3 Stumbling into the MEP Racket: An Historical Perspective

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Summary. An historical tale is told of the author's involvement with research on the possible application of a principle of maximum entropy production to simulation of the Earth's climate system. The tale discusses a number of reasons why the principle took so long – and indeed is still taking so long – to become generally acceptable and reasonably respectable.

From my point of view the whole business of a principle of maximum rate of entropy production (MEP) emerged as a consequence of abysmal ignorance of much of the basic physics one is supposed to learn as an undergraduate. I never did understand thermodynamics – or indeed the purpose of it. It was taught to me in the old classic way as a never-ending stream of partial differentials which, at least at the time, didn't mean all that much. They meant even less when thrown together with a raft of pistons, cylinders and strangely behaving gases. As for things like entropy, enthalpy, Maxwell demons and Gibbs' functions, they were all quickly consigned to the scrap heap of memory as soon as the relevant course was finished. I suspect the words 'irreversible thermodynamics' never passed the lips of our lecturer. Mind you, I can remember being impressed with the Second Law, despite the fact no-one seemed quite to know what practical use it might be. The lack of an 'equals' sign anywhere in its exposition seemed to consign it to the realm of qualitative beauty rather than quantitative value. Had anyone ever made a dollar out of it?

So you may picture in the late 1960's a rather sub-standard physicist randomly tossed into the field of atmospheric physics and meteorology. He was basically an experimentalist, and thereby hoped to avoid displaying ignorance of the more esoteric and difficult areas of theoretical physics and applied mathematics. Perhaps 'randomly tossed' is putting it a bit high. In fact he actively chose the career because he had a vague feeling that running around in aeroplanes measuring things with weird instruments would be a lot of fun. And in this (about aeroplanes being fun) he was right. Where things went a bit pear shaped was when he discovered that atmospheric physics was, and still is, populated by extremely bright people working on some of the most fundamental problems of physics. To take just one example, one could refer to Von Karman who said somewhere – 'There are two great unexplained mysteries in our understanding of the universe. One is the nature of a

A. Kleidon and R.D. Lorenz (Eds.): Non-equil. Thermodyn., UCS 2, pp. 33–40, 2005.

unified generalised theory to explain both gravitation and electromagnetism. The other is an understanding of the nature of turbulence. After I die, I expect God to clarify the general field theory for me. I have no such hope for turbulence.' And as for pure meteorology, it turned out to be absolutely full of those wretched partial differentials, pistons, cylinders and strangely behaving gases.

Somewhere about that time also was the adolescence of the great new game of numerical modeling. It was (and of course still is) a gentlemanly activity, and many a scientist found himself or herself believing that numerical modeling was the *only* way to solve some of the great problems of the world. And after a while the exercise of pure simulation became an end in itself. The classic example was the modeling of climate, where it was necessary to introduce lots of tunable parameters so as to arrive at answers bearing at least some semblance of reality. The disease is still rampant today, although fairly well hidden and not much spoken of in polite society. The reader might try sometime asking a numerical climate modeler just how many tunable parameters there are in his latest model. He (the reader) will find there are apparently lots of reasons why such a question is ridiculous, or if not ridiculous then irrelevant, and if not irrelevant then unimportant. Certainly he will come away having been made to feel quite foolish and inadequate.

In fact the climate modeling business in the early seventies, although very impressive, did smack a little of describing the overall behaviour of a gas by simultaneously describing the motion of each and every molecule. There are after all some quite nice laws governing the macroscopic behaviour of a gaseous medium. So one could legitimately be rather arrogant and look down the nose on the subject and be rather nasty about it in public. Such an attitude was particularly attractive to someone for whom numerical modeling was another of the disciplines which fell (like thermodynamics?) into the too-hard basket. And it was during one of these looking-down-the-nose periods that the present author read somewhere that the last gasp of the physicist who couldn't solve a particular problem was to cast about for an extremum principle of some kind. What the reading didn't make clear was that any scientist worth his salt would at least have a feeling before he began what sort of extremum principle he was after.

In any event the teller-of-the-tale began a more-or-less random search for an extremum principle which might work with a simple one-and-a-half dimensional energy balance climate model. Putting that in English, he developed a model of the Earth's atmosphere and oceans in which adjacent boxes represented latitude zones (there were ten of them from pole to pole) and each box had a pair of separate sub-boxes which individually represented atmosphere and ocean as shown in Fig. 3.1. There were rather a lot of unknowns left over, even when he had cunningly used a number of tunable parameters to represent things like cloud albedo and cloud height and so on. The left-over unknowns boiled down to the surface temperature T, the cloud cover θ and the sum $LE+H$ of the surface-to-atmosphere latent and sensible heat fluxes of each box, together with the set X of north-south flows of energy between adjacent boxes. He had already woken up to the fact (obvious presumably to everyone else but new to him) that the real problem when trying to model climate is that the Almighty seems to have ensured that there are always more 'unknowns' than there are relevant equations. Funny that! As Von Karman implied, turbulence has a lot to answer for. Anyway, where an extremum principle might get into the act would be as a substitute for the missing relevant equations.

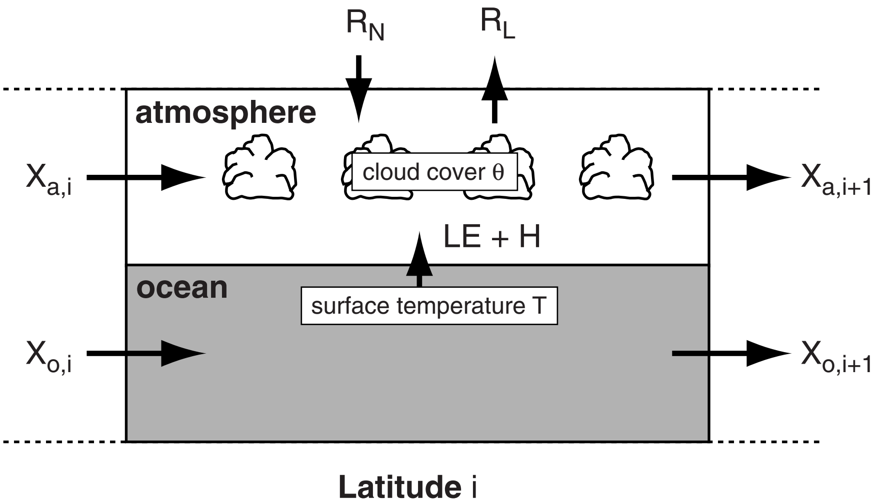

Fig. 3.1. Diagram of a latitude zone or 'box' of atmosphere and ocean with meridional energy fluxes X_o (in the ocean) and X_a (in the atmosphere) across latitudes i and $i+1$. The X of the text is the sum of X_o and X_a. The box has an ocean surface temperature T and an ocean-to-atmosphere non-radiant energy flux $LE{+}H$ of latent (LE) and sensible (H) heat. The fractional cloud cover of the box is θ. R_N and R_L are respectively the net short-wave and net long-wave radiation fluxes (at latitude i) into and out of the top of the box

It has to be admitted that the search involved a bit of cheating right at the beginning because there were only two energy balance equations which could be applied to each latitude zone – i.e., one at the top of the atmosphere and one at the ocean surface. The cheating took the form of a sort of subsidiary extremum principle. It was assumed that, given a particular net horizontal energy flux into a zone, its cloud cover and surface temperature would adopt values such that the vertical flux $LE{+}H$ from surface to atmosphere would be the maximum allowed by the two energy balance equations. There was some slight physical reasoning behind the assumption, but not so much that it would pass the censors. Suffice to say that the assumption gave good answers, so it didn't pay to be too critical.

Then it was simply a matter of looking at all sorts of strange overall parameters which might be made up from the individual variables calculated within the model. Among them were things like global-average surface temperature, average meridional flux, total solar radiation absorbed by the system and so on. In each case the distribution X of north-south energy flows between the boxes was juggled (this with a fancy numerical minimization routine) to see if the parameter had a minimum for a particular set Xp of the distribution X, and if so whether Xp and the associated cloud covers and surface temperatures of the zones looked anything like the real thing.

And so emerged a strange parameter involving the radiant fluxes into and out of the planet. Specifically, it was the sum over all the latitude zones i of the incoming net radiation ($R_N - R_L$ referring to the figure caption) divided by the outgoing infrared radiation R_L – that is, $\Sigma\{(R_{Ni} - R_{Li})/R_{Li}\}$. It worked beautifully. The only trouble was that, as a physical parameter, it didn't seem to mean much. In fact it didn't seem to mean anything at all, and eventually our intrepid investigator had to take the results to one of the old-style meteorologists who had a reputation for knowing what he was talking about. This was one Kevin Spillane, who immediately suggested taking the fourth root of the infrared radiation on the bottom line so that one would at least be dealing with recognizable units involving rate of energy flow divided by a temperature – that is, with units of the rate of entropy exchange. "So?" the author remembers saying. "What is entropy exchange and who cares?" Anyway, after something of a crash course on irreversible thermodynamics, he at last managed to convince himself that, if the results were to be believed, the atmosphere-ocean climate system seems to have adopted a format which *maximizes* the rate of entropy production within the system. The reader may note that it took some considerable time even to understand the reciprocal relation between entropy exchange and entropy production for steady state systems, and that minimization of the one was the equivalent of maximization of the other. To be fair, the physics behind the concept is not immediately obvious until one recognizes that the constraint of energy balance ensures comparison only of potential steady states of the system. The point is discussed again a little later in the paper. The overall entropy of any of these steady states must be constant, so in each case the internal rate of production must be balanced by the net rate of export across the boundary – i.e., out through the top of the atmosphere – via the radiative fluxes. The Second Law ensures that the internal entropy production is positive, so the net outward export is positive, and the net exchange (i.e., net inward flow) is negative because it is simply the outward export measured in the reverse direction. Mathematically, a minimum in the negative exchange has the greatest absolute value, and is the same as the maximum in the positive internal production.

Anyway, the result was ultimately published in a couple of papers (Paltridge 1975, 1978) in the Quarterly Journal of the Royal Meteorological Society. The second of them extended the idea a little, and among other things dealt with a 3-D '400-box' model which allowed calculation of the geographical distribution of cloud, surface temperature and horizontal energy flows in (separately) the atmosphere and the ocean. The journal referees of the time seemed to like the idea, and didn't give too much trouble.

And there matters stood for quite a large number of years. To be sure, a fair number of people addressed the issue in one way or another, and among other things confirmed the basic finding. They also provided a formal background to the analysis of entropy production associated with conversion of solar and thermal radiation from one 'temperature' to another. This was a considerable achievement, but as it turns out was probably fairly irrelevant to the particular issue of *why* the Earth-atmosphere system (or any other system for that matter) should adopt a format of maximum entropy production. Until that question could be answered, the MEP result could not be regarded, and rightly was not regarded, as anything other than a curiosity.

There were a number of things which didn't exactly help. Not the least of these was the rather forced and half-hearted physical explanation of the phenomenon which Paltridge himself propounded in a couple of associated papers (Paltridge 1979, 1981) in the late seventies and early eighties. It scarcely inspired confidence in the overall idea. But quickly setting that aside(!) some of the other unhelpful factors have at least an historical interest.

First, the seventies and early eighties were the great era of the sort of irreversible thermodynamics introduced by Prigogine and his colleagues. One of his theoretical results which had the simplicity to be well known and often quoted (though not perhaps really understood by a lot of people) was a principle of *minimum* entropy production. This was difficult to reconcile with a strange finding concerning maximum entropy production where, apart from anything else, the precise definition of entropy production was a bit loose. It required quite a lot of delving into the subject to appreciate that Prigogine's result applies to linear systems with fixed boundary conditions and (therefore) a single steady state. That single steady state is one of minimum entropy production relative to any non-steady condition to which the system might be pushed. The maximum entropy production concept concerns non-linear systems – so non-linear in fact that they can be thought of as having an infinite set of steady states, and by some magical means are able to select that particular steady state of their set which has the maximum production of entropy (see also Kleidon and Lorenz, this volume). The search for the 'magical means' was avoided by everyone.

Second, even if one can appreciate in principle the concept of a spectrum of potential steady states, it is not so easy to visualise a specific practical mechanism which has that peculiar characteristic. One is asking for a medium where the transfer coefficient (of the flux versus potential difference relation) can adopt any value it likes – a state of affairs which, even in principle, is difficult for any sensible fluid dynamicist to accept. The numerical modelers in particular are used to transfer coefficients which are proportional to some power of the potential difference, but such simple non-linear relations are still a long way from producing multiple possible steady states.

Third, there is no doubt that any result involving the word 'entropy' has a problem right from the beginning. For various rather obscure reasons, 'entropy' is a word that seems to attract the crackpots of the pseudo-scientific societies of the world. Its basic thermodynamic meaning is well enough defined, but its claim to universal application via the second law of thermodynamics is highly attractive to those who are, shall we say, rather more philosophic and hand-waving than is acceptable in the normal circles of the hard sciences. I have seen one of my early mentors pick out a madman in the audience of a scientific discussion simply because he (the madman) used the word 'entropy' in what might otherwise have been a quite sensible question. So one has to be a little careful not to be automatically assigned to the crackpot class when dealing with the subject. Perhaps this sort of thinking explains something of the fact that meteorologists and oceanographers and fluid dynamicists in general are far happier dealing with turbulent dissipation rather than the more general entropy production to which it is related.

And finally, when all is said and done, a global rather than a local constraint may be interesting physics but is not obviously useful in a world dominated by the numerical modeling of climate – that is, where the calculations done at each time step are inherently calculations about local conditions. One is apparently back to the problem with the second law itself – has anyone ever made a buck out of a global constraint?

Over the last little while the concept of maximum entropy production has got something of a new lease of life. More and more fluid-Earth (and indeed general planetary) examples have been proposed as cases where MEP might apply. The examples have provided hope, if not proof, that MEP might be used to bypass the difficulties of handling the specific processes of turbulence. Apropos of which, it is only over those last few years that it has been generally appreciated that the MEP principle, if it applies anywhere, must apply primarily to turbulent media where the necessary number and type of non-linearities can pertain. Certainly, while in the earth-atmosphere context the dominant process of entropy production is associated with the downgrading of solar radiant energy to energy at terrestrial temperatures, that particular process (which is essentially linear) does not contribute directly to the creation of a set of potential steady states. Such a set derives specifically from the various *turbulent* transfer processes in the atmosphere and ocean.

Paltridge (2001) tried again to provide a physical explanation of why a turbulent medium might adopt the particular format associated with MEP. "Tried" is the operative word, since the explanation, while qualitatively acceptable (he supposes) as a physical picture – it is at least more acceptable than his earlier attempts 25 years before – still lacks the final touch of fully quantitative rigour. Basically the picture is of a turbulent medium transferring heat between two boundaries of different temperature maintained by an input of energy from outside the system. The system has an infinite set of possible steady states, each corresponding to a particular time-averaged distribution of the kinetic energy, eddy scale and physical position of the eddies in the medium, and each thereby corresponding to a particular value of transfer coefficient k. The set ranges from very large k (large heat transfer and, as a consequence, small temperature difference between the boundaries) to very small k (small heat transfer and, as a consequence, large temperature difference between the boundaries). The picture makes use of the fact that on short time-scales there are fluctuations of the instantaneous rate of heat transfer away from steady state because of the random hand-over of energy from one scale of eddy to another. There is a drift along the locus of steady states as the system returns towards a new steady state after each fluctuation. It turns out that the net drift due to random fluctuations is towards the middle of the set because the amplitudes of 'upward' and 'downward' fluctuations of heat transfer are different functions of the driving potential (i.e., of the temperature difference). Albeit with an assumption about the broad shapes of the fluctuation dependencies, it can be shown that the net drift is actually towards the steady state which has the maximum rate of thermodynamic dissipation or (and it is a slightly different steady state) towards the maximum rate of entropy production.

Among other things the explanation suggests the possibility that MEP might apply on a sufficiently local scale to be of use as a governing equation for the diffusive fluxes into and out of the grid boxes of the typical numerical climate model.

But the biggest fillip to the business has been Roderick Dewar's recent paper (Dewar 2003; also Dewar, this volume) which seems to provide what amounts to a statistical thermodynamic proof of the MEP concept. As I understand it (and lets face it I don't understand much of it yet – one's basic ignorance hasn't changed much in the last quarter of a century) Dewar has added what might be called a codicil to the second law of thermodynamics. Effectively he seems to have proved that, not only will an isolated system move ultimately to a state of maximum entropy as dictated by the second law, but it will get there as fast as it can. When his paper has been kicked around for a couple of years and is finally accepted by the gurus of theoretical physics, then perhaps we will at last have a basis for people to spend serious time finding applications for MEP. The numerical modelers might at last seize upon its respectability and do something with it (Ito and Kleidon, this volume; Shimokawa and Ozawa, this volume).

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