**Atmospheric
Chemistry
and Physics**
Discussions

Interactive comment on "Definition of the energy norm induced metric and its application on the atmosphere" *by* F. Wan and T.-Y. Koh

This paper discusses the definition of the energy metric (and its associated norm) in fluid systems of increasing complexity, from a 2-dimensional barotropic model to a fully compressible, dry, adiabatic and inviscid atmosphere in different vertical coordinates. It is well written and provides a nice overview on how the energy metric could be defined in these different systems. The key question is whether we learn anything new, anything that we did not know before, from reading it.

It seems to me that its main 'new' conclusion is that the energy metric usually considered in data-assimilation and predictability studies to identify singular vectors, perturbations with fastest finite-time growth rate, has a wrong coefficient in front of the temperature component (of the state vector). The authors argue that the coefficient should be multiplied by a factor $\frac{1}{2}$, but they do not present enough evidence that this

should actually be the case.

The energy metric presented in this work is deduced from Tremberth (1997)'s energy norm expression for full fields, when applied to perturbation fields. The authors say that this expression has coefficients in front of the potential energy (say temperature) and surface pressure that are different from the coefficients deduced by Talagrand (1981) for perturbation fields. By contrast, Talagrand (1981) expression of the energy metric for perturbations was deduced from the shallow-water equations that he introduces in section 5 of his paper. Is one the two formulations 'wrong'? Or are they both consistent with their equations and assumptions, and thus provide two possible choices?

If the authors want the paper to be considered for publication, I suggest that they add an appendix where they compare in details the two formulations. They should start from Talagrand (1981) and Tremberth (1997) model equations, highlight their similarities and differences, discuss the differences in their assumptions (e.g. of reference temperature and reference surface pressure when defining potential temperature), and deduce the energy metric for perturbation fields in both cases. In this way they could trace back the origin of the $\frac{1}{2}$ difference, and argue with more evidence whether one is wrong. As the paper reads now, it is not clear why the two formulations are different.

Still on this issue of the energy metric formulation, it is worth also reminding what Ehrendorfer and Tribbia (1995) said on their choice of total energy metric (which is based on Talagrand (1981)):

"This norm is often called the total energy (TE) norm, because in a non-discretized model with periodic lateral boundary conditions, linearized about a state of rest with spatially invariant reference temperature Tr and surface pressure ps, it is the conserved perturbation kinetic plus available potential energy of the linearized system. . . . The discretized form of (4.1) is not a conserved quantity in the linearized MAMS1. . . . This implies that (4.1) should not be interpreted as an actual (perturbation) energy (indeed, it may be far from it), but instead only as a simple and convenient quadratic norm that

looks like an energy." (see their paper, Section 4, pg. 3479-3481).

This comment highlighted already in 1995 the difficulty in defining an energy metric that is conserved in a discretized system, and the fact that there is a certain degree of arbitrariness in defining it for complex, primitive equation discretized models of the atmosphere. As I have already mentioned above, can it be that both Talagrand (1981) and their formulations are possible? Or is one formulation clearly wrong?

Now let's suppose that there is indeed 'arbitrariness' (possibly an error) in the definition of the energy metric weight in front of the temperature component, and that both formulations are possible.

We know from the published literature that results are sensitive to the choice of the metric. This was clearly stated, e.g., by Palmer et al (1998):

"In this paper, energy, enstrophy, and streamfunction variance were all considered as candidate metrics for singular vector calculations. From two independent sets of calculations based on analyses, and short-range forecast data, it was shown that of these three choices, energy is the most appropriate metric for the predictability problem. ... However, the correct metric is dependent on the purpose for making the targeted observations (to study precursor developments or to improve forecast initial conditions). It is argued that for predictability studies, where both the dynamical instability properties of the system and the specification of the operational observing network and associated data assimilation system are important, the appropriate metric will differ from that appropriate to a pure geophysical fluid dynamics (GFD) problem."(see their conclusions, pg 650-651)

It would be interesting to know how much would data-assimilation or predictability results change by a modification of the temperature weight by a factor of $\frac{1}{2}$.

My second suggestion to the authors, if they want the paper to be considered for publication, is that, once they have documented the origin of the differences in the

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energy metric formulation, that they document the impact of changing the factor on data-assimilation or singular vectors.

Finally, I have also a couple of minor comments. In Section 2.2: - in the second paragraph, correct 'Eq. (5)' into 'Eq. (4)'; - in the third paragraph, in the (..,..,..) bracket, correct 'h v' into 'sqrt(h) v'

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