Weather Clim. Dynam. Discuss.,
https://doi.org/10.5194/wcd-2020-5-RC2, 2020

Interactive comment on "The sensitivity of atmospheric blocking to changes in upstream latent heating – numerical experiments" *by* Daniel Steinfeld et al.

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Received and published: 24 March 2020

Review of "The sensitivity of atmospheric blocking to changes in upstream latent heating – numerical experiments" by Daniel Steinfeld, Maxi Boettcher, Richard Forbes, and Stephan Pfahl.

The paper investigates the contribution of latent heating during the onset phase of blocking in four case studies spanning the North Hemisphere. The investigation is extended to the maintenance phase for one case study, which is described more thoroughly. The contribution of latent heating is quantified by switching off heating related to cloud processes in a region located upstream of the blocking in sensitivity experi-

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ments with the global IFS model. The impact is diagnosed using the potential vorticity anomaly and divergent wind at upper levels mainly. The results show a clear contribution of latent heating, including periods of bursts, to the intensity of blockings and their extent in space and time, with large case-to-case variability that appears to depend on the flow configuration.

The paper addresses an important topic in atmospheric dynamics, is based on well-designed numerical experiments, and is well written overall. However, as detailed in the general comments below, it contains majors flaws related to a lack of assessment of the numerical experiments, a lack of balance between one detailed case study and three quicker ones, and a general lack of consistency between text and figures. Although the paper is definitely interesting and valuable, it gives a feeling of subjectivity in the choice of case studies and interpretation.

Considering that a systematic analysis of all presented case studies would require much additional work, and as the discussion at the end of Section 4 currently suggests that the impact of latent heating depends on many parameters that cannot be properly covered here, I suggest to remove the additional cases altogether and focus on the Thor case more thoroughly. For instance, with less extra work, the additional sensitivity experiments with alpha=0.5 and 1.5 could be included in Figs. 9–10 to discuss non-linearity, or the respective contribution of microphysics and convection to latent heating could be quantified to contribute to the current discussion in the NAWDEX community.

General and specific comments are listed below to help improving the paper.

GENERAL COMMENTS

I. An assessment of the quality of control simulations is lacking: Fig. 1 provides some comparison with satellite observations for the Thor case but in an indirect fashion and at short range only, while nothing is provided for the other cases. A 10-day run is not expected to perfectly match observations but needs to capture the blocking at least. The lack of predictabillity during the onset of blocking makes this questionable. An

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easy solution would be to add panels for the analysis fields on Figs. 3–4 (and S1–S3) and curves on Figs. 2 and 6.

II. The organization is unbalanced: most of the paper is dedicated to the case study of Thor but related contents are spread between Sections 3 (which contains a subsection 3.1 without 3.2) and 4.1 with some repetitions in 4.2, while additional cases are briefly introduced in 2.4 (without motivation) then discussed in Section 4.2 only (without prior description of their specific dynamics). Describing one case study in details and several cases succinctly is a sound approach but in the present form I do not clearly see what to learn from these additional cases.

III. All along the paper, features such as the upper-level jet stream are discussed but not shown anywhere, while striking contrasts between case studies are not mentioned. Please make sure you actually display what you describe and describe what you display. And please avoid wording such as “it is evident”, in particular for statements that are not.

SPECIFIC COMMENTS

The title could be more specific: “changes” is vague

l. 3 “the causal relationship between latent heating and blocking formation has not yet been fully elucidated”: what is the paper’s contribution to elucidating this causal relationship? (which likely extends beyond blocking “formation” only)

l. 8–12 This does not reflect the contents of the paper: “the jet stream” is not shown anywhere; “warm conveyor belt airstreams” are barely discussed; “an accurate parameterization of microphysical processes” is not particularly supported by the results.

l. 31–33 please develop “the mechanism behind the classical view”

l. 47–49 why are diagnostic methods not able to show a causal relationship?

l. 64–65 This is lower resolution than the operational version and previous studies

suggested that LH is sensitive to resolution: can you compare your control simulations with the operational IFS forecast to estimate this sensitivity?

l. 78 LH is turned off for both microphysics and convection schemes but convective motions are not captured by backtrajectories: what is their contribution?

l. 140 typo?

l. 154–160 It is unclear why these specific cases were selected

l. 165–167 For clarity, and because the nomenclature is used by Maddison et al. (2019),“Stalactite Cyclone” should be mentioned here

l. 171–179 and Fig. 1 The paragraph needs improvement: (1) it is not “evident” to recognize the mentioned features, esp. at mesoscale (please zoom in and/or mark them); (2) a visual comparison between “upper-level” PV (defined as 500–150 hPa mean, please remind in the caption) and cloud top pressure (which is not directly “observed” by MSG) is not “quantitative”; (3) comparisons are for short lead times (36h and 42h, which should be indicated) thus do not support that the evolution is “well predicted” in the 10-day simulations and contradict the “large forecast uncertainty” mentioned above.

l. 186 what is the APV “index” exactly?

l. 190 “confirm”: is it expected?

l. 191 why are quasi-adiabatic processes associated with cooling?

l. 196–197 this is slightly below average compared to the climatology cited above

l. 207–208 is this shown somewhere? It is not obvious. . .

l. 210–219 The discussion is hard to follow, as the ingredients are not explicitly shown (“jet splitting”, “deformation region”, “poleward transport”, “ex-tropical cyclone”, “migratory ridge”). Either detail and add information on Fig. 4, or streamline.

l. 226 how many is “many”?

l. 230 “Fig. 3 a, c, e, g”: do you mean Figs. 3 a, c and 4 a, c? Only 3a and 4a are related to trajectories shown in Fig. 5a, c.

l. 241 see comment above

l. 245, 247 “quasi-adiabatic”: did you explicitly check the 2K heating criterion or do you refer to the stable pressure along trajectories?

l. 256 “mid-level”: better lower-level in contrast with upper-level for 500–150 hPa?

l. 258 “initial time steps” is confusing for day 2: better early evolution?

l. 270 “cold front” and l. 272, 285 “jet stream”: are these features shown somewhere?

l. 285 “as a consequence”: I am not sure this is due to R2 only as the wave pattern is modified altogether (see l. 280)

l. 291, 295 “deformation flow”, “diffluent flow”: not shown?

l. 296–297 see l. 285

l. 298 is cooling explicitly computed along trajectories?

l. 301 Fig. 4 c, d

l. 311–312 see l. 270, 291, 295, . . .

l. 322–323 again, what is the motivation for selecting these specific cases?

l. 331–332 not really: (1) there is substantial case-to-case variability and (2) differences cannot be attributed to “the upstream cyclone” if it is neither showed nor mentioned for the additional cases

l. 339–342 is this all shown somewhere or suggested only? Fig. S4 does not include the additional cases.

l. 350 where is the dipole pattern? Please indicate (a), (b), (c), etc.

l. 355–361 this description (and the related panels) must be moved to the Thor Section above.

l. 369–376 That is certainly interesting but I do not know where to see all of it in Fig. 8.

l. 380–382 Does the box move with the strongest ascent/divergence/advection? Can you show an example? On Fig. 8 in particular it is not obvious where it would be placed.

l. 383–385 again, there is substantial variability both between cases and between lead times (weak signal beyond one week for instance)

l. 385 “magenta”: rather violet?

l. 386–387 is this shown somewhere?

l. 396 sorry to insist but there is again case-to-case variability: the Canada case does not show bursts

l. 396–409 the discussion is not supported by any material: cyclones are not shown in the figures but for the case of Thor

l. 401 which trajectories exactly? Cooling is not explicitly shown in Fig. 5.

Fig. 2 spatial “extent”; labels T1–T5 appear to refer to troughs rather than cyclones.

Figs. 7 why focus on day 3 here and not on day 2 as above?

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