

WCD Discussion Final Response

RC1 Response

We'd like to thank the reviewer for their detailed and constructive comments. We appreciate the discussion has acknowledged the importance of this research and are encouraged by the fact that the link between hurricane Franklin's extratropical transition and the predictability of blocking is convincing. These comments have identified some areas where the arguments made in the manuscript could be strengthened or emphasised, including explicitly answering the research questions posed, implications for forecast skill linked to high-impact weather and clarity of ensemble sensitivity analysis.

Major comments

Research Question (ii) and Precipitation Impacts

The central questions enumerated in lines 96 – 98 are never explicitly answered. I suggest tying the existing discussion explicitly back to central question (i). Discussion of the forecast skill of the HIW over Europe is somewhat lacking, central question (ii). My understanding from your introduction is that the main impact from the period of study was heavy rainfall and severe flooding in Greece and Bulgaria but the relationship between the uncertainties in Franklin's ET and the rainfall in Greece and Bulgaria is not. As I see it, your argument for the relationship between the position and timing of Franklin's ET and the accuracy of the forecasted omega block is good and convincing (but could be highlighted more, see comment below). However, completing the investigation by examining the precipitation forecast skill's relationship to the omega block and following the chain from ET to block to precipitation would really make this paper shine. I understand this an additional analysis but I think that checking the connection between the high and low ZALO members and precipitation skill would solidify your arguments...

We agree that the predominant focus of the current manuscript is the link between Franklin and omega blocking, with the link between the block and Daniel being somewhat implied. Our intention here was to establish the ET-block interaction as the primary predictability pathway, with ensemble members that poorly predict blocking also poorly representing Daniel's formation (or lack thereof). However, we understand that demonstrating this explicitly would complete the chain of events comprehensively.

To address this we can undertake further analysis of precipitation surrounding Daniel and link this to ZALO. We think that a full ensemble sensitivity test, using precipitation forecast verification metrics like SAL (Wernli et al. 20008) may be out of the scope of

the paper, and would suggest that this would add unnecessary complexity. The key concept of the paper is that Franklin's position and subsequent waveguide interaction is strongly linked to blocking, with the block and Daniel being part of the same Rossby wave break. To emphasize the latter part of this chain of events, we could show the precipitation fields of members with high and low ZALO (similar to Figures 8 and 9 but for storm Daniel) therefore linking Daniel to blocking, given the link between Franklin and blocking is already well established. It is also worth noting that the second part of this chain of events has already been studied (Flaounas et al. 2025).

This further analysis will strengthen the argument of a "chain of events" and this will then be emphasised in the conclusions to make this clear to the reader. By replacing the sensitivity analysis of PV320 (discussed later in this response) with these plots, research question (ii) will also be explicitly answered.

Line 446 – I think the last sentence in this paragraph "It should be noted that Franklin's initial..." is a very clear statement of the central idea of the paper, at least as I see it. This should really be given more discussion and be the main point supported by figure 10 in conjunction with figures 8 and 9.

We agree with this and note that this point will be emphasised in the discussion. By removing PV320 sensitivity figures and expanding more on the downstream impact (adding figures detailing Daniel's genesis in good members and lack thereof in poor members) the link between Franklin's initial position and downstream impact will be clearly demonstrated.

Justification of Sensitivity Methodology

Line 206 – I don't see that any choice of r_{min}^2 can be said to "...retain only reliable sensitivities..." The implementation of alpha damps the value of S for grid points with low correlations coefficients without removing them. It's also unclear why 0.15 and not 0.25 or any other r^2 value. I checked the reference but no thorough justification is provided there. I think readers might reasonably wonder at 0.15 being quite a low bar to clear for a sensitivity to be reliable. One way to address this concern would be to test for spurious correlations using a statistical test as in Ancell and Hakim (2007). I also think a discussion of the drawbacks or limitations of this mode of analysis would be very helpful for readers (e.g. How does it deal with non-linear responses if your assumption of linearity breaks down)

Ancell, B., and G. J. Hakim, 2007: Comparing Adjoint- and Ensemble-Sensitivity Analysis with Applications to Observation Targeting. *Mon. Wea. Rev.*, 135, 4117–4134, <https://doi.org/10.1175/2007MWR1904.1>.

We agree with the reviewer here. Stating that this r_{min}^2 value is used "to retain reliable sensitivities" is not necessarily well explained and does not describe the effect of α . We

will reword this accordingly and expand on the limitations of this methodology, namely, the assumption of linearity and the interpretation of spurious sensitivity with weak correlation.

However, instead of performing a full test of the sensitivity methodology, we would suggest that the r_{min}^2 value can be justified by linking the critical p-value of the regression at each grid point and from there identify a r_{min}^2 value that is valid for each sample size (size of the ECMWF and MOGREPS ensembles). Using a different and justified r_{min}^2 value for each of the ensemble systems would justify its use and ensure that the methodology is robust. We will incorporate this change in the revised manuscript, together with a fuller description of the limitations of the method.

Sensitivity – PV320

Line 352 – I don't see the region of positive sensitivity you discuss. Is it covered up by the stars? Can you comment on the interpretability of one, presumably small area of sensitivity amongst many of similar size and magnitude throughout the Atlantic? I also have trouble locating a "defined dipole" around Franklin in figure 6b for similar reasons. In fact, without any statistical test on the significance of these sensitivity values I have trouble believing that much of the sensitivity of ZALO to PV320 is useful to interpret. The main signals I see occur in the gaps between the 2PVU contours not really near Franklin. I wonder if this whole section about the sensitivity to PV320 could be removed as I'm not sure what it brings that hasn't already been discussed in other figures or the sensitivity to Z500?

We appreciate how the patterns of sensitivity, especially in MOGREPS are difficult to interpret and the noisy PV field leads to sensitivity features that are not necessarily the key regions that we are trying to highlight in this case. We agree that this figure does not provide much more insight than figure 5 (Z500 sensitivity) already does. Therefore we will replace figure (6) with analysis of Daniel's precipitation in the best or worst members. Figure 6 can be added to supplementary material and the r_{min}^2 value for Z500 sensitivity will be justified.

Minor Comments

We thank the review for an extensive list of detailed comments regarding terminology, minor clarifications, working, grammar and figures. The following outlines the changes that will be made in response to each of the minor comments.

RC1: Line 27 – ECMWF should be defined before first use

AC: Line 27 – ECMWF will be defined here

RC1: Line 38 – ET is already defined in line 30 as "extratropical transition"

AC: Line 38 – The definition of ET will be removed here.

RC1: Line 85 – I think you mean something about the relative frequency of RWB and ET making the attribution difficult? Could you clarify what is meant in the sentence beginning “Climatological studies have...”

AC: Line 85 – Clarification will be made to emphasise that Rossby wave breaking is difficult to attribute to ET in the Atlantic basin.

RC1: Line 119 – Are there other predictability characteristics you mean besides poor predictability at longer than 96 hour lead times?

AC: Line 119 – Other predictability characteristics from the papers cited here will be mentioned as well as predictability of genesis (which is most relevant to the manuscript).

RC1: Line 121 – Is “this” the interaction of Hurricane Franklin with the waveguide or accurate block onset? The sentence in question has a lot going on, I recommend expanding to multiple sentences and being explicit.

AC: Line 121 – This sentence will be broken up and vague words like “this” will be replaced by what the sentence is referring to.

RC1: Line 125 – I believe that the “E” in ERA5 stands in for ECMWF not just European.

AC: Line 125 – RC1 is correct and this will be changed accordingly.

RC1: Line 127 – I’d appreciate some additional comment on the choice to coarsen the horizontal resolution. Also, why are there parenthesis around 0.5?

AC: Line 127 – Parenthesis will be removed. The explanation for regridding is given further down in the manuscript so this will be moved here and expanded upon later.

RC1: Line 135 – Did you regrid the data from its native resolution to 0.5 degrees? If so, why? Some discussion of your regridding choices would be appreciated.

AC: In accordance with the prior point, this will be expanded upon. We regrid for the sensitivity calculation as data retrieved from TIGGE is at 0.5 degree resolution. TIGGE is a multi-centre archive and does not store model data on native grids for ease of inter-model comparison studies (see Swinbank et al. 2016).

Line 159 – Is this constraint on Z' found to be sufficiently large in some previous work you could cite?

AC: Line 158 – A range of Z^* constraints were tested before use. We will consider adding a brief discussion on these tests and the possible inclusion of supporting figures in the supplementary material.

RC1: Line 162 – I don't think you need to tell readers that ZALO is similar to SAL and PAL when you don't explain how SAL and PAL are formulated. It makes things a little confusing.

AC: Line 162 – We will add more details on SAL and PAL and their purpose. This is important as the ZALO error metric is based on these error metrics, so further details on them are justified to provide context.

RC1: Line 174 – Do I understand correctly that the L in ZALO is directly affected by the choice of domain, through the length of the diagonal? If that's the case, did you do any testing of different domain sizes to learn more about how sensitive L and ZALO are to the choice of domain?

AC: Line 174 – You are correct, though testing was done to ensure that the domain chosen encompassed only the block over Europe and was no larger. This can be mentioned here.

RC1: Line 221 – I think you should add the word “both” after MOGREPS-G in this line to clarify that you mean each ensemble under and over predicts intensity rather than ECMWF ENS under predicts and MOGREPS-G overpredicts.

AC: Line 221 - We agree with this; the word “both” will be added here.

RC1: Line 223 – I'm not sure figure 2 clearly shows that L is “generally small” especially relative to Z error. If this is a point you want to make it would be helpful to show the components of ZALO on plots with the same scale of Y-Axis (you could center it differently depending on the spread of the individual metric)

AC: Line 223 – I agree that L is not necessarily small when comparing it to Z for example. This will be removed.

RC1: Line 226 – the sentence beginning “In the combined ZALO metric...” could mean a couple of things about figure 2e. Can you be more explicit in what you mean here. For instance what does effective mean in this context? Also best and worst on what dimension? Do you mean best and worst for single ensemble and lead time? Separated along what dimension is another question. Does separation refer to ensemble spread or something else?

AC: Line 226 – Effective in this case means that there is a large spread in the magnitude of ZALO and each of the individual components at long lead times, and low spread at short lead times. This is essential for sensitivity analysis to provide robust signals. This will be clarified here.

RC1: Line 228 – What do you think it implies about this metric, or the forecasting of blocks generally, that this is true for Area and Shape but appears not to be true for

Intensity and Location? Particularly for Location this decrease appears to occur 24 hours earlier? Is the predictability barrier for certain aspects different?

AC: Line 228 – The metric is designed such that each of the components are independent (though A and O are more closely related). Therefore, Z can be forecast well (small Z) for a block that has a poorly forecast area (very negative A for example). In this case I would say that block area and shape were poorly forecast, but there were members that forecast blocks within the domain (correct L) and with the correct Z500 anomaly (Z). This can be mentioned here in the manuscript.

RC1: Line 235 – To my eye location might experience its most rapid error convergence between 168 and 120. Is that wrong? Did you quantify error convergence in some way?

AC: Line 235 – This I think is open to interpretation. I would say that the maximum rate of error convergence $d(\text{ZALO}) / d(\text{lead time})$ occurs between 144 and 120 h, but the lead time at which the error and spread are significantly smaller than very long lead times (>168) is 96 h. This will be clarified in the manuscript.

RC1: Line 246 – I'm not sure what "short lead times" refers to here. I think we're still talking about figure 3 but lead time increases as we move down the y-axis. Additionally, towards the top of the graph, I can't really tell if there is a difference between the ECMWF and Met Office forecasts of Franklin's position which would be the "short lead-times" you refer to. Can you clarify the point you're making here? I'm also not sure how the last clause of this sentence relies on the previous one.

AC: Line 246 – Perhaps direct reference to the times referred to here would be useful. This will be added. What we refer to here is the difference in spread of Franklin's position between the ensembles 2-3 days into the forecast.

RC1: Line 285 – I think Is this equivalent to saying that lower Z500 in areas of positive sensitivity is associated with lower ZALO? Is it also true that higher Z500 in areas of negative sensitivity is associated with lower ZALO? I think an explanation in these terms of the relationship between deviations from the ensemble mean Z500 and ZALO would be very helpful for readers to understand what you've done here without going back and forth between the figure and the text identifying features. Additionally, I think providing an interpretation of negative sensitivity would help unfamiliar readers understand this slightly unintuitive concept.

AC: Line 285 – To RC1's opening questions, yes this is true and equivalent to the statement made in the manuscript. Perhaps this can also be explained and expanded upon when the sensitivity section is introduced.

RC1: Line 292 – Should "ensembles" be "ensemble members"

AC: Line 292 – the word "members" will be added.

RC1: Line 296 (and 315) – I don't quite follow the sentence starting "A wave-like pattern..." I don't quite follow this sentence. You start by mentioning a wave-like pattern in the sensitivity field but I don't see why that indicates something about the ensemble members with a good forecast of Franklin's position (those with small errors). Isn't the sensitivity field calculated across all ensemble members? The connection to the RWP is also a little tenuous. Can you explain how Rossby waves are expressed in the sensitivity field (if that's your claim) or clarify the relationship you're putting forward between the wave-like pattern in the sensitivity field and RWP. I can believe they're related but I need a little deeper explanation of that relationship from you.

AC: Line 296 (and 315) – The statement here refers to the fact that the flow is much more zonal in the poorest members (and amplified in the best members), therefore implying that in the best members, Franklin acts to amplify the synoptic-scale wave pattern (also seen in figures 3 and 4). This is then highlighted in the sensitivity signal. Perhaps referring to the synoptic-scale wave pattern is more direct, and the RWP excitation is more implied. This will be reworded to avoid the use of RWP, which is not necessarily defined.

RC1: Line 437 – the first sentence of this paragraph "Figure 10 shows how..." could be removed if you referenced figure 10 at the end of the following sentence.

AC: Line 437- This will be removed and figure 10 will be referenced at the end of the following sentence for clarity.

RC1: Line 456 – replace "cyclone one" with "cyclone in one"

AC: Line 456 – "cyclone one" will be replaced by "cyclone in one"

RC1: Line 474 – replace do with does

AC: Line 474 – do will be replaced with does.

RC1: Line 478 – replace "determine cause" with "determine the causes"

AC: Line 478 - "determine cause" will be replaced with "determine the causes"

RC1: Line 536 – I'd suggest starting this paragraph with "We have shown block error to be sensitive to..."

AC: Line 536 – the opening of this paragraph will be changed to "We have shown block error to be sensitive to..."

Figure Comments

RC1: Figure 1 – It should be fairly obvious to most readers but I think it's worth specifying 320K is a potential temperature, at least the first time.

AC: 320K will be defined as a potential temperature in the figure caption.

RC1: Figure 3 – I think it would be easier on readers if you wrote out the full ECMWF EPS and MOGREPS-G in the panel titles rather than EC and MO which are abbreviations that haven't been defined yet.

AC: Full acronyms will be used here.

RC1: Figure 3 – Why are there different time periods displayed between the ensembles? I strongly suggest standardizing them to facilitate comparison of the evolutions of these two ensemble prediction systems.

AC: The time shown will be standardised for ease of comparison. MOGREPS-G forecasts are shorter than that of ECMWF forecasts therefore the ECMWF forecast times shown will be brought in alignment with MOGREPS-G.

RC1: Figs 3,4,5,6 – Why are there no indications of ensemble members' locations of Franklin after September 3rd? Are they not available or is it some other reason? I think they would be interesting and help complete the picture of these forecasts.

AC: Ensemble member locations are not shown after the 3rd of September as in some members Franklin has dissipated, become part of a frontal wave or does not have a defined MSLP minimum. Only reliable data is used here and we think removing members in which Franklin is poorly defined could lead to a false interpretation of the spread of the members. This can be mentioned in the figure caption.

RC1: Figure 5,6 – You can't see the IBTrACS Franklin star in panel b. Could you plot it on top of the blue best fit stars?

AC: Franklin in IBTrACS will be plotted on top of the ensemble member positions.

RC1: Figure 8,9 – Please note what the solid black contours represent in the caption and specify the 2PVU contour is in red (if that's the case).

AC: It will be mentioned in the figure caption that this is the 2PVU line on 320K and the solid black contours are MSLP.

RC2 Response

We'd like to thank the reviewer again for their detailed and constructive comments. We are pleased that the reviewer sees novelty in the methods used and how this study fits into prior literature. We agree with RC2 (and RC1) that the link between blocking and storm Daniel is implied and should be supplemented with further analysis. The following will respond to the major and minor comments from RC2.

Major comment

Precipitation Impacts

RC2: Major suggestion: the connection made between the TC (Franklin) and the block is demonstrated with substantial analysis. The connection between the high-impact weather (HIW), Storm Daniel, and the block is given much less attention. The paper is already substantial in length, so I am not sure that adding more is the answer. I wonder if some of the papers that are cited in the introduction have already established the clear connection between the block and the HIW? If so, I suggest that the introduction, and/or discussion, section include a more explicit statement about this.

AC: Please see response to RC1. We suggest removing the sensitivity section referring to PV320 and substantiating the following section with figures, similar to 8 and 9 that demonstrate the differences in Daniel in the best and worst members (in terms of ZALO). This change would complete the chain of events and emphasize the idea that the representation of blocking was significant for high-impact weather.

Minor Comments

We thank the reviewer for an extensive list of detailed comments regarding terminology, minor clarifications, working, grammar and figures. The following outlines the changes that will be made in response to each of RC2's minor comments.

RC2: I suggest that you consider referring to Oertel et al. (2023) somewhere in this manuscript. Either in the introduction or in the discussion. It seems to me that it is highly relevant to the story being told in this manuscript. Oertel, A., Pickl, M., Quinting, J. F., Hauser, S., Wandel, J., Magnusson, L., et al. (2023). Everything hits at once: How remote rainfall matters for the prediction of the 2021 North America heat wave. *Geophysical Research Letters*, 50, e2022GL100958.
<https://doi.org/10.1029/2022GL100958>

AC: Thank you for the suggestion. This will be read and cited in the manuscript if relevant.

RC2: Line 110: Figure 1: You write that the Z500 contours are grey, but on my screen they look black, and the coastlines look to be grey.

AC: This will be corrected in the figure caption. The contours are indeed black.

RC2: Line 152: I acknowledge your rationale for utilizing this detection scheme that does not impose persistence. In this case, since other papers have established the essence of this z500 anomaly being a block, I think your approach is reasonable. However, I ask that you include some sentence to this effect, so that we avoid some situation where, if your is published, other future work leans into this idea of just detecting z500 anomalies as blocks with no consideration for persistence.

AC: We agree with the reviewer here. A comment will be made here to clarify that usually there is a persistence aspect to blocking.

RC2: Line 162: I suggest that when you cite a PhD thesis, you include a page range or a chapter. I enjoyed scrolling through the entire thing, but others might not. With books, I believe the accepted approach is to do something akin to what I have suggested, and thesis' are sort of like books.

AC: This makes sense to do. The page number referred to will be added to the references.

RC2: Also, a question about ZALO with how you are applying it for a fixed blocking region: does it penalize thrice for a model putting the block in the wrong location? Z, L, and O would all be effected wouldn't they? Perhaps that's fine. My impression wrt SAL in Wernli et al. (2008) was that for precipitation, there were sometimes offsets in the bias scores – see their schematic in their Fig. 1. But I might also be wrong. I have not worked with it directly myself. Thanks for considering this question, I am just trying to fully appreciate the what this metric provides. (This question of mine perhaps also demonstrates that I merely looked at equations in the Suitter's thesis to confirm them in your manuscript.)

AC: Z and L are independent of A. For example, you can imagine an erroneously small "blocked" region but the Z*' anomaly can still be large (and vice versa). A and O are somewhat related, so does double penalise poor blocks, but this is advantageous in this case as it increases the spread in the response function and penalises blocks with similar ZAL but poorly represented shape (explained in the manuscript). Perhaps this could be clarified in the data and methods section.

RC2: Lines 209 – 210: In this sentence where you mention multiplication with a SD, are you including this to make the point that you also focus on the standardized sensitivity? For me, as it is currently written, the intent is not clear.

AC: The sensitivity is multiplied by the standard deviation of the precursor field to ensure the sensitivity has the same units as the response function (after dividing by the variance of the precursor field to ensure that sensitivity exists even where the spread in the precursor field is small).

RC2: Line 214: Based on the previously published literature (which you cite in this same sentence), I am having a hard time accepting that your hypothesis is very hypothetical. For me, it is more of a presumption or a premise upon which you are analyzing the data.

AC: We agree. Perhaps this could be replaced with "it has been established that" instead of "we hypothesise".

RC2: Line 268: Figure 3: I like this figure. The location of Franklin for the different ensemble members is informative. Sorry if I missed this in the text, why do the model locations for Franklin stop on Sep 3 while the position in IBTrACS continues to be

shown? Also, I wonder if you could show the center-of-mass location of the block in each ensemble member as well?

AC: Ensemble member locations are not shown after the 3rd of September as in some members Franklin has dissipated, become part of a frontal wave or does not have a defined MSLP minimum. Only reliable data is used here and we think that removing members in which Franklin is poorly defined could lead to a false interpretation of the spread of the members. This can be mentioned in the figure caption.

RC2: Line 254: You refer to features in Fig. 3 demonstrating uncertainty in the timing of block onset, however, the dashed red line in Fig. 3 and the stars for the ensemble and ERA5 block starting on Sep 5 as well convey a message of a good match in the timing of the block onset. I wonder if you can find a way to add additional information to make your point about uncertainty in block onset more clear?

AC: We agree that this may not be clear. The ensemble mean positions of the Z500 maxima in these figures are merely to demonstrate the position of the blocks was poor. In this forecast, most members had a block developing (see figure 2) but with poor representation of position and amplitude. In earlier forecasts (longer lead times) the ensembles struggle to forecast block onset. Focus should be on the position of the blocking here and that will be made clear in the manuscript.

RC2: Line 285: This sentence is bit confusing to me, and so I cannot even tell if the “that that” is a typo or intentional. I suggest you find another way to state what you mean with this sentence.

AC: This was also mentioned by RC1. This will be reworded so that the meaning of the sign of the sensitivity is clear.

RC2: Line 312: Figure 5c vs Figure 5f: for me it is surprising to see a lot larger sensitivity in EC as compared to MO, given that the ensemble spread in track location of Franklin appears to be similar for EC and MO in Figure 3a vs Figure 3c dots. The text you have on lines 322 – 325, seems to be somewhat related to this, but I do not quite follow what you mean. For instance, please expand or explain the statement: “...the presence of multiple pathways to poor blocking.”

AC: The magnitude of the sensitivity is determined mainly by the spread in the response function (ZALO) and the slope of the linear relationship between the precursor field and the response function. In MOGREPS, ZALO has less spread. Figure 9 shows the multiple pathways to poor blocking that reduce the strength of the linear relationship between ZALO and Z500. This can be expanded upon here but is referenced again later in the manuscript.

RC2: Line 396: Figure 7: The figure demonstrates a strong correlation in error for lat and lon. And a relationship between the amplitude of the error and ZALO. But I would be

interested to also see the exact location of the block center-of-mass per ensemble member. I am curious to know if block amplitude or block position is more impacted by the biases in the representation of Franklin.

AC: From analysis not in the manuscript, Franklin's position is strongly related to block amplitude and therefore both area and location, but with block area being affected most by Franklin's position. Small north-south displacements in blocking are outweighed by area changes in all directions of block extent.

RC2: Line 400: Section 4.3 focuses on Franklin during transition. This analysis is useful and consistent with other work. However, I wonder if a bit too much of the emphasis is on the error associated with the TC. Have you convinced yourself that the models all capture the upstream features of the trough in the same manner? My suggestion is that the TC transition matters, but so does the model representation of that trough – in terms of location. And subtle difference in the upstream trough might matter significantly, see this preprint for instance: <https://arxiv.org/abs/2604.00348>

AC: We agree, small changes in the amplitude and position of a short-wave trough was a likely cause of Franklin's position errors at short lead times. However, the focus of the paper here is the impact of the positional errors on the waveguide and the downstream effects of ET, related to high impact weather. A detailed analysis of this short-wave trough interaction is out of scope of this manuscript though we agree that it should be given more attention in the discussion section to contextualise with prior literature.

References

- Flaounas, E., Dafis, S., Davolio, S., Faranda, D., Ferrarin, C., Hartmuth, K., Hochman, A., Koutroulis, A., Khodayar, S., Miglietta, M. M., Pantillon, F., Patlakas, P., Sprenger, M., & Thurnherr, I. (2025). Dynamics, predictability, impacts and climate change considerations of the catastrophic mediterranean storm Daniel (2023). *Weather and Climate Dynamics*, 6(4), 1515–1538. <https://doi.org/10.5194/wcd-6-1515-2025>
- Swinbank, R., Kyouda, M., Buchanan, P., Froude, L., Hamill, T. M., Hewson, T. D., Keller, J. H., Matsueda, M., Methven, J., Pappenberger, F., Scheuerer, M., Titley, H. A., Wilson, L., & Yamaguchi, M. (2016). The TIGGE project and its achievements. *Bulletin of the American Meteorological Society*, 97(1), 49–67. <https://doi.org/10.1175/bams-d-13-00191.1>
- Wernli, H., Paulat, M., Hagen, M., and Frei, C.: SAL - A novel quality measure for the verification of quantitative precipitation forecasts, *Monthly Weather Review*, 136, 4470–4487, <https://doi.org/10.1175/2008MWR2415.1>, 200