Response to Referee 2 GMD-2022-255

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Reviewer 2

Summary

This paper looks into the issue of simplifying and speeding up objective front identification in re-analyses, with simple application across a range of numerical model types/resolutions in mind, to facilitate intercomparison of (e.g.) resolution impact. The goals are OK, but in my view they have not really been addressed or reached in any useful or useable way. And I do not agree that the main method chosen is the right one. In tandem, the paper has many other weak areas: textual clarity, figure clarity, disingenuous scale selection, result misinterpretation, unsubstantiated claims, etc. This all means that the manuscript does not unfortunately contain sufficient new science, new methods or new results to warrant publication in my view. With regard to the claimed innovations (e.g. in the abstract) these are either minor adjustments, or have been taken from another publication without acknowledgement. This also raises questions of scientific integrity, which is clearly disappointing. There are a few areas where, with further investigative work, a wholly restructured manuscript could potentially reach a publishable standard.

The authors appreciate the detailed comments and insights provided by the reviewer and have addressed them in detail below. In particular, all figures *have been revised with new colour scales for increased clarity, and the scale selection clarified. The mistake in not correctly identifying that the alternative order of operations had already been proposed elsewhere has been corrected. However, we believe that identifying and demonstrating the difference compared to the previous implementation which had been used in multiple studies is still a valuable contribution. The goal of this study was to document a new portable, scalable, open-source software implementation of a widely used algorithm in a transparent manner, and to provide updated climatologies, including new high resolution climatologies. In addition we believe that the proposed quantile based definitions of the front identification thresholds will facilitate easier comparison between datasets of different resolutions, whether or not additional smoothing is applied to make those datasets more similar.*

Detailed comments

Major Points

1. In highlighting changing the 'order of operations' in the abstract you are merely copying the method of Hewson (1998) (his Fig 2), without acknowledging that. Similarly the recommendation to use a contouring algorithm (L111) is what Hewson (1998) proposed and implemented, and you don't acknowledge that either. So whilst these updates do deliver better results, the authors are wrong to imply it is their innovation. Then referencing 'more accurate finite differencing' in the abstract would mean, one would think, that this is something new and very different, whereas it is a second order centred finite difference, which to my mind would ordinarily be the default way to compute del-squared on a grid. This is also rather trivial compared to the extensive work done to compute derivatives correctly in Hewson (1998) – ref: P46 and appendices 1 and 2. So I can't really see how any of these aspects can be justifiably referenced in the abstract. Likewise the other methodological changes mentioned in the abstract - direct calculation of wet-bulb potential temperature and better handling of short fronts – are to my mind small adaptations that should appear only in the main text as minor points, and not in the abstract as if they represented major progress. So virtually all the "key points" of the abstract are

not key points at all, but either copies of previously published work or small algorithm changes.

- The reviewer is correct to point out that Hewson [1998] did indeed originally propose a contour-then-mask approach, and we thank them for pointing out this oversight. As stated in the text, the intention was to create a portable and scalable implementation of the algorithm as implemented by Berry et al. [2011b]. The underlying study by Hewson [1998] was used primarily to understand and check the mathematics of that implementation, but this particular detail of the suggested implementation, however obvious in retrospect, was overlooked. We maintain that we genuinely believed this to be an independent innovation, and not an attempt to take credit for the work of another, but have withdrawn any such claim and correctly attributed the methodology to Hewson [1998]. While this does diminish the originality of the study, we believe that highlighting and demonstrating the difference in implementation and performance still has value.
- We agree that a second order centred finite difference should be the default way to compute ∇^2 on a grid, and list it in the abstract only as one of "a number of numerical improvements in the implementation", which taken together yield substantive differences compared to the previous software implementation, demonstrated in Figure 4d.

2. L159-170. Here you describe the new approach of making different resolution datasets comparable from a front perspective, and this lies at the heart of the paper's aims, particularly with regard to code provision. It is therefore of pivotal importance. You opt to adjust the smoothing whilst keeping the masking thresholds the same. I fundamentally disagree with this strategy, because, as comparison of Figs 3c and 3d shows, you can easily end up 'smoothing to death' in a way that results in the two input fields (and therefore resulting fronts) after smoothing looking almost identical, thereby destroying the whole point of having the higher resolution in the first place. A much better, much more scientifically justifiable strategy would be to do it the other way round; limit the smoothing, but adjust the thresholds, to give somewhat similar front lengths. Then any subsequent intercomparison will

show where frontal frequencies differ because of resolution impacts. In that case maybe there would need to be a bit of latitude on the amount of smoothing, perhaps a bit more for the higher resolution datasets to get rid of contaminating non-frontal noise (and there some subjectivity becomes inevitable I feel). But the very big increase from 8 smoothing passes for ERA Interim to 96 for ERA5 you use goes well beyond, to my mind, what any such latitude should allow. One might argue 96 is on a par with the number of smoothing passes (100) used in Jenkner et al (2010). Whilst one could contest that too, importantly they were using a model with a 7km horizontal resolution, much less than the 31km of ERA5. To conclude my comments here I quote from the manuscript under review: "When comparing analyses from different weather and climate datasets, the most common approach is to interpolate all the datasets to a common resolution, usually the lowest resolution among the datasets of interest. For some features such as fronts that are more easily identified in higher resolution data, this can be limiting". To me applying very heavy smoothing is just as limiting.

(a) We agree that ideally fronts would be identified at the native resolution of the dataset in question and with minimal smoothing applied. However, we found that without extensive smoothing, the amount of noise in the data was excessive and could not be addressed by careful choice of thresholds alone.

(b) The smoothed fields in Figures 3c and 3d are almost identical after smoothing, as would be expected after matching the climatological quantiles of the masking variables. However, Figure 8 demonstrates that more fronts are identified in the higher resolution dataset, even after smoothing, in line with the findings of Parfitt et al. [2017] after interpolation to lower resolution.

(c) Although the study by Jenkner et al. [2010] used a higher resolution model, it also used a different thermal variable, and a different identification method which placed fronts inside the baroclinic zone rather than on the edge, making a direct comparison difficult.

(d) Smoothing is certainly not a perfect solution, however it does have the advantage of allowing front identification to be carried out at the native resolution of the dataset being analysed, which can be useful for linking fronts with other phenomena such as precipitation, and facilitates comparison between datasets without introducing additional subjectivity through the choice of new thresholds, as we have demonstrated.

Other Points

1. Title: Does not seem to reflect the contents of the paper. It gives the impression, to me, that this is a case study paper, which it is not.

   We have changed the title to "Objective identification of meteorological fronts: climatologies from ERA-Interim and ERA5"

2. L32-33: Why is placing the front in the middle of a frontal zone, and on the warm air side of it, "very similar". Seems pretty different to me.

   Changed from "This results in the fronts lying in the centre of a frontal zone, rather than at the leading edge as a synoptic meteorologist would typically put them. A very similar method was developed by Berry et al. [2011b], who directly applied the methods of Hewson [1998] to gridded data at $2.5° \times 2.5°$ *resolution, placing fronts on the warm side of the strong temperature gradient." to "This results in the fronts lying in the centre of a frontal zone, rather than at the leading edge as a synoptic meteorologist would typically put them. Berry et al. [2011b] directly applied the methods of Hewson [1998] to gridded data at* $2.5° \times 2.5°$ *resolution, placing fronts on the warm side of the strong temperature gradient."*

3. L55: What is a "contemporary high resolution re-analysis". ERA5 at 31km resolution or, say, CERRA at 5.5km? Or even higher still. I would be inclined to say that means convection-resolving, which might mean of order 2km or less. So this evidently needs clarification.

   Changed from "that is able to scale to contemporary high resolution (re-)analyses." to "that is able to scale to contemporary (re-)analyses with horizontal grid-spacings of $0.25°$ *or less."*

4. L65: How do you use the u and v values at 850mb to compute front speed. If it is as per a previous publication then cite and say so as a minimum, but ideally expand here.

   This is already fully explained on Line 95, complete with citations, but cited again for completeness.".

5. L75-77: Hewson shows this option but then highlights the limitations of this approach for curved fronts, and accordingly discards. Please take care to not imply otherwise.

   We have added the following: "In practice, most atmospheric fronts are curved and not simple one-dimensional objects. Hewson [1998] derived an alternative (their Equation 6) to Equation 1, based on the computation of "five-point mean axes", designed to mitigate the effects of frontal curvature on the evaluation of Equation 1, which can lead to noise and exaggerated frontal curvature. Although the alternative definition was preferred by Hewson [1998], we keep the definition in Equation 5 for compatibility with Berry et al. [2011b] and the numerous studies which have utilised that implementation. However, the option to use the alternative definition may be included in a future version of the code documented by this study."

6. L75: "..as used in Berry.." – how do you know - you need to say. There is no reference in this Berry et al. paper to what method they have used. Same comment applies to lines 171-172: "used repeated applications...". Again how do you know?

   The authors have access to the original code written by Berry et al. [2011a] and subsequently applied in Catto et al. [2012] among other studies. This has been clarified in the final paragraph of Secion 1 "The code developed by Berry et al. [2011b] and applied and applied in those subsequent studies was originally developed on the European Centre for Medium-Range Weather Forecasts' (ECMWF) ERA-40 reanalysis [Uppala et al., 2005] at $1.125° \times 1.125°$ *resolution, and later applied to the ECMWF ERA-Interim [Dee et al., 2011] reanalysis at* $0.75° \times 0.75°$ *resolution. However, that implementation was not easily portable due to* *being written in a mixture of NCL and Fortran, and would not scale to the ECMWF ERA5 reanalysis at* $0.25° \times 0.25°$*, or other high resolution datasets."*

7. L78 & L84 & L91: "For a one-dimensional front (Type 1 front in Hewson, 1998)" would be better than "in one dimension".

   We have changed this as suggested.

8. L91: Not necessarily 1/root(2).

   Expanded to: "The value of $m = 1/sqrt2$ *was suggested by Hewson [1998] and we found it to be effective at the resolution of ERA-Interim (*$0.75°$*) and ERA5 (*$0.25°$*), but it may require additional tuning in very high resolution data sets."*

9. L91-93: Sentence means nothing. Please re-write from scratch.

   Rewritten as "For a one-dimensional front, this criterion states that the magnitude of the gradient of θ_W *must be greater than* K_2 *at a point a fraction of a grid length in the direction of greatest increase in the gradient of* θ_W*, i.e. inside the adjacent baroclinic zone."*

10. L97: Why the superscripted T?

    Standard notation for vector transposition, but only important for computation so removed to avoid confusion.

11. L109: Degrees of what?

    The Euclidean distance based on degrees of latitude and longitude, clarified in text. This is obviously a poor choice due to the variable physical separation between parallels with latitude.

12. L117: "Moderately high resolution analyses such as ERA-Interim" – by today's standard this is low resolution. See point 3 also.

    See response to next point since, same sentence.

13. L118: "..is often narrow, frequently only one grid box wide..". I suspect this would apply across many resolutions. If you don't agree you have to provide clear evidence to justify this statement.

Changed to "At or below the $0.75° \times 0.75°$ *resolution of ERA-Interim, the region that satisfies Equation 2 is often narrow, frequently only one grid box wide."*

14. L110-125: This is very jumbled. It looks from the figure like you are using contouring-based colour-fill to mask out potential fronts that don't meet the masking criteria, but in the discussion you focus on using contouring to represent the locating diagnostic. The reader is left confused. And for a fair comparison – versus Berry et al – surely you should include the "search radius larger than one gridlength" on Fig. 1?

Contours have been omitted from Figure 1a and references to zero contours of the Equation 2 have been removed from the description of the mask-then-join approach in order to avoid confusion, and the description expanded. The search radius is used in Figure 1 and an example is visible and now described.

15. Figure 1b: This is nothing new. It looks basically the same as in Hewson (1998). If you think otherwise then a detailed and convincing description of why it is different needs to be added.

We have corrected our previous oversight and correctly attributed the contour-then-mask approach to Hewson [1998]. However, we believe there is value in demonstrating the difference between the two approaches, which has now been clarified further in the text.

16. L128: "key parameters" is vague. Need to be much more specific; presumably you mean "tuning thresholds for masking diagnostics"?

The sentence does not refer to front identification specifically, but to objective feature identification in general, e.g., extra-tropical cyclones, therefore such a specific change would be inappropriate, also the amount *of smoothing is a parameter requiring tuning. The parameters requiring tuning for front identification are immediately clarified in the next sentence.*

17. L135: "local minima and maxima". This may be noise, or it may be a function of you having used the Hewson (1998) del-squared (eqn 5) approach, which is known to amplify frontal curvature, rather than the Hewson (1998) eqn 6, which does not. Needs further investigation and comment.

    A paragraph has been added discussing this and the local front criteria applied by Jenkner et al. [2010]

18. L139-141: Also discussed in the following reference (which is listed in the Jenkner paper you quote). So please acknowledge. - Hewson TD. 2001. Objective identification of fronts, frontal waves and potential waves. In Cost Action 78 Final Report – Improvement of Nowcasting Techniques, Lagouvardos K, Liljas E, Conway B, Sunde J (eds). European Commission EUR 19544. Cambridge University Press: Luxembourg: 285–290. At the same time a simple illustration of the 'cusp' behaviour you describe as smoothing passes increase would add quite a lot I think.

    Citation added to the new paragraph indicated in the previous point.

19. L144-145: There seems to me to be overall as much seasonal variation as there is latitudinal variation (whilst quantifying any difference is of course difficult given the different units, I am referring to differences across the range of values encountered for the two metrics, latitude and season).

    This section has been rewritten and the comment made more specific

20. L146: "relatively constant" – in time or in space?

    In space, this section has been rewritten and the comment made more specific.

21. L148-152: 25th and 50th percentiles seem rather arbitrary values; they also feel higher than I would have expected – giving 25% and 50% acceptance rates across the domain which is a lot. It would be nice to get a fuller picture of different percentile behaviours in some way, with also some example map plots of the diagnostics in two or more colours to show where the proposed criteria are satisfied and how they relate to the input fields. Use of percentile references is one of the genuinely newer parts of the paper and you should expand by showing more data and more related discussion.

    Example plots and discussion have been added as an Appendix.

22. L155-157: discussion of $K2 = 0$ in the Berry paper and the current paper is muddled and not possible for me to follow. One aspect is that (so far as I can tell) Berry et al did not use the second mask, which K2 refers to, so to say they set $K2 = 0$ is a bit misleading.

    Clarified as follows "The second threshold K_2 in Equation 3 was not implemented by Berry et al. [2011b], equivalent to setting $K_2 = 0\,\mathrm{K\,m^{-1}}$ since $|\nabla\theta_W| \geq 0$ by definition."

23. Figure 2 caption: Why zonal TFP? What does this mean? Surely you should us the full TFP here?

    Zonal in the sense of the zonal mean, not the zonal component of TFP, since climatological percentiles are plotted by latitude and season. Removed to avoid confusion.

24. Figure 2: Poor colour selection. Yellow is a bad colour to choose on a white background, and red and orange are a bit too similar for my liking.

    Figure updated with thicker lines and different colour palette.

25. Figure 3c: there are two of these.

    Fixed.

26. L177-178: "is valuable due to the small scale of the quantities of interest, e.g. K1=...." This statement makes no sense to me. In what way is quoting a threshold indicative of small scale?

The thresholds used for front identification are very small order 10^{-11} *and* 10^{-6}*, therefore the stable and accurate numerical schemes are required to evaluate the fields to sufficient precision to accurately locate fronts. Clarified in text.*

27. L195-196: "...tends to relate to pre-frontal troughs...". No it doesn't. These are generally dynamically inert humidity gradients on the leading edge of the warm conveyor belt. They are discussed in Hewson (1998) and Hewson and Titley (2010). The latter paper indicates that, for operational implementation purposes, a third front mask based on theta only (so no humidity impact) is included with the aim of erasing these features. The authors might like to consider improving their study, and the resulting climatological frequencies, by doing the same.

Thank you for the correction, changed to: "Such features were noted by Hewson [1998] and are associated with a warm conveyor belt running adjacent to the front. Hewson and Titley [2010] suggest a third masking criteria based on potential temperature rather than wet-bulb potential temperature that may be implemented in a future version of the code documented in this study."

28. L201-202: The sentence spanning these lines does not reflect, at all, what the figure 4d shows.

Corrected to: "Figure 4d shows that our numerical updates result in slightly lower numbers of fronts identified in across most of the northern and southern hemisphere extra-tropics, and slightly higher numbers of fronts identified in the tropics."

29. L202: "far southern ocean" – in large part this is actually sea ice.

Removed.

30. Figures 4d-f and discussion thereof: It would be far more informative for the reader to see percentage change in frequency on these plots, and have that discussed instead.

    Changed to percentage differences, also in Figures 5 and 8.

31. Figure 4: Colour schemes are poor. One should be able to read off values without manual counting. If you are going to use colours why use monochrome red shades – the whole spectrum is available to you.

    Colour palettes have been updated throughout to improve readability.

32. L214: "Climatology" means "climatological quantile values".

    Changed.

33. Figs 6 and 7: Again a poorly chosen colour scheme. Only reds are used, which is bad as in EE above, besides which the scale selected highlights very little on panels 6a, b, c. The frequencies of cold and warm fronts would be of fundamental interest and utility, but this is all but lost due to the scales and colours selected.

    Colour palettes have been updated throughout to improve readability. Scales were and are chosen throughout so that the highest frequency of front occurrence over the open ocean between approximately 60N and 60S (usually over the North Atlantic or Kuroshio) can be read directly from the figure legend.

34. L219-229: Yes, interesting, but are such aspects not already in the Berry paper(s)?

    We have added "as previously shown in Berry et al. [2011b]"

35. L221: Gradients? Of what? Do you mean light winds?

    Thank you, yes, we have changed to say "where winds are weaker, particularly in the horse latitudes and inter-tropical convergence zone".

36. L227: “Somewhat surprisingly” – it would be helpful and probably illuminating in the context of this discussion to look at seasonally average SST contours and gradients (and sea ice distribution).

    We have removed the “somewhat surprisingly”, looking as SSTs contours and gradients and sea ice is beyond the scope of the current study, but we agree it would likely be illuminating.

37. L235: It is vital to understand what you mean by aggregated. There are several possible meanings and because you don’t say it’s impossible for me to comment on Fig 8 or your inferences from that.

    Clarified in text: “Aggregation is performed by counting individual fronts identified at the higher resolution passing through the lower resolution grid.”

38. L238: “Increased ability to resolve the required derivatives”. I have no idea what this means.

    Removed.

39. L240: “where there fronts” is bad English

    Typo, changed from “with more fronts seen where there fronts were already common” to “with more fronts seen where they were already common.”

40. L241: “a result of the increased resolution”. Really? But with extreme smoothing you make the input fields look virtually the same (Fig 3c and d)? And if what you state were the case then why would an increase not be seen elsewhere?

    Removed.

41. L245: What do you mean by stable? Is this something to do with convection? And how does quasi stationary front frequency increase as a result? I don’t understand that.

We have edited this to say "due to the light winds associated with the ITCZ."

42. L247-248: What does this sentence mean? The scale on Figure 10 is very different to the scale on Figure 7, which is worrying in itself, and besides which there is so much smoothing applied that for ERA5 that I don't think you could legitimately say anything about the resolution impact anyway.

    Sentence removed, however the reduction in frequency is to be expected and is clarified in the previous paragraph in the text: "One ERA-Interim grid box contains nine ERA5 grid boxes. A perfectly straight front passing through one ERA-Interim grid box would pass through only three of the nine associated ERA5 grid boxes. Therefore, one might expect the front frequency in ERA5 at its native resolution to be approximately one third of the frequency in ERA-Interim. Comparing Figures 6 and 9 shows that this is approximately the case." The same applies to Figures 7 and 10.

43. L250: "clearly visible". Yes, of course it will be if the scale has been adjusted in such a way as to make it more visible than on the counterpart plot (Fig. 7)! And what about other SST gradient regions – edge of Kuroshio etc?

    Colour palettes have been updated throughout to improve readability. Scales were and are chosen throughout so that the highest frequency of front occurrence over the open ocean between approximately 60N and 60S (usually over the North Atlantic or Kuroshio) can be read directly from the figure legend.

44. L258: "single core of a 2 year old laptop" is not a very professional or durable way to describe computational requirements.

    Expanded to "a single core of an Intel i7-8565U based laptop with a theoretical maximum speed of 4.6 GHz*"*

45. L265: This sounds like an unsubstantiated comment and should be removed or demonstrated.

Modified to "and greater improvements are expected in lower resolution datasets for the reasons demonstrated in Figure 1

46. L279-280: "Modest performance increases". I don't know what the basis for saying this is.

    Clarified to: "modest increases in both the number of fronts and front points identified"

47. L286: "No performance benefit". Likewise what is the metric used here? This needs to be introduced much earlier, and substantiated, rather than being just dropped into the conclusions from nowhere. For a broadscale picture the del-squared approach you have used is simpler and of itself is likely to be adequate in my view, because errors arising from exaggerated frontal curvature that you get with del-squared will probably not be so critical as they might be in real-time forecasting applications, but you don't discuss this at all.

    Additional discussion has been added to Sections 3 and 3.2. The comment on the performance has been withdrawn. A more detailed comparison of the two methods would be of value since the approach preferred by Hewson [1998] has not been widely studied, but that is outside the aims of the current study.

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