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3rd of February, 2015

Prof. Peter Haynes
Co-Editor
Atmospheric Physics and Chemistry

University of Cambridge
Wilberforce Road
Cambridge CB3 0WA
United Kingdom

Dear Prof. Haynes,

Re: Submission of manuscript no. acp-2014-9 after minor revision

We are submitting the revised manuscript with the new title “Theory of the norm induced metric in atmospheric dynamics” by Tieh-Yong Koh and Fang Wan, for publication in Atmospheric Chemistry and Physics.

We are appreciative for the patience the editor and the reviewers have shown us in allowing us to further polish the manuscript. In response to the three comments by Reviewer 1, we have made a point-by-point reply and document the one change to the manuscript in red. Reviewer 2 did not make any further comments.

My coauthor and I would further like to thank you for your facilitation and advice. We looking forward to hearing from you.

Yours sincerely,
Tieh-Yong Koh

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2nd of February, 2015

Dear Reviewer 1,

The authors would first like to thank you once again for the helpful comments. We have further thought about them and what follows is our response to the matters raised.

In the following response, the reviewer's original comments are in **bold and blue** and within quotation marks whereas our response are in normal black font. Any reference to sections, pages, lines and equations refer to the further revised manuscript. The latest revisions in the manuscript are in red.

"1. Contrary to what is stated in the authors' manuscript, the expression involving perturbation T in the norm described by Ehrendorfer and Errico (1995) and also by Talagrand (1981) is indeed the available potential energy (APE) (plus a small portion of unavailable energy as shown later by Errico (2000) in a primitive-equation model linearized about an isothermal, flat surface, resting atmosphere, as carefully stated in that first reference. For nonlinear models, it can also be considered as an approximation to APE (albeit, a rather crude one) if the horizontal mean of the temperature lapse rate is neglected along with variations of surface pressure when describing mass within a column of air. These are poorer approximations than made by the authors of this paper, but otherwise of similar character. So, the total energy norm is indeed motivated by an expression approximating available potential energy. <u>For its applications, in fact, it does not matter how good an approximation it may be.</u> This should be made clear by the authors so that they do not contribute further to the confusion about this norm that already exists."

We agree mostly with the reviewer's comments above: the temperature perturbation term in Talagrand's expression E_{T81} is equivalent to Lorenz's APE for an isothermal atmospheric reference state (which means $dT_0/dz = 0$, or equivalently, $d(\ln \theta_0)/d(\ln p)$ = constant). However, in the same vein of thought as the reviewer's, one would also describe absolute vorticity as Ertel potential vorticity in a primitive-equation atmosphere under the different but equally crude approximation that isentropic density of the basic state is constant

($d\theta_0/dp$ = constant). Now, that would be confusing indeed... So likewise, it would be confusing to claim that E_{T81} is well-motivated for applications in the real atmosphere no matter how crude the approximation of an isothermal atmosphere is (cf. underlined remark above). Thus, we added the second paragraph of Section 1.2 (page 5) and deleted the last line of Appendix A1. The formula for Lorenz's APE has correspondingly been moved forward from Section 6.1 to Section 1.2 as equation (2) to clarify the spoken relation.

"2. In all applications of the total energy norm that I have seen before this paper, the norm has been used to measure either differences between pairs of forecasts or between forecasts and corresponding verifications. Such measures will typically grow due to chaos, whether or not the norm is a true invariant in the parent model. The appeal to it as some approximation to a true invariant truly refers only to the motivation for its relative weights of contributions by wind and mass variables. Effectively there is otherwise <u>nothing special about the formulation of that norm</u>. Indeed, its <u>utility should be judged on</u> whether it adequately describes characteristics of error that are of interest. An example of a simple norm that may not be adequate is the "moist total energy norm" using a scaling parameter epsilon=1 derived heuristically similarly by Ehrendorfer et al. (1999 J. Atmos. Sciences, 1627-1648): It unfortunately weights the contribution by the moisture variable so much that it can obscure measures of the dynamics. For that reason, although motivating a norm by energy conservation may be appealing, it should be regarded as an insufficient reason."

When one excludes the principal advantages of using a new formulation, it is not surprising that one arrives at " nothing special about the formulation of that norm" (underlined above). But understood correctly, the same statement by the reviewer also recognizes the principal advantage of using the new metric as it removes the arbitrariness of the relative weights of kinetic and enthalpy variables. The example from Ehrendorfer et al. (1999 J. Atmos. Sciences, 1627-1648) further substantiates the undesirable consequence of leaving such arbitrariness (in the value of epsilon in this case) unchecked. Now, whether "utility should be judged on" one set of criteria or another is arguable and depends a lot on the user's purpose. We agree that motivating a norm by energy conservation alone is not going to be sufficient for *all* cases. But such a motivation is theoretically appealing and has its practical uses in some cases. As we already highlighted and discussed these issues extensively in Section 7, we do not see the need to make further revisions in response to this comment but would like to thank the reviewer for the further discussion.

"3. Reducing descriptions of forecast difference or errors to a single number, as accomplished in applications of the total energy norm, for example, will always be problematic. The issue is that for most problems we are not actually interested in a single metric. Two error fields or forecast difference fields may have the same value of the norm, but have considerably different characters. As an example, we generally will qualitatively distinguish between a case where all the error is extremely large but at a single point compared with one where the error is very small but spread over the entire domain. The complete characterization becomes even more obscured when weights are applied to differing fields considered together. It is therefore generally best to first consider what desired character is to be measured and then to consider expressions of energy, if still appropriate."

We agree with the above comment. As no claim was made in the manuscript that it is always possible or advantageous to reduce forecast differences or errors to a single number, no changes to the manuscript seem necessary in response to this point. The spirit of the reviewer's concluding sentence is already captured in the third last sentence of Section 7, the concluding section of the manuscript.

Yours faithfully,

Tieh-Yong Koh