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Interactive comment on "Dependence of the critical Richardson number on the temperature gradient in the mesosphere" *by* Michael N. Vlasov and Michael C. Kelley

Anonymous Referee #2

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In this manuscript the authors study the dependence of the critical Richardson number on the temperature gradient by rewriting the buoyancy frequency and wind shear terms to be dependent solely on the temperature. They evaluate the critical Richardson number for isothermal atmosphere and for temperature decreasing with altitude. At this stage, the level of the study is poor. It has to be completely rewritten and undergo a new round of reviews to be assessed for publication. Regarding the title, I completely do not get the relationship to the mesosphere, besides the fact that the authors also consider situations with negative vertical gradient of temperature. Ref#1 also raised this issue and in the AC comment the authors contradict themselves by arguing (on three full pages) that the other studies (like Obukhov (1971)) are not appli-

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Discussion paper

cable for the mesosphere, but then surprisingly in the final paragraph they write:" ...Ric dependence.....is obtained by us without using density, neutral composition, and other parameters of the mesosphere.." With some weird remark that the applicability is linked with the uniform turbulence. Btw. the study of Obukhov (1971) gives a rigorous summary of the Ri and Ric dependence on the temperature gradient and the author need to explicitly cite this study and show, where they give superior scientific information. The connection of the study under review with the mesosphere is demonstrated by the figures, where the x axis shows height about 90 km. But, this is just due to the author arbitrariness connected probably with the choice of temperature values they used for evaluations.

MAJOR CONCERN: A)Most importantly, I have serious concern about the validity of the methodology and flawlessness of the analytical derivations in this paper: The crucial point of this study is that the authors assume adiabatic expansion. While this can be a good assumption for the GW induced perturbations, it is completely irrelevant for the background, where e.q. the solar tidesgovern a significant part of the mesospheric variability. Also, the authors use this assumption to connect the vertical gradient of full (background + disturbed) density distribution to the full temperature and its gradient and wind shear (Eqs. 6,7,8,9, 10). Also in the light of tides, this assumption crucial for the paper needs to be properly justified, ideally by referencing observational studies. But more than just general doubts about the validity of this assumption, the authors make errors also in analytical description, where in eq. 8, which shows partial derivative of T with altitude they refer to it as (P4L81) "temperature gradient in the parcel (sic) with upward motion and adiabatic expansion" - but for this, total derivative would have to be shown. Most importantly, on their way from eg. 6 to 10 they use in P4L80 an equation for Ri based on different assumption (they don't tell anything about this formula, which is crucial) and then they consider this Ri (general?) to be equal to the Ri in eq. 7 (adiabatic expansion) for deriving eq. (10). A similar situation takes place in section 3, where they give equation 13b (P6L110) without properly discussing how they derived this equation and the underlying assumptions (polytropic atmosphere?). This formula (13b) and the formula for wind shear (eq. 10) are the crucial parts of the paper, because every other result then presented is only a trivial evaluation of Ri based on those formulas. The authors need to carefully rewrite all of their analytical derivations, distinguish properly between local and total derivatives, list the assumptions made and ensure consistency between the assumptions and also distinguish in their formulas between constants and functions of altitude (f(z)). Without this it doesn't make sense to discuss any results given later in the text (poor evaluation of the derived formulas), because my personal opinion (the authors are welcomed to prove otherwise) is that the results are dominated by flaws in their analytical construct.

B) Language: Non-scientific language is used frequently, with weird phrases like: we could find just one paper..or the authors write that some study is wrong, but do not prove it. Just to list: What is the acceleration in wind shear? P5L92 Does wind shear really induce vertical accelerations? (no, you have to replace the word induce by e.g. support) Page 3, L 67 not wind shear nor stability are forces.. Those were the most striking ones. I am not listing all the typos made in the manuscript because I expect major changes before it can be assessed for publication.

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