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**The importance of diabatic processes for the dynamics of synoptic-scale extratropical weather systems—a review**

by Heini Wernli and Suzanne L. Gray

***Replies to the reviewers' comments (2nd round)***

We thank the three reviewers who looked again at our paper and the revisions. Below we address the minor comments from reviewers 4 and 5. The reviewers' comments are in black and our replies in blue.

## Reviewer 4

Recommendation: Minor revisions

General Comments:
The authors state that their interpretation "reflects the fact that the balanced flow is determined by a quantity we call PV (and suitable boundary conditions). The omega equation then helps to obtain information about vertical motion." This is ok. My point was that all non-rotating components as well as all rotating components that do not project onto a theta surface are neglected when only using PV. Given that the authors also veer into mesoscale arguments with significant circulations perpendicular to theta surfaces, the caveats and limitations of only using a PV framework could have been elaborated on.

We agree that the validity of the balanced flow assumption required for agreement between the flow field obtained from PV inversion and the actual flow field becomes more questionable at the meso- and smaller scales. However, there is evidence to support that the PV concept is still useful at such scales. Davis and Weisman (1994) showed that mesoscale convective vortices evolving from mesoscale convective systems (with horizontal scales of 100-200 km) are nearly balanced, although their formation depends on unbalanced motions. Weijenborg et al. (2017) concluded that the statistically significant flow anomalies associated with PV anomalies resulting from cells of summertime deep moist convection imply that the PV dipoles might be invertible in a statistical way and discussed possible routes to inverting PV at the convective-weather scale. This study, and those of Weijenborg et al. (2015) and Chagnon and Gray (2009), also found that PV dipoles can have longer lifetimes than the convective updraught that initiated them, increasing the likelihood that balanced circulations exist. Finally, individual mesoscale PV anomalies can aggregate to form larger anomalies that are associated with coherent larger-scale horizontal circulation anomalies, implying the qualitative validity of PV inversion at this scale. For example, Oertel et al. (2020) showed how the upper-tropospheric PV anomalies associated with embedded convection in a WCB aggregate to form elongated PV dipole bands. To better discuss this important aspect about the validity of PV inversion at the mesoscale, we added a slightly shortened version of this reply to Sect. 2 (new L252-263).

Regarding the authors' response: "When we mention the usefulness of the PV concept, then we don't regard PV in isolation. Rather, e.g., in a QG framework, we mean that PV determines the geostrophic flow, which in turn determines the ageostrophic flow. For the context of this review article, this conceptual framework still holds, with the additional (complicated) element that diabatic processes can create or destroy PV, and in this way modify the flow." It is not clear what the authors mean by that the "conceptual framework still holds". In general, PV thinking is based on postulating the invertibility principle, i.e., as the authors state, that one makes balance assumptions to revert PV into a purely non-divergent balanced and hydrostatic flow. The original PV, however, was calculated using the full flow field; so, depending on the actual state of the atmosphere, the flow field obtained by PV inversion can have significant deviations from the actual flow field. When moving more and more to meso and smaller scales, one should hence be allowed to wonder how justifiable a purely balanced flow assumption is. …

We think that this comment addresses the same issue as the first comment, namely that balance assumptions inherent in the conceptual PV framework are admittedly more questionable at the mesoscale than at synoptic scales. See our response to the first comment.

… And regarding diabatic heating; it can neither destroy nor create PV, except if occurring at the boundary. It merely rearranges PV. When "creating" or "destroying" PV in the interior by diabatic heating, these creations and destructions cancel each other; so, it can be seen as misleading to refer to as creation and destruction.

Here we disagree with the reviewer, or, more precisely, we prefer using a different terminology. As discussed in the Haynes and McIntyre papers, positive and negative PV tendencies cancel each other if integrated over the entire atmosphere (disregarding processes at the boundaries). However, the PV of air parcels can change, which we refer to as material PV production or destruction. As discussed at length in our paper, the fact that material PV production happens typically at the bottom of a region of intense diabatic heating, and material PV destruction at the top, has a profound influence on the evolution of cyclones and upper-level Rossby waves. The fact that these tendencies cancel each other if integrated over the entire domain does not imply that diabatic heating has no effect on the atmospheric flow and/or that this effect cannot be investigated with the aid of PV.

Regarding the authors' response to the flaws in the discussed surface pressure tendency. The "extensions" introduced by the authors who originally introduced the diagnostic did certainly not remedy the physical flaw in the diagnostic. A hydrostatic surface pressure cannot be changed directly by diabatic heating, as implied by their diagnostic (see Bannon 1996 and Spengler et al. 2011). It is therefore unfortunate that the authors decided to maintain their reference to this flawed diagnostic.

References:

Bannon, P. (1996): Hydrostatic Adjustment: Lamb's Problem. J. Atmos. Sci., 52, 1743-1752,

Spengler, T., J. Egger, and S. T. Garner (2011). How Does Rain Affect Surface Pressure in a One-Dimensional Framework? J. Atmos. Sci., 68, 347-360, https://doi.org/10.1175/2010JAS3582.1

We recognise the continued controversy regarding the use of this surface pressure tendency diagnostic. However, as this is a review article it is important to document the use of this diagnostic. We also note that the reviewer has not explained why the extension introduced in Fink et al., (2012) fails to address the physical flaw in the diagnostic described in Spengler and Egger (2009). Specifically, following the comment on their earlier work by Spengler and Egger (2009), in Fink et al. (2012) the authors introduced an extended form of the surface pressure tendency that includes a term for the changes in the geopotential at the upper boundary of the column, $\rho_{sfc}\frac{\partial \phi_{p_2}}{\partial t}$, (in addition to other changes). Sensitivity tests yielded a value of 100 hPa for the upper integration boundary, $p_2$, as integrals were found to remain nearly constant for upper integration boundaries beyond the tropopause. Figure S1, and the associated text, in their appendix showed that the authors recognise that diabatic heating can only modify the hydrostatic surface pressure if mass is removed from the column – as explained in Spengler et al. (2011) – and that this new term has been included as a consequence. When applied to the five explosively deepening extratropical storms considered in the study by Fink et al. (2012), this new term was found to be significant for a small number of time steps.

As a consequence of the reviewer's continued concern about the surface pressure tendency diagnostic, L2503 (in the last version; now L2526) has been edited to provide more information through the addition of a footnote:

"The surface pressure tendency equation as formulated by Knippertz and Fink (2008) and Knippertz et al. (2009) was extended, in response to Spengler and Egger (2009) [Footnote], and used to quantify the contribution of diabatic processes to extratropical cyclone development by Fink et al. (2012) …

Footnote: This extension included the addition of a term for changes in the geopotential at the top of the considered atmospheric column (set to 100 hPa) to recognise that while heating can result in the adjustment of pressure and density profiles in an atmospheric column, reduction in hydrostatic surface pressure can only be caused by net mass removal; see also Spengler et al. (2011)."

## Reviewer 5 (Lance Bosart)

Recommendation: Minor revision

Outstanding paper on a decadal time scale. The scope of the paper prompted me to revisit some of the earlier re3search that i did with Chris Davis (and others) on tropical transition. Appended are comments and additional references on tropical transition for consideration by the authors ...

I reread sections 1-4 of the Wernli and Gray manuscript (and I skimmed the remaining chapter) as you requested. Appended is my second review (I had trouble navigating some of the chapters due to user error at my end which made it difficult for me to insert comments and suggestions). Appended is my additional "old-school" low-tech review. Wernli and Gray have produced an excellent and comprehensive overview paper on the dynamics of extratropical weather systems that will be a "must read" document once it is published. One area that could benefit from some additional discussion is the problem of tropical transition (TT). I have appended some additional material on the TT problem for consideration by the authors and the editors. Thank you.

Lance Bosart

We are most grateful to the reviewer for the nice words and for suggesting several papers to potentially include in the review. Below, we explain for each of them either where we included the reference or why we decided to not include it.

Some possible additional Tropical Transition references for possible inclusion in the Wernli and Gray review paper:

1. Bosart, L. F. and J. A. Bartlo, 1991: Tropical Storm Formation in a Baroclinic Environment: DOI: https://doi.org/10.1175/1520-0493(1991)119<1979:TSFIAB>2.0.CO;2

This was the very first paper on tropical transition (TT) to my knowledge. Only one problem: I did not propose a "catchy name" for this phenomenon. That would not come until 13 years later when Chris Davis and I published a paper in the AMS Bulletin of the American Meteorological Society (BAMS) entitled: "The TT Problem: Forecasting the Tropical Transition of Cyclones": (reference #2 below)

2. Christopher A. Davis and Lance F. Bosart (2004): The TT Problem: Forecasting the Tropical Transition of Cyclones: Published Online: 01 Nov 2004, Bull. American Meteor. Soc., DOI:https://doi.org/10.1175/BAMS-85-11-1657

The paper by Davis and Bosart (2004) was already included in the last version on L1930 (now L1964). The paper by Bosart and Bartlo (1991) is now included in the new paragraph about the role of diabatic processes for tropical transition in Sect. 5.3.2 (L1965).

Backstory: I was at NCAR the previous summer (2003). Chris Davis showed me a numerical simulation of a Gulf of Mexico tropical cyclone (TC) that left a lot to be desired (in more ways than one). I remembered my paper with Bartlo from 1991 on TC Diana (1984). I showed Chris how TC Diana formed from a baroclinic system and suggested that he run MM4 on TC Diana (1984). We got an excellent simulation … which prompted us to write our 2004 BAMS paper referenced above. Our full science paper on TC Diana was published one year earlier and can be found here:

Davis, C. A. and L. F. Bosart (2003): Baroclinically Induced Tropical Cyclogenesis; DOI: https://doi.org/10.1175/1520-0493(2003)131<2730:BITC>2.0.CO;2

It seems that the detailed paper about TC Diana is the paper by Davis and Bosart (2001): Numerical simulations of the genesis of Hurricane Diana. Part I: Control simulation. Mon. Wea. Rev., 129, 1859–1881. We included this paper in the new paragraph (L1967). The study by Davis and Bosart (2003) was also worth including (L1970), as it discusses the important role of diabatic processes for reducing vertical shear during tropical transition.

3. Galarneau, T. J., L. F. Bosart, C. A. Davis, and R. McTaggart-Cowan (2009): Baroclinic Transition of a Long-Lived Mesoscale Convective Vortex

We regard MCV dynamics as beyond the scope of this paper and therefore did not reference this study.

4. Bosart, L. F., and G. M. Lackmann, 1995: Post landfall tropical cyclone reintensification in a weakly baroclinic environment: A case study of Hurricane David (September 1979). Mon. Wea. Rev., 123, 3268–3291. DOI: https://doi.org/10.1175/1520-0493(1995)123<3268:PTCRIA>2.0.CO;2

As explained in our previous reply document, we decided to be comparatively brief about the topic of extratropical transition (ET) because of the recently published excellent two-part review paper about ET written by the experts (which we are not). Therefore, we prefer not to add more papers about ET. Thanks for your understanding.

5. McTaggart-Cowan et al., 2010: Development and Tropical Transition of an Alpine Lee Cyclone. Part I: Case Analysis and Evaluation of Numerical Guidance, Mon. Wea. Rev., DOI: https://doi.org/10.1175/2009MWR3147.1

We included a reference to this paper in L1974.

6. McTaggart-Cowan et al. (2006): Analysis of Hurricane Catarina (2004): DOI: https://doi.org/10.1175/MWR3330.1

Backstory: Initially, we met a lot of resistance from some folks in Brazil about whether Catarina was a legitimate TC due to domestic political reasons (e.g., the unspoken implication that the Brazilian meteorological service was asleep at the wheel). Fortunately for us, the science … and the MWR reviewers of our paper … were on our side (if it looks like a duck, quacks like a duck, then it is a duck) and our paper sailed through the review process.

This is certainly a very interesting event and study, but since it does not focus particularly on the role of diabatic processes, we decided to not include it in our paper.

7. McTaggart-Cowan et al., 2007: Hurricane Katrina (2005). Part II: Evolution and Hemispheric Impacts of a Diabatically Generated Warm Pool. DOI: https://doi.org/10.1175/2007MWR1875.1

See response about ET studies above.

*Additional suggestion sent to the first author via Email:*

I left off a relevant tropical transition reference in my second review of your excellent paper with Gray; see:

Bentley et al. (2017): A Dynamically Based Climatology of Subtropical Cyclones that Undergo Tropical Transition in the North Atlantic Basin. DOI: https://doi.org/10.1175/MWR-D-15-0251.1

This paper appeared in 2016 and was already included in the previous version on L1939 (now L1951).