

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

**Anonymous Referee #1**

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I am not sure what to make of this paper. It is unfortunate that the usual multi-field norm used in singular vector determinations and observation impacts for numerical weather prediction studies is called the "energy norm." That name implies that it is rather fundamental, as energy is, although it is not the true energy invariant for the applied models and is not fundamental to its applications.

As the authors note, the usual energy norm formulation expresses an energy only in a greatly simplified model derived by linearizing about a rather unrealistic state that has homogeneous temperature and surface pressure, flat topography, and zero wind speed. More fundamentally, however, is why a norm based on such energy is the desired one, especially since the norm is applied to differences between model realizations or forecast errors for which the invariant does not apply. In the absence of

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any strongly supported argument otherwise, the choice of this norm for the problems to which it is applied appears quite unmotivated.

Consider an expression of total (kinetic plus some form of potential) energy that is an invariant under adiabatic conditions. Its variations in time then reveal aspects of diabatic processes, but otherwise express little about the dynamics, although looking at its separate kinetic and potential contributions may be only slightly more informative. Why then, should the same total energy metric applied to fields of forecast differences or errors be deemed so much more informative than other possible metrics?

For the above reasons, the properly interpreted meaning of the usual energy norm is that it is simply a convenient sum of integrated, weighted, squared forecast differences or errors of wind, temperature, and pressure fields. The weights must account for the different units of the fields considered so that a single measure is determined. For the parameter values applied to the usual energy norm, a 1 m/s difference in the zonal wind contributes approximately 3 times a 1 K difference in temperature. For someone who wants a quadratic measure of errors or differences that considers winds, temperature, and pressure simultaneously, the weights expressed in the energy norm appear acceptable as long as using its implied weights (e.g., the approximate value of 3 applied to the temperature contribution) is acceptable. In this case the energy norm is simply a convenient combination of quadratic metrics applied to diverse fields although it has little to do with "energy."

For singular vectors determined with regard to the usual energy norm, the wind field contributes more than the temperature field to the value of the norm at forecast day 1. If the authors norm (15) is used, the temperature field would contribute even less given that its weight is reduced by a factor of 2. I do not think this is desirable. The issue is not what looks more like energy. Rather, it is what forecast aspects are of interest and does the metric so skew the contributions by different fields that it behaves as though some desired fields are effectively excluded.

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One aspect of the usual energy norm that the authors do not address is that it is actually derived (from the overly-simple linear model) as the kinetic (KE) plus available potential energy (APE) rather than kinetic plus potential energy (PE). There is a big difference in the expressions for APE and PE (see Lorenz, E. N., 1955 *Tellus* 157-167 Available potential energy and the maintenance of the general circulation, and Lorenz, E. N., 1960 *Tellus* 364-373 Energy and numerical weather prediction). Might differences in approximations to APE and PE explain the difference by a factor of 2 between temperature contributions for the usual energy norm and the expressions the authors derive? If so, one for APE seems more appropriate if an energy norm is indeed relevant in this context.

The expression for APE that Lorenz derives for even a simple 2-level model is highly nonlinear. Perhaps a quadratic approximation to Lorenz's expression can be used for some applications, in which case it may look more like the term in the usual energy norm. Even so, it is not obvious that a more rigorously derived quadratic approximation to APE would be any more informative than the usual energy norm for the applications to which the latter is normally applied. In fact, the simplicity of the usual norm may arguably render it more desirable.

The expression " $h\nu$ " on line 18, page 3738, should be " $\sqrt{h}\nu$ ."

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