Review of "Precipitation, moisture sources and transport pathways associated with summertime North Atlantic deep cyclones" Stoffels et al. submitted to WCD

This paper adopts a cyclone-centred perspective to evaluate the moisture sources and transport pathways of North Atlantic deep cyclone precipitation in summer. The paper strongly builds scientifically and methodologically on a previous paper (Papritz et al. 2021) focussing on winter deep North Atlantic cyclone precipitation. The paper is well written and the main findings are interesting and related to

i) moisture residence times being relatively constant of about 4 days throughout the cyclone life cycle;
ii) the moisture sources of cyclones originating from the tropics and making extratropical transition being mainly located in the subtropics and midlatitudes, with very limited amounts coming from the tropics directly
iii) contributions from different key geographical and cyclone-relative regions, such as land, the warm side of the Gulf Stream as well as evaporation from the cold sector of preceding cyclones, although the discussion mainly relates to geographic regions and not quantitatively to cyclone-relative regions.

I have a few minor comments mainly related to the writing and presentation of the results.

1) Innovation: I think the paper could become a bit sharper in terms of its innovative contributions to science. In my reading, I got the impression that it was very closely following the preceding paper by Papritz et al. 2021 both in terms of scientific focus and methodological approach. The fact that summer deep cyclones are generally less studied has good reasons, they are rarer and less intense than winter cyclones. Therefore, the motivation for this study could be carved out a bit more convincingly. I do think there are good reasons to investigate summer cyclones separately, e.g. to investigate the dynamical impact of added moisture from the land sources, the role and moisture transport pathways related to cyclones from tropical origin, potential similarities with future warmer conditions with weaker baroclinicity also in winter, contrasts between cyclones over the ocean vs. over land… I encourage the authors to make a stronger case for their paper in the abstract and the introduction (e.g. at L. 51-55). The fact that summer cyclones are less studied does not make it a good reason to study them.
2) L. 62: WCBs here I think Madonna et al. 2014 and Heitmann et al. 2024 should be referenced. And at L. 73 about the link between WCBs and cyclones: Binder et al. 2016.
3) WCBs: throughout the paper the authors should be much more cautious with their definition of the WCB and clearly define what they mean. Usually, this airstream is defined as ascending by 600 hPa or more in 48 h. It is very likely that in summer the airstreams are ascending less (see also the substantially lower frequency of WCBs in summer over the North Atlantic). Also, the convective parts of the ascent are probably missed in the approach chosen by the authors (Oertel et al. 2021) calculating the trajectories based on 3 hourly 3D wind fields on the still relatively coarse ERA5 grid.
4) L. 76-87: I don't understand the use of discussing atmospheric rivers in such great detail since they are not identified or discussed further in the results of this paper. I would suggest shortening and shifting the discussion on the potential role and link with ARs to the conclusion.
5) Q2 is not addressed in a cyclone-relative way. I do think that the authors would have the necessary data and tools to address this question, with a bit more coding work and gridding the uptakes for different times relative to cyclone maximum depth.
6) L. L117: Here the Lagrangian method used should also be referenced (Sodemann et al. 2008).
7) L. 120: these are not adequate references for the use of Lagrangian moisture source identification to distinguish between different air streams. For example Pfahl et al. 2014 could be a good option.

8) L. 123: if the North Atlantic was studied several times before, then mention several studies. Here maybe e.g Gimeno et al. 2012, Perez-Alarcon et al. 2022 could be good options.
9) L. 160: "Thereafter a new time axis … is defined", I think it's not a new axis, just a new time of reference.
10) L. 168-170: Why did you exclude these cyclones? Some of them might also have made extratropical transition and actually be quite interesting to study in more detail.
11) L. 181: "exhibit strong movement" what does this mean exactly?
12) Fig. 1: add contours of track density to help interpret Fig. 8.
13) L. 202: add Wernli and Davies 1997 for LAGRANTO to reference the original publication as well as.
14) L. 212: justify the 8-days based on studies about the moisture residence time in this region and season.
15) L. 214: "LAGRANTO's ability to allocate a significant portion of precipitation to the right moisture sources" not sure I understand what you mean here. Do you mean Watersip instead of LAGRANTO? And how do you know what the "right sources" are?
16) L. 219: Did you use the official Watersip code? Or an own implementation in which case it would be clearer to simply reference the original publication of the algorithm with Sodemann et al. 2008 and not call the algorithm Watersip. For reproducibility it would be easiest if you provided a link to the code used.
17) L. 239: in the 48 h around maximum cyclone depth.
18) L. 255: at arrival in the cyclone (trajectory start is confusing).
19) L. 370: I would not call this the upper troposphere, there are very few trajectories coming from above 500 hPa.
20) L. 376: implying that local moisture recycling is becoming important: what does that mean exactly?
21) L. 415-416: here Aemisegger and Papritz, 2018 would be more fitting.
22) L. 453: It's not clear what the initial moisture is: the diagnosed precipitation?
23) L . 465: I think this is really an important point of this paper. It should be emphasised more. This contradicts the usual assumption that in cyclones from the tropics making extratropical transition, subtropical or even tropical moisture gets exported into the midlatitudes.
24) L. 469-472: this raises the question of cyclone-relative sources, which is not addressed quantitatively in this paper.
25) L. 474: Here "local" needs to be defined more quantitatively. Local relative to the cyclone?
26) Fig. 9b: I think panel b does not make much sense given the relatively large North-South temperature gradient.
27) Fig. 9d: make clear that panel d is based on the explained fraction.
28) L. 526: "…hinting at cold-air advection" I think this is speculative.
29) L. 545: then I would say they are not WCB trajectories. What is the role of convection and the relatively coarse spatial and temporal resolution of the ERA5 data?
30) Section 4.5: the discussion in this section is a bit speculative without cyclone-relative analysis of the moisture sources.
31) L. 570: "… facilitating strong upward latent heat fluxes": yes and what matters even more for your study, is that given that the subsiding air is dry, it's efficiency in taking up humidity is large (Aemisegger and Papritz 2018).
32) L. 571: here maybe Illotovitz et al. 2021 about the impact of the dry intrusion on boundary layer dynamics and surface fluxes would be a good reference.
33) L. 657: The fact that you do not find a feeder airstream is due to the fact that you do not look at the moisture sources in a cyclone-relative perspective. Without such a cyclone-relative analysis (which I do think is feasible and which I would strongly recommend), you cannot really make this statement. If you really do not find a "feeder airstream" in your summer cyclone based on such an analysis this would reveal an interesting contrast and rise new questions about the cyclone-relative moisture cycling in summer vs. winter cyclones.
34) L. 688: "… or from the developing cyclone's own cold sector (which appears to be more important in summer)", this sounds contradictory with the explanation at L. 573ff, where the authors discuss that this moisture recycling pathway is unlikely due to the necessary cross-cold front motion of the air parcels that would rain out in the same cyclone. I recommend to rephrase this according to the statements made earlier in the paper.

References:

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