Weather and Climate Dynamics Discussions

Weather Clim. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/wcd-2022-36-RC1, 2022

Comment on wcd-2022-36

Anonymous Referee #1

Referee comment on "The three-dimensional structure of fronts in mid-latitude weather systems as represented by numerical weather prediction models" by Andreas Alexander Beckert et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-36-RC1, 2022

In the manuscript "The three-dimensional structure of fronts in mid-latitude weather systems as represented by numerical weather prediction models" document an implementation and slight adaptation of an existing 3-dimensional front surface detection algorithm in the context of the interactive analysis software Met.3D. The authors then showcase their implementation for two case studies of autumn/winter storms over Europe and relate their detected fronts to other (mostly very well established) meterological concepts.

The manuscript is quite comprehensive and touches upon many aspects around mid-latitude cyclones and their fronts. With the wide variety of aspects covered, it however remains somewhat unclear what, in its core, this manuscript is about. Discussions generally remain superficial and don't add much new to the literature except further anecdotal support for otherwise already very well established meteorological concepts (i.e., WCBs, Shapiro-Keyser cyclone model). Because of this overall lack of direction and novelty in the discussions, the manuscript appears in its present form to be mainly an advertisement for Met.3D. I recommend the editor to reject this manuscript, and the authors to submit more focused and targeted analyses/discussions on any or all of the mentioned topics individually.

More specifically:

(1) The front surface detection seems to be only a minor modification/optimisation of the algorithm introduced and implemented in Kern et al. (2019). Judging from the illustrations in Kern et al. (2019), the algorithm was already then implemented in Met.3D. Yet, the algorithm is introduced and discussed here in as much detail as if it was new. Further, the authors "validate" well-established meteorological concepts such as the Shapiro-Keyser cyclone model and the spatial relation between WCBs and fronts using their visualisation and front detection algorithm. Given how successful these concepts have been over decades, I find this quite assuming. If these concepts had failed to show up in their

analysis, I would much rather doubt the implementation and visualisation in question rather than these meteorological concepts. Now, given that everything looks as expected, I am unsure what to take away from the "validation" beyond that the algorithm and visualisation is working fine---and so much that had already been shown by Kern et al. (2019).

(2) The authors discuss briefly the best choice of thermodynamic variable for the front detection. This choice remains an subject of debate, and a new perspective on this choice could warrant another publication. This would however require considerable additonal analyses; based on only two case studies, the authors are not in a position to give general recommendations (as presently done in the summary and discussion section).

(3) Similarly, a front classification into humidity and temperature-dominanted fronts would most likely be worthwhile and well warrant a publication. But this aspect is discussed by far too superficially to justify the publication of the present manuscript.

(4) Similarly, the comparison of WCBs and frontal structures in parameterised-convection versus convection-resolving models is both timely and interesting. It would certainly warrant a publication of its own. But again, this aspect is discussed by far too superficially here.

I would very much encourage the authors to extend their analyses in particular on the topics in issues (3) and (4) and to submit manuscripts with in-depth analyses on these. The 3D front surface detections will surely certainly be helpful for either.

Finally a comment on the frequent use of 3D visualisations in the present manuscript. Besides the 3D visuals, I find the visuals and text to be clear. Met.3D is undoubtedly a powerful tool for the *interactive* and *exploratory* analysis of a meteorological dataset--the interactive demonstration in video supplement 3 shows that clearly. At the same time, as static images on (digital) paper, I don't find the shown 3D visualisations useful. Figure 3 in the manuscript is a good example: Everyone who has looked at weather charts before will be able to grasp the chart in panel (a) within fractions of a second and have a clear impression of the synoptic situation. This would still be true even if additional lines were plotted to depict the intersection of the frontal surface with different vertical levels. But it is not true for panel (b), where I remain unsure about the frontal structure and synoptic situation even after looking at the panel for minutes. The main cyclone core is not visible behind the front surfaces, so I need quite some mental energy to disentangle the occluded from the cold front (which some ambiguity remaining) to then infer with my meteorological intuition that there should be another cyclone core hidden somewhere---an inference that I couldn't be sure about without the verification in panel (a). Because, given only panel (b), may be the occluded front is actually a secondary cold front and the only cyclone core in the chart is the one close to Iceland, the one that is peaking out behind the occluded front? Essential aspects of the 3D structure can still be conveyed in 2D maps; I would find such maps much less ambiguous, easier to parse, and thus more suitable for a static publication.