

Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2021-31

Anonymous Referee #1

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-RC1>, 2021

This paper presents a new construct, the 'vorticity-and-stability' diagram as a tool to provide better understanding of Lagrangian changes in potential vorticity (PV), due to nonconservative physical processes such as diabatic heating or turbulent transport of heat and momentum.

The idea is to consider an air parcel (or the mean of an ensemble of air parcels) over some period and to display the evolution in a two dimensional space, with the coordinates being two factors, one absolute vorticity and the other static stability, that multiplied together give the PV. The argument of the paper is that the new construct is 'a means to study and identify periods of stability and identify periods of stability- and vorticity-driven changes in PV'.

One certainly cannot dispute the usefulness of studies that look in detail at the processes that change PV, e.g. in forecast models, as are exploited in the later part of this paper. But my reservation about this paper is over whether or not it provides a genuine advantage for such studies. The particular shortcoming of the approach proposed here is that the diagram does not by itself provide any information on the physics operating.

Using a highly abbreviated notation, let the PV be Q , the absolute vorticity be Z and the static stability be S . Using dQ to denote Lagrangian change in Q , for example, we have

$$dQ = S dZ + Z dS$$

Now suppose that $dZ = dZ_P + dZ_D$ where the first term on the right-hand side is due to non-conservative physics and the second is due to conservative dynamics, with corresponding notation for dS . Then $S dZ_D + Z dS_D = 0$ because there is no change in

Q is conserved under the effects of conservative dynamics alone.

The diagram gives us information about dZ and dS (i.e. two pieces of information). Even though we have the constraint $S dZ_D + Z dS_D = 0$ we cannot determine dZ_P and dQ_P separately -- so whether or not there are non-conservative diabatic processes (acting on temperatures) or non-conservative mechanical processes (acting on velocities) cannot be determined.

To me then, it seems that the behaviour seen in the diagram can, of course, be explained in terms of the physical processes acting if those are known, but the information given in the diagram is NOT sufficient to determine what those processes are (i.e. how they are partitioned between diabatic or mechanical in sense used above). In your case studies described in Sections 3.2 and 3.3 you have (and present) the detailed information about how different physical processes contribute to PV changes -- how does the use of the diagram add anything?

Therefore I am skeptical that these diagrams add anything genuinely useful to the analysis of PV and its evolution in weather systems.

I have made some further detailed comments on the paper below.

Detailed comments:

Abstract: very long -- seems to miss the point of an abstract which is to provide a brief summary of the aim, methodology and findings of the paper. In fact the text in general is overlong -- there is a lot of background material.

l10: 'hyperbolic' -- term will be meaningless to reader without explanation

l61: "'latent vorticity' generation' -- first time I had come across this term -- which essentially seems to mean forcing of PV by diabatic processes (with the 'direct' effect, so to speak, on the temperature/stability) together with the very familiar principle that the partitioning of PV between relative/absolute vorticity and stability can change through purely reversible conservative processes. The term seems to be very rarely used and, with all respect to Chagnon and Gray (2009), I'm not convinced that it helps general understanding to perpetuate it.

l62: 'adjusts to a new balanced state in the process of hydrostatic-geostrophic adjustment

... during which inertia, gravity and sound waves radiate away from the heating perturbation' -- there is a question -- perhaps it is a matter of taste -- about whether it is appropriate to describe evolution of a balanced flow as a continuous process of geostrophic adjustment. One subtlety is that the amount of emitted wave activity is determined not just by the difference between the two states A and B, say, at different times, but by the time that elapses between A and B (see Vanneste 2013). There are some advantages to restricting the term geostrophic adjustment to an initial value problem or a problem with 'impulsive' forcing. (But as I have noted -- this is partly a matter of taste -- I'm not insisting on a change.)

l86: 'moist diabatic processes' -- not all processes that affect PV, even in the troposphere, are moist.

Figure 1: My understanding is that the colours of the dots here are not providing 'extra' information -- they are simply displaying information that could be deduced from the diagram -- since (using the notation I have introduced above) what you are indicating is $|S dZ / Z dS|$ -- which can be deduced from the position in the diagram and the slope of the curve. (This is not a criticism of the use of the colours -- but I think it is important to be clear on what is 'new' information and what is not.

l99: 'vertical component' -- I realise that taking account only of the vertical component of absolute vorticity is a useful simplification, and I don't have any particular problem with that, but I do think that the term 'vertical component of PV', which you use subsequently at various points in the paper, is a unfortunate. PV is a scalar, so it doesn't have a vertical component in the sense that absolute vorticity, as a vector, has a vertical component. Your terminology muddles use of 'component' with respect to a vector, with the more general use of 'component' as meaning 'part of'. It is not a serious problem, but it is not very elegant or precise.

Figure 3 caption: 'vorticity'.

l229-31: goes back to my earlier comment re 'geostrophic adjustment' -- evolution of the balanced state does not require substantial emission of gravity waves -- it may or may not. A better statement in my view would be something like 'the vorticity decrease occurs as part of the evolution of the balanced state (under conservative dynamics)'. I'm not convinced that the 'geostrophic adjustment' sentence is needed. ', which seems unlikely' could simply be added to the previous sentence. Certainly 'desired' is not the correct word to use.

l339: 'In the absence of diabatic processes, this vorticity reduction and stability increase (i.e, column shrinking) would occur in tandem to conserve PV. However, the lesson learned from the vorticity-and-stability diagram is that it seems as if the large-scale divergence drives a vorticity reduction, but the diabatic and adiabatic influences on static stability are engaged in a tug-of-war, such that PV is not conserved.' The first sentence, of

course, is simply describing PV conserving dynamics, in the absence of, say, diabatic processes. If diabatic processes act then there will, unless the diabatic forcing term in the PV equation is zero, be a change in the PV. Some of this change will appear in vorticity, some will appear in static stability -- that is all well known and it depends on non-local effects -- it can't be determined simply from what happens in a single air parcel. I don't really see how the diagram is helping -- apart from showing that the two quantities change -- where does one go from that?

VANNESTE, J. 2013 Balance and spontaneous wave generation in geophysical flows. *Annu. Rev. Fluid Mech.* 45, 147–172.