Review of egusphere-2023-2678

"The importance of diabatic processes for the dynamics of synoptic-scale extratropical weather systems—a review"
by
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Recommendation: Major revisions

General Comments:

The authors have put a lot of effort in summarizing and reviewing the evolution of our thinking of the effects of diabatic processes on synoptic-scale cyclone development. As usual, with such attempts, it is difficult to do the entire field of research justice and certain selections must be made by the authors in terms of focus. While the review is quite extensive, some recent literature and arguments have not been included thus far. Furthermore, potentially related to the background and work of the two authors, the manuscript focuses mainly on PV diagnostics based on Lagrangian, tracers, or other direct diagnostics of PV. Hence, the title might be a bit misleading and should potentially reflect this choice.

In addition, the manuscript reads more like an extensive summary of research conducted the last decades and one and a half centuries but does not necessarily do justice to the term "review". Review, to me, would imply that the authors go beyond summarizing the material and provide a more detailed discussion of the different methods, their pros and cons, as well as if the community along the way concluded on the adequacy, or lack thereof, of different concepts, diagnostics, and theories. Such a review of diagnostics, concepts, theories would be highly desirable. For example, most scientists would probably not anymore believe that CISC is really an appropriate concept for moist-baroclinic development.

The authors provide a general introduction to latent heating, which is maybe not needed given the audience. There is also a rather lengthy introduction of symmetric instability, which is potentially also beyond the focus of this review. In general, the part on slantwise convection seemed a bit out of place given the focus is mostly on synoptic scales.

Given the authors' background, their almost exclusive focus on PV is understandable, though there have also been other attempts to understand the effects of diabatic heating on the synoptic evolution that remain unmentioned. For example, the diagnostic of the tendency of baroclinicity (Papritz and Spengler, 2015), which has subsequently been used to postulate the hypothesis that cyclone clusters are formed diabatically (Weijenborg and Spengler, 2021) is not mentioned. This concept does neither rely on neglecting certain parts of the flow (PV only deals with the circulation on theta surfaces) nor on having to separate into a basic state and anomaly, while it clearly demonstrates the workings of diabatic forcing on the synoptic, and even larger-scale, evolution.

When the authors introduce the concept of PV in 2.1, the discussion lacks a discussion of the disadvantages of the usage of PV. For example, equation (1) highlights the fact that only that part of the circulation that is perpendicular to the gradient of theta is represented by PV, i.e., only the circulation within a theta surface. All other parts of the circulation are neglected and can thus also not be addressed by the PV tendency in equation (3). Examples for such kind of circulations would be a sea-breeze, or frontal circulations. Of course, in a QG framework, these would be regarded as ageostrophic and thus potentially of lower relevance, but the implications and limitations of using a PV framework should be more clearly stated. The formulation in equations (5)-(7) could have been related to the circulation theorem, which yields the same result (implied impermeability), when only choosing that part of the circulation that projects on a theta surface.

With respect to the interpretation of the detected tendencies and changes in PV (e.g., section 5.3.2 and others), one should be aware that to understand the system and its evolution, it is not sufficient to merely quantify changes in PV. For example, while evaporation below an area of latent heating might yield a stronger PV anomy at the interface between these differently signed diabatic forcing, the actual effect on the evolution of the cyclone might still be detrimental (Haualand and Spengler, 2019). Thus, the discussion of diagnosed anomalies should be put in more context to the overall development when presenting the derived fields, trajectories, and tracers.

I am admittedly not too familiar with all the detailed historic developments in the field prior to the mid 20ies century and greatly appreciate the effort the authors put into providing a wider historic overview of the concepts of cyclone development. However, it feels strange that the work of the Bergen School of Meteorology is not mentioned when it comes to the concepts of the structure and airstreams in cyclones, as several seminal papers on the structure of cyclones came out of this school. Even though the naming of the airstreams and sectors of cyclones at the time was different, the cold and warm/moist airstreams were clearly depicted in their conceptual cyclone models. These contributions should be included in sections 3.1 and 3.2.

With respect to polar lows in section 5.2.5, Terpstra et al. (2015) stressed the role of diabatic processes, despite them often being argued to be small due to the low values of absolute available moisture. One of the main arguments of Terpstra et al. (2015) being that despite the diabatic heating being significantly smaller than in extratropical cyclones, the effect on the PV tendency is comparable to extratropical cyclones, because the vertical extent is also significantly reduced, thereby increasing the effect of the gradient of the heating on the PV tendency. Furthermore, Stoll et al. (2021) clearly classified polar lows as moist baroclinic cyclones in a recent climatological analysis, while clarifying that the genesis through hurricane-like processes is rather unlikely, due to the excessive amounts of baroclinicity and shear at the genesis time. In general, the notion of an upper-level PV anomaly must be applied with caution for polar lows, as the usual altitude chosen to detect these PV anomalies are well within the stratosphere and thereby not necessarily directly relatable to the development of the surface-based polar lows. Furthermore, these upper-level anomalies are often of much larger character than the developing polar low, also rendering a direct interaction questionable.

The surface pressure tendencies introduced at the beginning of 5.3.4 are significantly flawed (Spengler and Egger, 2009). A simple thought experiments directly reveals the false physical nature of the diagnostic. Assume a horizontally uniform atmosphere, equivalent to an atmospheric column with no horizontal advection, which is initially motionless and where a mid-tropospheric heating is applied. Note that all concepts are hydrostatic. In such a hydrostatic setup, the heating cannot result in a direct change of surface pressure, which can only be caused by horizontal mass rearrangements (secondary circulation), or precipitation (Spengler et al., 2011). However, the diagnostic introduced by the various authors in section 5.3.4 directly implies a “diabatic” surface pressure tendency, which is not physical and points to a significant flaw in the diagnostic. Such an attribution could at best be achieved in a balanced framework, such as QG, though then the challenge is to define suitable boundary conditions for the inversion that is difficult to prescribe a priori (Spengler and Egger, 2012).

Related to predictability and the effects on the upper troposphere, a recent study highlighted the importance of diabatic heating compared to tropopause structure for initial error growth (Haualand and Spengler, 2021). Even more drastic changes, or implied initial errors, along the tropopause are easily dwarfed by the effects of misrepresenting latent heating.

Given that the authors state that they “touch on the effects of […] surface fluxes, in particular where studies have contrasted the effects of these processes with the effects of latent heating”, it is surprising that recent studies highlighting the direct and indirect (enhanced latent heat release due to latent heat fluxes) effects of surface fluxes are not mentioned (Haualand and Spengler, 2020; Bui and Spengler, 2021). Previous studies in general showed that surface sensible heat fluxes have a detrimental effect, as they reduce baroclinic structure in the cyclone, while these more recent studies emphasise that the additional latent heat release available due to the surface latent heat fluxes can easily dominate the cyclone development in a favourable way.

Line 636-647: It is not clear what this discussion of lee cyclogenesis has to do with the main topic of the review article, i.e., the importance of diabatic processes. Consider removing or putting in context.

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