Comment on wcd-2021-31
Anonymous Referee #2

Referee comment on "A vorticity-and-stability diagram as a means to study potential vorticity nonconservation" by Gabriel Vollenweider et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-31-RC2, 2021

Synopsis:

This paper takes a potential vorticity (PV) perspective on atmospheric dynamics. Given that PV is broadly speaking a product of static stability and absolute vorticity, a material rate of change of PV can occur through non-conservative terms in either the heat equation or the momentum equation or both. However, it is well possible that both static stability and vorticity suffer a non-zero material rate of change, but PV is, at the same time, conserved: this is exactly what happens in the event of purely conservative flow with vortex stretching.

Major concern:

The authors analyse the situation with the help of a novel diagram that represents the motion of an air parcel in a two-dimensional phase space spanned by absolute vorticity and static stability. This is an interesting approach. At the same time, I think that the paper does not live up to the expectations, and it seems to me that essential aspects of "adjustment to balance" need to be discussed more lucidly in order to make this a useful contribution to the literature. In particular, it seems to me that there is a fundamental flaw in the argument. Consider the following thought experiment which was originally suggested by (I believe) M. McIntyre and/or B. Hoskins quite some time ago (sorry, I cannot find the respective reference). Assume that initially a parcel is instantaneously being subject to differential heating such that its static stability increases; the point in phase space would move straight upward. The ensuing adjustment process is thought to be adiabatic such that the parcel moves along one of the red hyperbolas. During this adjustment process, part of the original material increase in static stability is reduced and converted into a material increase of absolute vorticity (such that PV is conserved during the adjustment process). Where exactly the point ends on the diagram during the adjustment process essentially depends on the ratio of static to inertial stability and the aspect ratio of the heating (see, e.g., the work of Eliassen 1952). Of course, in reality the (initial) diabatic change and the (ensuing) adjustment process cannot be separated from
each other, rather they occur more or less simultaneously. In addition, the occurrence or absence of inertio-gravity waves depends on the time scale during which the initial non-conservative process is applied.

Now consider a second thought experiment where initially there is only an (impulsive) non-conservative material tendency on absolute vorticity, followed by the adiabatic adjustment process. Both thought experiments may lead to the same end point in the phase space diagram. Thus, considering only the change of the point in phase space from the initial to the end state does not really tell us anything about the nature of the non-conservative processes – they may be diabatic (non-conservative heat equation), friction (non-conservative momentum equation), or a mixture of both.

For this reason, I cannot follow the basic argument that underlies the reasoning of this paper. The argument first occurs on line 110: "... PV changes in regions where grey hyperbolas are oriented more vertically .... tend to be driven by changes in static stability." Not accounting for the problem that I am not sure whether “changes in static stability” here are meant to be conservative or non-conservative, I think that any such statement cannot be made based on the trajectory of the parcel on the diagram alone.

In the end it does not become clear to me what we have really learned from the analysis using this novel phase diagram. As far as I understand the text, the authors themselves are not very clear about that, and this materializes in the fact that the abstract is very long and very detailed. If one has so many results to report, this raises the suspicion in me that there is not really any true result.

Further issues:

I had a problem with this manuscript in that I could sometimes not really evaluate the validity of individual statements because I did not fully understand them. For instance, one should very carefully distinguish between (1) observed (material) tendencies in vorticity and static stability (which may be due to either conservative or non-conservative processes) and (2) non-conservative (material) tendencies in vorticity and static stability (which could be obtained by analysing the corresponding non-conservative terms in the momentum and heat equation (although the authors consider this to be beyond the scope of the paper).

Another important concern of mine are the many occurrences of formulations (A “drives” B) that suggest a direction of causality where (as far as I can tell) the authors do not provide any prove of such causality. I suggest to simply replace the word “drive” or “driven” by a more appropriate word, and often this more appropriate formulation would simply be “A is associated with B”. To give an example: I could not follow your interpretation of the diagram in Fig 3d: what do you mean when you say that a PV-ver change is “driven” by ..., and what does this mean? You should be more explicit here. Especially it is not clear to me whether a “change” in stability or vorticity is meant to be
conservative or non-conservative.

PV-ver is a strange variable. What do we know about it? It is not necessarily materially conserved for conservative flow. This is dangerous, since the impact of non-conservative processes is at the heart of your analysis. A few lines later (and in the remainder of the text) PV-ver and PV are essentially treated as synonymous....

I provide an annotated manuscript in which I point to several issues which are partly summarized above, plus some further issues (e.g., with terminology such as the use of the word “diabatic”, “component of a scalar”, etc.).

Reference:

Eliassen, A. 1952. Slow thermally or frictionally controlled meridional circulation in a circular vortex. Astrophys. Norv. 5, No 2, 19–60.

Please also note the supplement to this comment: https://wcd.copernicus.org/preprints/wcd-2021-31/wcd-2021-31-RC2-supplement.pdf