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Interactive comment on "Potential vorticity structure of embedded convection in a warm conveyor belt and its relevance for the large-scale dynamics" *by* Annika Oertel et al.

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Review of "Potential vorticity structure of embedded convection in a warm conveyor belt and its relevance for the large-scale dynamics" by Annika Oertel, Maxi Boettcher, Hanna Joos, Michael Sprenger, and Heini Wernli.

The paper presents a detailed analysis of a case study of warm conveyor belt associated with an extratropical cyclone observed during the NAWDEX field campaign and described in a previous paper. Thanks to Lagrangian trajectories computed online in a convection-permitting simulation of the case study, convective ascents are distinguished from slantwise ascents within the warm conveyor belt. A composite analysis

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Discussion paper

highlights the warmer and moister origin, the production of graupel and more intense precipitation for convective ascents. A horizontal dipole of potential vorticity in the outflow region is explained theoretically by considering the evolution of horizontal and not only vertical vorticity components. An example of frontal convection is then described to illustrate the formation of a dipole band of potential vorticity and its evolution suggests a contribution to upper-level gradients and jet acceleration.

The paper addresses the important question of how small-scale convective processes to large-scale dynamical processes, which is often neglected due to the inability of most NWP model runs to encompass both scales. The results are based on relevant methods, sound interpretation and high-quality figures. However, the paper is unbalanced between, one the one hand, a composite analysis of all convective ascents to investigate their dynamics and, on the other hand, an illustration of a specific convective ascent to investigate its impact, which implications tend to be over-interpreted. Moreover, the reasoning is often difficult to follow due to breaks in the logic flow, jumps from one figure to the other, and statements that lack precision. Several general and numerous specific comments are listed below to help improving the structure and clarity of the paper.

GENERAL COMMENTS

I. Unlike the systematic analysis of convective ascents based on composites, the impact on the large scale is investigated based on a single convective PV dipole band, is rather qualitative and uses offline instead of online trajectories. Either extend toward a more quantitative framework, e.g. also based on composites, or at least carefully discuss the results and implications throughout the paper (including in title and abstract) considering these limitations.

II. The “big picture” is diluted in the introduction: the contrast between small and large scale is clear but what is referred to as mesoscale? There is a confusion between isolated cells and organized convection, and embedded convection needs a proper definition (from the example in Fig. 8 it appears to develop along the cold front only). Citations need to be revised and the organization should be improved (see next comment).

III. Theoretical considerations on PV production need reorganization: they appear too early in the introduction and are largely repeated in Sections 3.4.3 and 3.4.4. Consider moving the detailed PV discussion to a short theoretical section, which could later be referred to. Some elements may be moved from the discussion section and grouped with either the introduction or the theoretical considerations.

IV. Most of the paper is based on composites of Lagrangian trajectories, which is a very interesting approach but may look abstract to the reader; sometimes a figure in geographical coordinates would be helpful. Placed earlier, Section 4 would well introduce the case study and illustrate the different concepts that are developed in sections 3 and 5, thus remove the need to constantly refer to Oertel et al. 2019 and to Fig. 8(d) early in the paper.

V. The text contains several repetitions and frequently jumps back and forth between figures. In addition, it often refers to Oertel et al. 2019 and other papers to explain results, which creates a confusion between what is expected from previous work and what is actually found here. Please streamline and clarify to improve the flow.

SPECIFIC COMMENTS

Abstract

l. 4-5 not sure this sentence is needed

l. 8-10 the sentence somehow suggests that graupel is part of surface precipitation

l. 11 what does “they” refer to?

l. 17 perhaps insist on negative PV values? (not just anomaly)

l. 17-19 this is speculative, as only one example is presented

l. 21-23 this is also speculative, for the same reason

l. 23-25 this basically repeats what is written above; what are broader implications of the study?

Introduction

l. 27 “their”?

l. 29 The two references are not clearly related to moist diabatic processes in extratropical cyclones.

l. 35 “potentially affect”: better “can affect”?

l. 37 The transition from general WCB dynamics to precise PV theory is abrupt.

l. 42 Please define terms in brackets (or omit).

l. 46 Please define f, zeta and theta_dot.

l. 47-49 This sounds very similar to l. 34-36.

l. 51 The first two references do not mention mesoscale convective systems.

l. 52 What are “convective storms”? (vs. MCS above)

l. 58-59 Is this not what has just been stated in l. 53-57?

l. 61 The horizontal vorticity vector w_h must be defined when it is introduced first.

l. 60-63 This process is not obvious and requires more details. I appreciate it is supported by a schematic but I do not exactly understand what is what in Fig. 1. Can you make the schematic more reader-friendly, e.g. by illustrating why the vectors are oriented as they are, using more explicit colors and referring to them in the text?

l. 71-75 There is a confusion between negative PV, the different types of instabilities and their consequences. Schultz and Schumacher (1999) rather discuss conditional symmetric instability, which is another type of (slantwise) instability.

l. 79 “observed” is not the appropriate word for model data. Better “found”?

l. 81-93 This general paragraph would rather better appear before the previous, more specific one.

l. 87 The cited studies use very different types of “remote-sensing data”. More specifically?

l. 89-93 There is a confusion between what impact and which study relates to WCB or convective systems. Furthermore, the link with forecast errors would better fit with the modification of the large-scale flow in the first paragraph.

l. 94-114 Rather than pointing what has not been done yet and focusing on very precise questions, this part would better motivate the study if it would emphasize what are open questions (e.g., contribution of embedded convection to WCB dynamics), why they are important (e.g., reconcile small- and large-scale views) and how they are addressed here (e.g., convection-permitting simulations of a case study).

l. 94 MCSs are not just “individual convective updrafts”.

l. 116 which case study?

Data and approach

Past and present tenses are often mixed in this Section, please stick to one or the other.

l. 123 is it the same setup or the same simulation?

l. 125 “grid spacing” rather than “resolution”.

l. 142-158 It is difficult to get a general picture of the cyclone evolution without reading Oertel et al. (2019). A graphical summary would be helpful, e.g. by adding the cyclone track (and the upper-level trough) on Fig. 2(a).

l. 142 explicit NAWDEX

l. 150 more than 400 or 600 hPa?

l. 156-157 this last sentence is unnecessary

l. 163-166 Why combine two criteria? (400 hPa in 1 h and 600 hPa in 3 h) Is a fast ascent (top 10%) sufficient to be considered "convective"?

l. 170-172 The description is confusing: temporal evolution of what? is it really the position relative to the cold front? Where is the upper-level trough?

l. 174 behind rather than "ahead of"?

l. 176 again, showing the cyclone position would be helpful for the general picture.

Characteristics of convective and slantwise WCB ascent

l. 183-184 what about the vertical coordinate?

l. 188-189 not only the impact but also the environment of trajectories

l. 190 is this shown somewhere?

l. 193-194 does it mean that circles in Fig. 2(a) are also at 00 UTC 23 and 24 Sep mainly?

l. 201-204 "warmer and moister region": is it really warmer? Fig. 3(a,b) does not explicitly show the contribution of theta to theta_e and Fig. 4(a,b) suggests that the difference in absolute temperature T is due to a different height. Also, some indication about how much the two composites overlap is needed, either by showing statistical significance on cross-sections at least by giving the standard deviation around mean values.

l. 205-211 apart from highlighting low-level convergence, it seems that these lines do not add any new information to "the region is warmer and moister"; clarify or streamline.

l. 214-215, 222-223 what about the difference in height? Is it significant?

l. 218-219 can you be more precise about where to find this information in Oertel et al. 2019?

l. 221 “the observed rapid convective updrafts” and associated references: it sounds like a conceptual description of potential instability but not necessarily of what happens here

l. 222 mention somewhere the different time scale to emphasize the different ascent rate

l. 226-228 can you be more precise by giving a value of attained height (average+/-std)

l. 230 Please design new panels for the time evolution of surface precipitation in Fig. 5; panels (c,d) are already very busy and mixing vertical profiles with a scalar value is extremely confusing. It should also be mentioned somewhere that (a,b) are instantaneous values taken at the respective time of max surface precipitation.

l. 236 “comparatively thick cirrus cloud” compared to what?

l. 237 turquoise contours?

l. 237-241 it is unclear how the cirrus cloud related to convection, as its core is located well above the composite trajectory; is it due to a fraction of faster-ascending trajectories?

l. 243-244 “horizontally more homogeneous”: can this really be seen in time-height plots?

l. 246-247 this is interesting indeed, but may it be due to the compositing process, or are there actual profiles where ice water extends above the tropopause level?

l. 251-253 this largely repeats what is written above and is thus unnecessary

l. 255-256 number of trajectories starting their ascent at that time?

l. 256 is the precipitation area roughly constant, i.e., are variations in Fig. 2(b) due to variations in intensity or in concentration?

l. 257 “Nevertheless”: furthermore?

l. 258-259 what is the citation here needed for? Clarify or omit

l. 278-282 where is this effect seen in Fig. 5(c,d)?

l. 286 “observed PV distribution”: more specifically? Avoid “observed” if from model

l. 294-295 “in particular” seems to contradict “despite” above

l. 301-302 why that time? (maximum precipitation rate?)

l. 303-306 mention the different scales in (a,b) or add box of (a) in (b)?

l. 308-309 “as a consequence” appears to repeat “due to”

l. 317 Fig. 5(c) does not explicitly show diabatic heating

l. 322-324 this statement appears speculative

l. 323 specify what to look at in Oertel et al., 2019, Fig. A1

l. 330-332 I do not clearly see the vertical PV dipole expected in this case according to the previous sentence

l. 345-346 how is this shown in Fig. 7(a)?

l. 346-350 is it an interpretation or is it really shown somewhere?

l. 351-358 similar to above, is this shown somewhere for the composite or does it refer to the theoretical schematic only?

l. 359-366 This largely confirms what is explained in the introduction

l. 368 again, Schultz and Schumacher (1999) mainly discuss conditional symmetric instability

l. 382-384 this partly repeats l. 373-375

l. 395-398 does it occur here? Is it shown anywhere?

l. 402 “is accelerated by” contradicts “hardly exceeds” above

l. 406 “PV dipoles” plural or singular?

l. 407-409 for comparison, what is the value of the vertical shear?

An illustrative example of WCB-embedded convection

The purpose of this section is unclear at that point, as it mostly repeats ideas developed in the previous section; such an “illustrative example” would better fit early in the paper to motivate the systematic analysis based on composites.

l. 420 at 09 UTC 23 Sep 2016

l. 422-423 the previous section insists on the presence of graupel to distinguish convective from slatwise ascent: display graupel here only? And does it occur along the cold front?

l. 423-424 “rapidly ascending WCB trajectories”: convective WCB trajectories?

l. 427-429 this last sentence mostly repeats what has just been stated

l. 432 is PV on the original grid or aggregated in the cross-section?

l. 434 PV below -2 PVU cannot be seen with the colour bar; horizontal PV gradients?

l. 435-436 is the heating maximum shown somewhere?

l. 436 “lens” without e

l. 437-438 please motivate the statement and clarify “mesoscale PV dipole”

l. 441 “rapid WCB ascent”: convective WCB trajectories?

l. 445-446 “which are generated and further enhanced by convective ascent” sounds speculative

l. 459-450 is the thermal wind vector shown somewhere?

l. 458-464 This belongs to the introduction

l. 470 northwest

PV anomalies on a larger scale and relevance for large-scale dynamics

l. 478 is this supported by section 3 (for this case) or by Shutts 2017 (in general)?

l. 479-480 remove “these”

l. 480-483 this sounds as three times the same statement, clarify or streamline; “effective resolution” has a specific meaning for numerical modeling, better avoid

l. 485 is this the case for all larger-scale PV anomalies, or for the example of section 4 only?

l. 486 is the cold front shown somewhere?

l. 490 this seems to describe a specific feature rather than “PV dipole bands”

l. 491 southeastward

l. 492 repetition of earlier statements

l. 493-497 what is seen where? (which contour, colour, panel)

l. 502-509 more arguments are needed to support that the convectively-produced PV dipole in Fig. 10(a) evolves into the anticyclonic anomaly in Fig. 10(e): the trajectories spread over a much larger area than this specific feature at 18 UTC, and other PV structures exist during the evolution

l. 514 why use offline trajectories, while online trajectories better follow convective ascent?

l. 517-518 this largely repeats the previous sentence

l. 525-534 the paragraph contradicts the last sentence in l. 523-524 and is confusing altogether; please clarify

l. 538 how exactly do parcels “gain negative PV”?

l. 544 indeed, a comparison with online trajectories is needed to support this result; but again, why use offline trajectories here?

l. 565 what should be compared between these figures? (which contours)

l. 569-570 this is not sufficiently supported; develop or omit

Discussion and open question

l. 575 not only one case study but one single PV dipole within a cyclone; a first step would be to look at other structures within this cyclone

l. 568 avoid “observations” if model-based

l. 586-594 these various impacts of embedded convection appear speculative; please clearly distinguish between what is due to convectively-generated PV anomalies and to the WCB outflow in general, and be precise about what the cited studies have shown

l. 607 heating is also parameterized, even at convection-permitting resolution, through the microphysical scheme

l. 611-612 “a horizontal resolution of at least 10 km would be required to resolve the convective updrafts”: rather a grid spacing of a few km mostly, as in your simulation

Figure captions: “shading” better than “colours”

Providing titles to subfigures would be helpful, as most display rather complex content

Fig. 2(a) is too busy: consider showing less trajectories (every second, fifth, tenth, . . .) and one representative, thicker theta contour per lead time. It took me a while to understand what is depicted and I still do not fully see the position of trajectories relative to the cold front.

Fig. 4 are these really “Vertical cross-section composites”? l. 186-187 rather refers to “composites of vertical profiles along the trajectories, i.e. time-height sections along the flow”; (a,b) 300, 320 and 340-K isentropes; (c,d) “(moist-adiabatic) lapse rate” rather than “potential instability”; d_theta/dz or d_theta_e/dz?

Fig. 5 “As Fig. 3a,b”: not really, better explain again; what do RWP, SWP, RWC, SWC, . . . stand for? Check units; plot (a) box on (b) for comparison?

Fig. 8 this figure does not meet the otherwise high quality standard of the paper: tick-marks are too small and need °N/°E to indicate geographical coordinates (in contrast to km in composites); white contours are hardly seen on panels (b-d); vectors and vector legends are too small on (c-d); the colour bar is not adapted to the noisy field in (d); colour bars are completely saturated for negative values in (d-f); finally, (a) is not standard infrared imagery, what does it show exactly?

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