

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

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This paper presents a comprehensive review of the role of diabatic processes in the development and structure of weather systems, as envisaged from the nineteenth century to the present day. This is a complex topic, as the physics is dominated by small to mesoscales and there is so much variability between cyclones that it took a long time for meteorologists to agree on the nature and importance of diabatic processes compared to the mathematically elegant theories of dry baroclinic instability. Much progress on this topic has been achieved over the past twenty years and this review is timely and welcome. It should be published with minor corrections – but because of its scope and length there are a lot of these.

This is not, and should not be, a comprehensive review of every paper ever published on diabatic processes in cyclones, as the authors make clear in the Introduction. It will be most valuable as a summary of these distinguished authors' own understanding of the topic, and of the papers that led them to that understanding, as this will give the review a coherence that those unfamiliar with the field will most appreciate. Of course, much of this already comes through strongly in the manuscript and my comments will mainly be aimed at improving the flow and readability of the paper.

I liked the structure of the review sections, with 'pauses for breath' or summaries at the end of each subsection. As my detailed comments will show, I think some of these could be developed further to provide a synthesis of the science that was revealed by the papers, rather than just reiterating what they did. Although I will concentrate here on sections 5 and 6 (following the Editor's request), these comments actually apply most strongly to the earlier sections where the evidence presented was sometimes contradictory, making it difficult to see what advances in the science actually occurred.

Comments on section 5

The advent of reanalyses has indeed been a game-changer for meteorological research generally, not just for this topic, and amply merits detailed discussion in 5.1. The section is informative and reads well, but the Summary is perfunctory and adds little. What is needed here is a summary of what the reanalysis papers found e.g. regarding the distribution among cyclones of the importance of diabatic heating, its possible added relevance to the more extreme cyclones, the link to 'atmospheric rivers' etc. How did this approach advance the science?

Section 5.2 discusses 'diabatic processes in (special categories of) extratropical cyclones', presenting seven loosely-connected subsections beginning with cyclone classification.

- i. I recommend that the authors reconsider the order of their subsections, moving extratropical transitions to the penultimate slot with a more natural transition to tropopause level vortices. The transition from Type C cyclones to subtropical and then Mediterranean cyclones would then be smoother.
- ii. Section 5.2.1 would make more sense to a reader unfamiliar with cyclone categorisation if the authors explained what the Peterssen and Smebye A/B scheme actually is before launching into a type C discussion.
- iii. The Mediterranean section should be shortened, concentrating on distinctive properties of these cyclones, other than where they occur (e.g. paragraph 1736-1746 could be omitted, and I'm not sure what the last paragraph, on moisture sources, is adding to the science).
- iv. It is appropriate to include polar lows in this review because of the contribution of convection to many of them, but the section could do with editing to make the key points clearer. I suggest that the text from 1815 to 1821 be removed as it lapses into jargon inconsistent with

the rest of the section and detracts from the theme of diabatic heating. Likewise, the paragraph 1843-1858 goes into a level of detail not required here, given the existence of reviews specifically of polar lows.

- v. To my mind Diabatic Rossby Waves are of a different order of importance to the other subsections here, as this is a distinctive dynamical process in its own right. Could this become section 5.2, then section 5.3 would include the other subsections? I leave this to the authors' discretion but it would allow for mention of DRWs in the sections that currently precede it, and better overall coherence.
- vi. The summary subsection 5.2.8 is appropriate to this section

Section 5.3 discusses novel diagnostics of diabatic PV modification. Again the subsections are appropriate but could benefit from some critical editing.

- i. The first paragraph of 5.3.1 is too detailed – readers should consult the original papers for the detailed methodology – while the second paragraph could benefit from more examples of results obtained from PV tracer analysis.
- ii. The final paragraph of 5.3.2 doesn't lead anywhere – did Büeler and Pfahl find anything useful from their study? If not, this paragraph could be deleted.
- iii. On line 2175 we are cautioned that 'caution needs to be applied when inferring dynamical causation from ensemble sensitivity analysis' yet in the very next paragraph the word 'sensitivity' is used instead of 'association' three times! These paragraphs need to be consistent with each other.
- iv. Has the adjoint technique led to any new insights into diabatic processes? The result that 'forecasts of high-impact cyclones were found to be strongly sensitive to low to mid-tropospheric moisture in the initial state' is hardly novel. Section 5.3.3 is one which could be considerably shortened.
- v. The summary is again appropriate

Section 5.4 concentrates on the impact of diabatic processes on the dynamics at tropopause level, through the outflow of WCBs and tropical cyclones. I thought this was balanced and coherent, with an informative summary, and but for a couple of minor comments (see separate section below) I have no major problems with it.

Section 5.5 describes the two field campaigns DIAMET and NAWDEX which the authors consider to be the only two experiments of note since 2000 to study diabatic processes in cyclones.

- i. Given that extratropical transitions fall into the domain of this review, mention should also be made of T-PARC (2008).
- ii. The concept of the sting jet arose from analysis by Browning of the Great Storm that struck Southern England in 1987 and was developed by diagnosis of high-resolution models by Clark et al. This paper needs to explain more clearly what the DIAMET measurements contributed, for example by explaining what figure 16b is supposed to show. It also needs to acknowledge that the sting jet (defined as a descending airstream) is a transient phenomenon, especially when compared to the cold conveyor belt which dominates the low-level wind field in the southern quadrant of a mature cyclone.
- iii. Although 5.5.2 is entitled 'Embedded convection', 'Embedded convection and negative PV bands' would better describe the content.

Section 5 concludes with a discussion of the relevance of diabatic heating in cyclones to climate change research. On the face of it, a warmer climate will mean more moisture in the atmosphere and more scope for diabatic heating. But, as 5.6.2 shows, the problem is far from linear and the location of diabatic heating relative to the cyclone centre is critical when considering its effect on the cyclone. It appears that in a warmer climate the PV source region will be further from the cyclone centre on

average – but the tail of the distribution, where the two line up, may result in a few very powerful windstorms. Very interesting! The long discussion in 5.6.3 of the effect of model resolution on the diabatic effects on cyclones in GCMs could be shortened considerably, as the details are covered in many other papers. This review could simply summarise the conclusion of these studies, i.e. a short introductory paragraph then pick up at line 2487. Although the summary of this section is informative, I recommend that it be expanded by a few sentences to cover the issues of propagation and blocking discussed in the text.

Section 6 provides a summary of the basic concepts presented in the review and looks to the future of the field. It reads well and I have no major comments on it.

Minor comments

1. 1496 millenium

2. 1.1587 Either omit ‘only’ or restructure to ‘...revealed that only in the NH winter are cyclones usually accompanied by a strong WCB...’, depending whether the sentence is meant to contrast winter and summer as well as the two hemispheres.

3. 1. 1603 The idea of a cyclone with no WCB is most likely an artifact of the definition assumed for a WCB in Binder et al’s study (or a problem with their method) than a reflection of the dynamics of the cyclone. Read literally it means there was no ascending airstream ahead of the cyclone, which is hard to square with explosive development. The idea is counter to the whole thesis of this review and requires more discussion if considered to be a real result.

4. 1. 1635. Please explain what the Petterson and Smebye A and B cyclones actually are. The section is difficult to follow for someone not versed in cyclone classification because the story starts in the middle.

5. 1.1851 lesser

6. 1.2330-1 Isn’t downstream development a consequence of Rossby wave propagation? The underlying dynamics are the same so this sentence needs to be re-phrased.

7. 1.2420-2425 Which simulation corresponded best to reality in this case?

8. 1. 2727 isn’t the argument that, basically, the same cyclone in a warmer climate will produce more precipitation just because it is warmer (the ‘Clausius-Clapeyron effect’)?

9. 1.2979. It is provocative to claim, without evidence, that the field campaigns were a direct result of a handful of high-priority storms. For example, FASTEX was not organised as a response to the Great October Storm and DIAMET (according to its description earlier) was not organised as a response to the discovery of sting jets.

10. 1.2995 The authors have chosen not to say much about satellite measurements in this review, despite the crucial role they played in the thinking of key figures in the field, such as Keith Browning. That is their prerogative, though still an omission. But one of the key conceptual tools of the satellite era was time-lapse videos from geostationary satellites, which actually show how the WCB develops alongside the cyclone and the cold front, and that should be mentioned here. It is not speculation to point out the key role of satellite images in developing understanding, and that word should be removed. So also the ‘real-time availability’, so important for forecasting but not for research where the better-quality images available after the event (especially in the 1980s) were more useful.