems automatically to select a particular steady state from a spectrum of potential steady states—but it does not of itself provide an obvious reason why such selection should take place. Dewar suggested a statistical mechanical explanation [3,4], but even if a version of that explanation is ultimately proved correct and accepted, it scarcely falls into the class of "obvious reasons".

This bears on the practical usefulness of an MEP principle. It is likely that MEP will remain entirely in the realm of academic discussion until someone—probably someone from outside the field—identifies a complex system whose behaviour cannot be predicted by any means other than application of MEP. It is also likely that the "someone" will need to visualise fairly easily the physics of what is going on so that the result can be understood without relying entirely on some esoteric mathematical proof.

Now it has always seemed to the present author, for no good reason other than it would be rather neat, that the ability of a system to dissipate energy and to produce entropy "ought to be" (?) some increasing function of the system's structural complexity. (Actually the idea is not original, and has appeared as a qualitative suggestion in a number of places. Two examples occur in references [5,6]). In thermodynamic terms, it would be nice if there were some general rule to the effect that, in any given complex system, the steady state which produces entropy at the maximum rate would at the same time be the steady state of maximum order and minimum entropy. The reason it would be "nice" is that the typical scientist (or non-scientist for that matter) probably has a somewhat better physical picture of the meaning of "order" than of "rate of entropy production". And if that is true, perhaps he or she could visualise rather more easily why there should be a maximum in "order" rather than why there should be a maximum in entropy production.

All of which is no more than wishful thinking. On the other hand, it is encouraging that Kleidon recently produced a simple analysis of a two-box thermodynamic steady-state system which indeed shows that the state of maximum entropy production corresponds (for that system) to its state of minimum entropy [7].

So it is perhaps worthwhile to make a formal recommendation to the effect that research should be encouraged to establish whether Kleidon's result can be generalised to cover all complex systems for which a principle of maximum entropy production may be relevant.

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