Response to Second round of reviews GMD-2022-255

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Reviewer 1

- Title/Abstract: The new title puts the focus on the climatologies when they are more a by-product of the improved methodology. The abstract especially is missing any climatological results. Please remove this discrepancy by adapting the title (replacing the colon by "and" would already go a long way) and Abstract (add a couple sentences on the climatological results, if necessary at the expense of some detail on the methodological adaptations).

Title has been changed as suggested.

- Figures: Add "UTC" to datetimes in the captions.

This has been added as suggested.

- Figure 2: The legend to me implies that each line represents a season (e.g., DJF in blue), so I first thought three years were shown, when in fact each line corresponds to one month but the three months of each season are colored the same. Listing each month separately in the legend would resolve that. Also, the horizontal dotted lines are almost invisible (printout); change that, and list the latitude values in the caption.

We have changed the caption to make this clearer. The horizontal dashed *lines appear very clearly when I print out the page, so we have kept the lines as they are, so as not to add extra clutter to the figure.*

- Figures 7/10: Consider shortening the caption to "... (a–d) cold fronts and in (a,e) DJF, (b,f) MAM, ...". Either way, don't unnecessarily capitalize "cold fronts" and "warm fronts".

We have edited the caption as suggested, and have not added any unnecessary capitalisation.

- Lines 5/6: "Smoother fronts with fewer breaks" doesn't necessarily sound like a drawback of the original method. Please reformulate to express why "distorted fronts with many breaks" are actually more desirable (as implied by the sentence).

The reviewer is correct that we believe smoother fronts with fewer breaks is desirable. The "original method" referred to here is the Hewson method of contouring. We have edited the text to make this clearer.

- Line 15: Add references for "modelling" as well as more than one for the "numerous" case studies.

Added additional references and dropped the "numerous"

- Line 78: Consider introducing an acronym for "Hewson (1998)", e.g., "H98", given how often it's referenced in the text.

We have done this as suggested.

- Line 103: Use "ABZ" or don't define it in the first place.

Changed as suggested.

- Line 109: Use "K3" in Equation 4 (like K2 in Equation 3).

Changed as suggested.

- Line 115: The descriptions of the two approaches are a bit hard to follow. Refer to Figure 1 at the beginning of each description so the reader is aware of this visual aid while reading the test. Also, consider naming the equations when referencing them (e.g., (1) TFL, (2) TFP, (3) "ABZ", (4) "front speed"; "... that satisfy te TFP Equation 2 to form a mask (the ABZ criterion in Equation 3 is ...") so the reader is spared from memorizing the equation numbers or jumping back and forth in the text.

We thank the reviewer for this suggestion to make it easier to read the text. We have added these names to the description as suggested.

- Line 156: The presented method uses "three parameters", but that's not necessarily true of "front identification" methods in general.

This has been corrected.

- Line 199: Capitalize Northern/Southern Hemisphere (here and elsewhere).

This has been corrected.

- Line 206: Add references for "previous studies".

Added several additional references.

- Line 273: To what degree is the "increase by almost 100%" due to larger vs. newly identified fronts? Add an estimation if you can provide one, or at least mention that both effects play a role (assuming that's the case).

This is difficult to quantify, so we have mentioned both effects as suggested.

- Lines 288/289: I had to look up "horse latitudes". Consider a more common term (unless you deem this common knowledge). Also, define "ITCZ" at first use (currently the definition is on line 309).

ITCZ is now defined earlier. The latitudes are now given along with the phrase "horse latitudes".

- Lines 294/295: Please discuss why cold fronts are more common in SH summer than winter when the opposite is true in the NH (while frontal precipitation nevertheless peaks in SH winter; see editor's remark and your response to is).

We have added a reference to Berry et al. [2011] and Satyamurty and de Mat-

tos [1989]

- Line 308: It is unclear whether the increase is in the range of 20–40%, or whether the increase is by 20% from 20% to 40%. Please reformulate.

This has been changed to read "where front frequency increases by between 20 % and 40 %."

- Discussion: Please revise the text, as it's in a markedly rawer state textually than the rest of the paper.

It is difficult to know exactly what is required here. We have tried to make the text more concise.

- Line 328: Given NCL is mentioned for the old implementation, also mention R for the new one.

This has been added.

- Lines 342/343: Either remove the sentence "Computational performance ...", or move it to the earlier paragraph where the performance improvements are discussed.

This has been removed as suggested.

Reviewer 2

This is the second review I have provided for this manuscript. As such I can first say that it is good to see that the record has been put straight regarding correct attribution of methodologies for creating objective front plots, following on from extensive comments in the first review round. Another big plus for this revised paper is the supplementary material, which supports the main scientific (methodological) achievement of the paper, namely the use of the climatological quantiles to define the masking criteria; accordingly more pointers should in my view be made to this within the main text.

We thank the reviewer for their positive assessment of the revised manuscript.

We have made more reference to the supplementary material as suggested.

My primary comment now concerns the description of the motivation and objectives of the paper. In terms of scientific/meteorological content the paper does not go far into describing or explaining the features on show from the different re-analyses – e.g. Figure 10 attracts but one short paragraph of somewhat sketchy comments. Indeed such content, overall, falls short of what would be needed for acceptance in a "standard" meteorological journal (e.g. Monthly Weather Review). On the other hand, the coding and threshold setting aspects, and thereby the provision of a tool for the community to use do fit better the aims of GMD, which can justify publication there, and so the authors should be clearer on that aspect from the outset, stating this more clearly as the purpose of the paper (in the abstract and, within the introduction, at the start and not just at the end).

We have stated the main aim of the paper early in the abstract as suggested.

Following on I really would like the authors to rewrite the abstract. It is particularly jumbled at the moment, and will in my view put readers off! This jumbled nature is a legacy of poor attribution of previous work in the first submission, and incomplete attempts to address that in the wording this time. For example: both "implemented a number of changes to a previous implementation" and then "previous implementation used a different order compared to the original algorithm" are confusing, especially taken together. You might like to say something like "we have resurrected the original methodology, used this as the basis for a new and modern open source code implementation, and show how this delivers output which in various ways is rather better than a previous code implementation which was unwieldy, slow to run, and deviated too much in its methodological approach from the original work". By all means then go on to reference the additional changes you have made, but I stand by my comment from the first review that "more accurate finite differencing" is not really your work. Neither is it innovative, or new, and so it should not appear in the abstract as if it were.

We thank the reviewer for the comments and have now edited the abstract to (hopefully) be clearer.

Related also to the additional changes, Section 3.4, on numerical updates, is problematic for various reasons and needs a bit of a re-write (details below). The remaining comments I provide below should generally be fairly easy to address, but are nonetheless important for final acceptance, in my view.

We have responded individually to the comments below.

Other points 1. L1 - "are important for their..." is poor English; please reword.

Changed.

2. L14 - what is "large proportion of total and extreme precipitation"? This is poor English.

Added the word "both".

3. L24 - in the view of most I think 1998 would not be classified as a recent year.

Have removed "in recent years".

4. L34 - not sure this can be "the final piece" of the puzzle as you go on to criticise this.

Changed.

5. L44 – you cannot really introduce "the threshold" in this way without saying what it is. Readers will be confused.

This sentence has been removed.

6. L48 – "shown lead" – English error.

Corrected.

7. L57-58 – "The aim" ... "as implemented by Berry et al" is not really the aim here. You are actually ditching the Berry et al implementation in many ways. Please correct. This is a really important point as indicated in "summary / main points" above.

We have changed these lines.

8. L90 – "of the equation 1"; delete "the"

This has been corrected.

9. L104-5 – it may require additional tuning in very high resolution datasets. This is an interesting comment which I don't think I agree with. Anyway to justify inclusion you would in my view need to say more about why you think this is the case. Otherwise leave this out.

This has been removed as suggested.

10. L109-111 – you need to say why you use the -1.5 to +1.5m/s range for quasi-stationary.

It is stated at the start of the sentence that we have followed Berry et al.

11. L155 – "usually necessary" – surely it's always necessary to define what you are doing!

We have added the word "subjectively".

12. L155-156 – "it is still necessary to provide certain configuration settings" would be better here; using the term "parameter" here is a bit confusing.

See previous response.

13. L164 –To qualify your statement that you particularly want to avoid "unwanted" local extrema in the TFL you do need to say why you expect them. You deal with this in the next paragraph, so maybe some text rearrangement here would be the right approach.

The text has been rearranged as suggested.

14. L170 – It think you can replace "may in part be" with "will in part be".

Changed as suggested.

15. L171-2 – designed to "quell the amplification of frontal curvature" would

be a more accurate reflection of the contents of that paper.

Text has been changed as suggested.

Also, I really would strongly recommend that you reference the frontal "hook" seen south of Iceland on Figure 3d as a very nice example of the detrimental impact of not using equation (6). This looks (and would be) important locally, for forecasting applications, but is very probably not for your 'climatological' purposes, which gives to my mind a nice visual justification for your simplified approach.

We appreciate the reviewer's suggestion regarding the hook seen in the frontal structure south of Iceland. We have thought very carefully about this. Looking at Figure 8 in Hewson [1998], there are some similar hook type structures identified even when using Equation 6 from that paper. As the more stringent masking criteria are applied, these hook features are removed. We don't believe it is possible to say for sure that the hook feature seen in our Figure 3 would not be present if we had used the Hewson [1998] Equation 6.

16. L159 – practically, such charts will never all be produced by a "single meteorologist", but rather a set of meteorologists working shifts (which of course all bring their own, sometimes different, subjective judgements to the table).

The text has been changed slightly to reflect this.

17. L158-9 and L187 – Is there a bit of a contradiction here; first you criticise the cross-referencing to charts, yet then go on to do that yourselves?

We have added a sentence of qualification "While comparing to charts is a necessary check of an objective algorithm, calibrating in this way..."

18. L182 – "a threshold of..." – for what parameter? K1 or K2?

K_1 *has been added here.*

19. Fig. 2 caption: should say what period, in years, this data is for, and what the input dataset what, at what resolution and with what smoothing.

The time period and input dataset and smoothing level have been added.

20. L227-228 – you used January 2000, then say that was consistent with Figs 1 and 3, but those figures are for 2001.

We thank the reviewer for pointing out this typo, January 2001 was used.

21. L236-238 – the explanation of the del-squared computation is incomplete / confusing / incorrect. Surely it's one gridbox either side? There are no "components" as such to del-squared.

We thank the reviewer for helping us clarify this area of the text. Yes it is only one grid box, that sentence was a hangover from when fourth order accurate derivatives were considered, and has been removed. And, yes strictly there are meridional and zonal terms, not components, to del-squared and this had been clarified in the text.

22. L239 – what are the edges of the domain? I didn't think a global domain had any edges?

In Cartesian coordinates, the east/west edges of the domain can be handled by wrapping, but the north/south edges cannot, and if it is not necessary to run the analysis globally, a smaller area may be considered. We believe the readers will understand this.

23. L239-242 – the fact that thresholds are supposedly "very small" does not in any way mean that we need accurate and stable schemes. Small is merely a function of the unit. If we were working in microns they would not be nearly "as small". So this sentence does not make sense and I really don't know what you are trying to say here. Please re-think.

This sentence has been removed and the computational efficiency clarified.

24. L243-245 – This sentence does not make sense. I am not quite sure what you aim to say here, but it would at least be best to start by referring to the "standard relative humidity parameter provided by ECMWF which accounts for ....". Then go on to say (I think!) what the NCL code requires as input.

The intention was simply to be transparent about a change in methods. The text has been updated to clarify this point.

25. L245 – How did Berry in 2011 use a tool from 2019?

The reference for NCL has been backdated and moved to where NCL is first mentioned.

26. L250 – why250km?

This is a subjective choice and a sentence of explanation has been added.

27. L259-260 – there are two warm fronts here. Please clarify for reader's which one you are referring to (it's evidently the southern one).

Clarified in text.

28. L261 – "suggest..." - I think "use..." would be a more accurate reflection of this paper's contents.

This has been changed.

29. L271 – the "greatest increases". This is incomplete / misleading / wrong. First you need to define what you mean by increases; reference to figures 4e and 4f suggests that you mean in percentage terms, yet the areas you highlight are more like minima, notably on Fig 4e. Then if you do mean in absolute numbers we can't see that clearly. I think this issue may be a legacy of the figure having been changed to a percentage change, following the previous review round, but the text not made compatible. So this clearly needs fixing.

Text has been updated to reflect the shift to percentage changes

30. L272 – whatever way you look at it, I don't think this can be said to be evidence of "the effectiveness of the contour-then-mask approach..". We have already seen evidence of that aspect, so that should in my view be enough and I would leave this statement out.

We have removed this statement as suggested.

31. L273 – by almost 100%. This is not correct. My inspection gives about 40% (from about 6 to about 8.5).

Thank you for pointing this out. We have changed this to say between 40 and 80%, and referenced the figure panel where this is seen.

32. Figure 4 caption line 1: English - please change to "...front frequency in % (using 6-hourly frames from ....)." reminding users what period is under investigation from what re-analysis after the "from".

This has been edited as suggested.

33. Figure 4e, f, caption, Figure 5 caption and other locations – whenever you refer to percentage difference/change it's vital to say how that is computed, as various approaches are possible. For Fig. 4e for example is it: (100*(c-a)/a) or (100*(c-a)/c) or (100*(c-a)/(2*(a+c))) or some other variant. And then one could also ask what happens when the denominator is nil.

We have added this information. There are no grid boxes where the denominator is zero.

34. L275: maybe you should refer to Figure 6 here first, before discussing Figure 5 directly, as it provides input?

Figure 6 has now been referenced here.

35. L280: edges would be better than edge.

Edited as suggested.

36. L294-295. It would be nice to see some discussion of possible reasons why there is a difference between the SH and NH - i.e. in the NH we clearly have more cold fronts in winter than in summer.

We have added a reference to Berry et al. [2011] and also to Satyamurty and de Mattos [1989] regarding the frontogenesis locations.

37. L295-296. I am not sure what you are trying to say here – it's ambiguous. Do you mean to imply that on average the storm track moves poleward in SH winter. If so I don't agree that that is the case. One can see for example in Hoskins and Hodges (2005) that the storm track zone expands equatorward in the SH winter. Or maybe you mean that there are specific occasions that the storm track strays poleward in SH winter that lead to a higher warm front

density near Antarctica than in SH summer. I don't think that that would be true either though, and I think the cause may actually have something to do with sea ice and/or katabatic drainage, but I am not sure exactly what. Anyway, please clarify or remove.

We have edited this sentence to remove the implication that the storm track moves polewards.

38. L306. "...more fronts are identified...". Any thoughts on why this is the case?

This could be related to the way the aggregation is done. The higher resolution dataset could have two individual fronts identified that pass through the same larger grid box. In the lower resolution dataset this would count only as a single frontal point. We have added a line of explanation: "Since aggregation is performed by counting individual fronts, this indicates that ERA5 is able to resolve more fronts due to its higher resolution".

39. L307. Any idea why there might be a reduction over high orography?

This is likely related to the better representation of orography in the higher resolution dataset.

40. L308. "20% to 40%" must be changed to "20 to 40%" otherwise the meaning is wrong.

This text has been changed.

41. L308-310. So what are the ITCZ fronts? The ITCZ is not a place where one generally expects to see fronts, I think, or am I missing something?

These could be similar to the fronts identified in the South Pacific Convergence Zone, with strong temperature gradients. We have added that the numbers are still very small so this large percentage increase is still a very small absolute increase in the frequency. We have also referenced Figure 9d here instead of later.

42. L313-314. "would typically pass through" is the correct terminology.

This has been edited as suggested.

43. L315. It would be much easier to see this if you made the scale divisions on Fig 9 equal to one- third of the size of the divisions on Fig 6, instead of one quarter! Please think of the reader!

We prefer not to have the scale divisions at 1/3 etc. It is only an approximation, and therefore we have chosen to keep the scale as it is.

44. L321. SST fronts would be fronts in the ocean. Therefore please clarify that you are referring to atmospheric fronts which are to some extent indicative of underlying or nearby SST structures, with the connecting physical mechanism being vertical fluxes of heat and moisture.

We have now mentioned that the high frequency of atmospheric fronts associated with the SST fronts are visible.

45. Figure 8a, b and Figure 9a. Whilst the colour schemes adopted on the figures are overall a big improvement on the previous manuscript version, in these panels we have descended into some sort of "multicolour mayhem" making visual interpretation difficult. I see you have a scheme from ColorBrewer (I found it here: https://en.wikipedia.org/wiki/ColorBrewer). I think this particular scheme would be recommended for use with multiple paired classes, not for a data continuum that you have. One particular issue is that red draws the eye, and here it represents mid-range values. Unless there is a particular physical reason to use a particular colour in a particular part of the range (which is not really the case here) then brighter colours like red should usually be reserved for more extreme values. There are a couple of recorded presentations here on colour usage that might be of interest: https://vimeo.com/717994549/76f08433b8 and https://vimeo.com/718387621/4ec07e604b.

We thank the reviewer for the helpful comments and the links to the presentations. Since the reviewer believes the figures are improved from the last version, and so as not to have to reproduce every figure, we have elected to keep the colour scheme as it is.

46. L 342-343 – This sentence duplicates what was said a little earlier on and can be deleted.

This has been removed.

47. L347 – deleting "of the masking variables" would make for a sentence with better English.

We thank the reviewer for this suggestion, but choose to keep in "of the masking variables" so that it is clear where these variables are used.

48. L358-361 – I don't think that moving to higher order accuracy in the finite differencing in this way is really something to be countenanced here. Such schemes have their roots, I believe, in improving the fidelity and accuracy of numerical scheme implementations, where accurate forward integration is of paramount importance, whereas here the goal is replicating at a local scale the instantaneous model-derived picture. There is no forward integration. The higher order schemes inevitably bring into play more data from points remote from the local area, so can contaminate the local picture that the user wants to focus on. Indeed this was in a sense something that you criticised the Berry et al code for earlier in the manuscript. So I would strongly recommend that you either discuss this aspect in full, or just leave this part of the text out. Just having a modest increase in the number of front points identified is, of itself, neither good or bad, so I really don't think there is much useful to say here.

We have removed these points.

49. L364-365 – see bullet point 15 above.

Text has been edited in line with previous comment.

50. I had a quick look at the Zenodo link. Please remember to update the publication link there when ready.

We will ensure we change the link when we are ready.

51. Appendix L8. "of the time"?? =manually produced charts for that particular time? If so which ones, from which centre (there can be a lot of differences between those too, depending on country of origin, which may incidentally also be a point worth making earlier in the manuscript).

We have changed this to "that may be identified by a synoptic meteorologist may not be correctly identified".

52. Appendix L18. Ditto.

We now refer to Met Office charts from that date.

53. Appendix. Perhaps on Figures 2, 3, 4 you could put a box around the plots to show which thresholds you use in the end, to help the reader, or add a big star, or something equivalent?

We thank the reviewer for this suggestion and have highlighted in the caption which threshold is used.

References

Gareth Berry, Michael J. Reeder, and Christian Jakob. A global climatology of atmospheric fronts. *Geophysical Research Letters*, 38(4):1–5, 2011. doi: 10.1029/2010GL046451.

T. D. Hewson. Objective fronts. *Meteorological Applications*, 5(1):37–65, 1998. ISSN 13504827. doi: 10.1017/S1350482798000553.

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