Review of egusphere-2023-72

'Lagrangian transport simulations using the extreme convection parametrization: an assessment for the ECMWF reanalyses"

by Lars Hoffmann et al.

Paper in review in Atmospheric Chemistry and Physics

1 General comment

This study quantifies convective mass and tracer transport using the Lagrangian MPTRAC model driven by re-analysis data and coupled to the extreme convective parameterization ECP. The ECP randomly redistributes air parcels and tracers between the surface and the equilibrium level if a certain CAPE threshold is exceeded. The authors quantify 'explicit' versus parametrized convective updrafts and evaluate the choice of the required CAPE threshold to initiate convective transport. Moreover, the sensitivity of artifical tracer distribution to different settings of CAPE, CIN, vertical mixing and surface layer depth is evaluated.

The manuscript is well-structured, comprehensive and well-written. My major concern addresses the stated key research question and conclusion. The key question (l. 62 ff) addresses the question whether ERA5 (compared to ERA-Interim) is sufficient to properly represent convective updrafts and global tracer transport. My concern is that per definition, one would not expect ERA5 (nor ERA-Interim) to adequately represent convective features based on Lagrangian trajectory calculation from the resolved wind fields, as ERA5 uses a convection parameterization and does not represent convective updrafts explicitly. Thus, it is not surprising that ERA5 without ECP does not 'explicitly' resolve a lot of convective features, and requires a parameterization to efficiently vertically mix air parcels and tracers. Thus, the key conclusion (e.g., l. 18 ff and 466 ff) that ERA5 still needs a convection parametrization to better represent convective and tracer transport is expected given the properties of the re-analysis data. Moreover, it is expected that the spatially and temporally coarser ERA-Interim represents less convective features, which has also been already stated by Hoffman et al. 2019 (l. 41 ff and 422 ff). I think the manuscript could profit substantially from focusing on the sensitivity tests and idealised tracer experiments, the effects of implementing CIN, and more directly setting their results into perspective with other convective transport models, instead of comparing ERA5 and ERA-Interim without ECP, as well as ERA5 with and without ECP, which for the reasons mentioned provides expected results. Instead, I would suggest to streamline the manuscript focusing on sensitivity experiments and systematically evaluating their impact on global tracer distribution. Furthermore, the sensitivity tests and the influence of the tuning parameters CAPE and CIN on convective event frequency are shown and discussed. The results are somewhat expected: For example, if the threshold of CAPE to trigger convective mixing is increased, the frequency of convection is reduced in regions that are climatologically characterized by lower CAPE values. Yet, it remains unclear to me which of the tested ECP setting is more realistic beyond the expected impact. Concerning this aspect, the study may benefit from setting their results into perspective with other studies, observations and comparison to more sophisticated convective parameterizations.

In summary, I think the manuscript (including abstract, conclusion and results) could benefit from focusing on key aspects of their sensitivity studies and streamlining the sensitivity experiments and novel key results. I have several additional questions and remarks to the authors that are listed below.

Major comments

1 Introduction
Although the introduction reads well, I find the content is rather general and recent studies and developments are not sufficiently explained. For example, l. 54 ff state that "various techniques and parametrizations have been developed to better represent the effects of convection in Lagrangian transport simulations". However, no details are provided which makes it difficult to understand the novelty of this study and to set results into perspective. I would ask the authors to provide a more in-depth overview of current research (which is to some extent refered to later in the manuscript).

2 Methods
I would ask the authors to include the first paragraph concerning the sensitivity to the depth of the surface layer into the Methods section (first paragraph in Section 3.7). Moreover, the different settings of the MPTRAC simulations (Sections 3.1, 3.2 as well as 3.3 l. 270-296) could also be placed in a subsection of the Methods. This study uses different settings for the ECP simulations (e.g., CAPE, CIN, vertical mixing, boundary layer depth), which could already be briefly introduced in the Methods part. I was wondering why the authors chose different settings for trajectory seeding? Does seeding on a regular global 0.3x0.3 grid introduce biases in convective mass transport as the seeds are not equidistant (for the tracer simulations a latitude weight was applied)? For convective transport the 0.3x0.3 grid was also applied to the coarser ERA-Interim data. In the case of no ECP parameterization (i.e. no random vertical re-distribution), does this result in trajectories that are quasi-identical in ERA-Interim?

3 Comparison of ERA-Interim and ERA5
I think the authors could focus on the sensitivity tests with different parameters / threshold for the ECP, instead of comparing ERA5 to ERA-Interim without ECP. It is not surpring that convective updrafts occur less frequently in the coarser ERA-Interim data. Moreover, as also mentioned in l. 41 ff and 422 ff Hoffmann et al. (2019) already showed that "ERA5 explicitly resolves peak updrafts more frequently than ERA-Interim". Moreover, trajectory calculations are hardly comparable due to the different spatiotemporal resolution, in particular, the coarse temporal

resolution of ERA-Interim of 6 h. Is it correct that ERA-Interim trajectories are calculated from only two timesteps (as 6-h trajectories are used)? I would thus suggest to focus on the comparison of different ECP settings, and how this affects global tracer distribution.

4 Results
In addition to the comments above, I believe that the Results Section could be improved by streamlining and rearranging some paragraphs. Please see also specific comments below.

2 Specific comments and technical corrections

1. l. 24 ff: Please streamline, and include a few relevant references (e.g., for convective mass transport into the stratosphere).

2. l. 30: The statement to shallow convection could be removed as it is not relevant in this study.

3. l. 32: Instead of referring to textbooks, the authors could include references about the convection parameterization that is applied in ERA5 and ERA-Interim.

4. l. 48 ff: This paragraph outlines that coarser re-analysis data sets are not capable to explicitly resolve small-scale convective features for various reasons. My question is why the authors use ERA5 and ERA-Interim without ECP as it is expected that convection is underestimated in both data sets? Instead, I would suggest to focus on the ECP settings required to obtain reasonable results. See also general comments.

5. l. 99 f: Please add the number of resulting pressure levels and the approximate vertical spacing in the lower troposphere where trajectories are seeded.

6. l. 118: 'particle data': Does this refer to air parcels? Please clarify.

7. l. 124 f: How was the timescale of 6 h as typical convective timescale determined? Please clarify and/or add references.

8. Figure 2a and l. 175 ff: Does the occurrence frequency show convective events and/or the frequency distributions of CAPE values or is this the same, since convective events are triggered simply by the CAPE threshold. Please clarify.

9. l. 177 ff: Given equation 1, a relation between CAPE and the height of the EL is given. Regarding the still considerable scatter in Fig. 2b, I'm not sure if CAPE only is a reliable quantity to estimate EL (e.g., for a CAPE value of 100 $\mathrm{J\,kg^{-1}}$ the EL height varies between 3 and 10 km).

10. l. 196: Why did the authors seed trajectories at random vertical positions in the boundary layer and not at various fixed heights?

11. l. 200: How was the time interval for ECP of 3 h determined? For subsequent analysis, I believe a threshold of 180 s was used (l. 152). Does this apply to all systematic ECP simulations? How sensitive are simulations to this threshold? I would appreciate if this was discussed in the Methods Section.
12. l. 235: Could the authors please elaborate on the spatial downsampling? Are wind fields averaged, smoothed, etc. ?
13. l. 248-250: Could the authors please elaborate on the similarities and differences between this study and Konopka et al. (2022).
14. Figure 5: I would appreciate if the figures could consistently include labels for the applied CAPE threshold (e.g., as panel 5b).
15. Figure 6: I apologize if this pertains to our printer, but I can hardly distinguish the colors for (i) ERA5 (1000 $\mathrm{J\,kg^{-1}}$) and ERA5 (spatial DS) as well as (ii) ERA5 (w/o ECP) and ERA-Interim (with ECP).
16. l. 270-296: Did the authors consider to move this paragraph to the Methods Section? See general comment 2.
17. l. 256 ff: Please cite the relevant literature here.
18. l. 256 ff: Comment related to vertical mixing: To streamline the manuscript, I believe the effects of vertical mixing could be removed. In particular, as no detailed information on potential tuning and the choice of time interval is provided and as it is not applied afterwards.
19. l. 265 and 354: Please consistently replace 'equilibrium level' by 'EL'.
20. l. 300: Please rephrase 'vertical layering' (e.g., stratification).
21. l. 315 f: Given the property of the underlying wind fields, this general statement is expected.
22. Figures 7 and 8: I think it could be sufficient to show January and July (e.g., move April and October to Appendix / Supplement) and show differences between Fig. 7 and 8 (similar to Fig. 9 showing differences between ERA5 and ERA-Interim), as all sub-panels look very similar.
23. l. 338: 'other months show similar results': They could be added to an Appendix (see also comment above).
24. l. 344 ff: Based on the global distribution of CAPE it is expected that the frequency of convective events decreases with increasing CAPE threshold. Please streamline this paragraph and/or move it to Section 3.2 (Statistics of explicitly resolved and parametrized convective updrafts).
25. l. 352 f: 'Moderate to strong convective events [...] play a major role in affecting the zonal mean e90 distributions throughout the troposphere': Based on Fig. 11a, I disagree with this statement. The figure suggests that up to a CAPE threshold of 5000 $\mathrm{J\,kg^{-1}}$ only minor differences in zonal mean e90 distributions arise.

26. l. 360: The authors mention an 'improvement' of the ECP simulations. What is the chosen reference to improve, how is this quantified?

27. l. 362-365: Parts of this paragraph rather belong in the introduction. Please streamline.

28. l. 373 ff: I find this paragraph rather suitable for Section 3.2 (Statistics of explicitly resolved and parametrized convective updrafts) as it does not relate to tracer distribution but considers convective event frequencies (see comment above to l. 344).

29. l. 383 ff: I find this information quite relevant and would appreciate if it could be placed in the Methods (see general comment 2).

30. l. 390 ff: Could the authors please elaborate on why sensitivity tests are performed without ECP although results suggest substantial underestimation of convective transport without ECP regardless of the driving dataset (which is also expected given that the applied re-analyses apply convection parameterization).

31. l. 411: Typo: 'It requires only on two input [...]': Remove 'on'.

32. l. 417: Please rephrase 'possible improvement' (see comment above on l. 360).

33. l. 420-440: Given the properties of the underlying wind fields, this is expected. Please see general comments.

34. l. 440-441: Please rephrase.

35. General comment on Conclusions: I would appreciate a thorough and differentiated discussion of the sensitivity experiments. For example, on the relevance of convective thresholds for local vs. global frequencies of convective events, and for the representation of tracer distributions locally versus globally averaged.