Answer to Review #1

Dear Reviewer,

Thank you very much for your positive remarks and constructive feedback! We really appreciate the time you have spent reviewing our work. Below, you find a line by line answer to your comments:

Abstract

Line 5-6: “in the average prediction of 10m windspeed and 2m temperature” - averaged over what? At what leadtimes and timescales?

We will change this sentence to “We find that data-driven models outperform ECMWF’s physics-based deterministic model in terms of global RMSE for 1d-10d ahead forecasts, and can also compete in terms of extreme weather predictions in most regions. “

Introduction Line 49: “comparison in terms of a standard metric (RMSE)” - it would be good to note that RMSE is the objective function of the ML models, so evaluating against RMSE is not a fully independent target (I know you touched on this above – but making it clear you’re taking this into consideration when you choose RMSE for your evaluation metric would be good).

Thank you for pointing this out, we agree with the reviewer that it is important to emphasise more this point in the manuscript. We will add a paragraph to the discussion section explaining the limitations of the extreme metrics approach (including the limitation pointed out by the reviewer), and inviting practitioners to integrate our analysis with case-studies and qualitative analyses of weather charts.

Models and Methodology

Line 71-72: “operational Pangu weather (Bi et al., 2023) and operational GraphCast (Lam et al., 2023)” - I think you need to explain the difference between operational GraphCast/Pangu weather and reanalysis versions. I assume it’s that the operational versions have been fine-tuned on the IFS HRES, but you should make that clear, especially since in the model description sections 2.2 and 2.3 you describe Pangu and GraphCast as being trained on ERA5 and predicting on the ERA5 grid, but then also talk about them predicting on the IFS HRES grid without an explanation of how that works. This is also important to be clear about, since (for example) providing IFS HRES ICs to a model trained on ERA5 then fine-tuned on IFS HRES will give a much better-quantified result than providing some other ICs to the model that it has not been fine-tuned on – in this case some reduction in performance can be expected (based on my own investigations) but the extent of this would be unknown.

We agree that a concise explanation of what is meant by “operational version” is needed. In this context, “operational version” means that the model is able to generate forecasts based exclusively on inputs available within IFS HRES - without using reanalysis data or variables which are not part of the operational analysis (e.g. precipitation) to produce new forecasts.

Pangu operational is exactly the same model as the original Pangu-Weather, yet being fed IFS HRES instead of reanalysis data. GraphCast operational is a slightly modified version of the original model, which, differently from the original model, does not require precipitation as input to generate new forecasts. Furthermore, GraphCast operational has been undergoing a slight fine-tuning of the parameters to compensate for the loss of precipitation as input. In this respect, we agree that GraphCast has an edge over Pangu. However, it is up to the model developers to decide to what extent to fine-tune their models, and which versions of their models to make available to the public. As neither we nor the WeatherBench 2 can affect this process, we think it still makes sense to compare the best available versions of each model in our benchmark. Following the reviewer's comment, we will add an explanation of the differences between operational and non-operational versions of GraphCast and Pangu-Weather to the respective sections introducing these models (Subsections 2.2-2.3).

Line 76-79: I'm a little disappointed that you didn't also include an SFNO-based model like FourCastNet, since they exhibit quite different spatial variation in their RMSE scores compared to GraphCast, and are quite a different architectural approach to any of the models you've looked at.

We agree that it would be interesting to include a SFNO-based model to the comparison. However, we are limited here by data availability since none of these models were included in the WeatherBench 2 (Rasp et al., 2024), and predictions produced by other tools (e.g. ECMWF plugin tool) are not comparable to the one included in the WeatherBench 2, as they do not take IFS HRES as input. It is our hope that some future work may include more models!

Line 109: "As for Pangu-Weather" - suggest changing to "As with Pangu-Weather"

We will make this change to the text.

Section 2.4: Given you only look at FuXi in the Appendix, I'd suggest moving this model description there.

We agree with the reviewer and will move FuXi's model description to the new Appendix D.

Line 122: "and all comparisons are based on a spatial resolution of 1.5 degrees" - I think it would be good to expand on this a bit. I assume you have regridded the data from 0.1 to 1.5 degree resolution - How did you do this? Any special treatment of the poles? Did you do this before or after calculating metrics (presumably before). Basically, some more procedural details would be appreciated.

All data have been regridded from their original resolution to a 240x121 equiangular conservative grid. This was done by ECMWF for the ECMWF data, and by the WeatherBench 2 for all other models. More details can be found in the WeatherBench 2 paper (Rasp et al., 2024). We will add a brief explanation to the manuscript.

Line 123: "sand" -> "and"

Thank you for spotting this type, we will correct it in text.

Line 126-139: I suggest putting this information in a table for easier reading.

We will implement the reviewer's suggestion and create a new table (Table 1) to include this information.

Line 154 and onwards: I would suggest avoiding use of the term 'observations' since you are evaluating against reanalysis. The term observations runs the risk of causing confusion and giving the impression you are evaluating directly against point obs for example.

We will replace the term "observations" with "data-points".

Line 169: So in case 2, the quantiles are computed using 702 values because that's what there is for each grid point? Might be worth making this clear, and making it clear that the number of points contributing in case 1 is 702 * num_lat * num_lon

Yes, that is correct. We will add a sentence to each of the two criteria to clarify the number of available data-points for evaluation.

Results

Fig 1:
It might make the figure a bit too busy, but have you tried drawing borders around the scores? As it is the HRES scores look a bit like headings because their colour is always white. Borders may not look better though – it's hard to tell without seeing it, so please take this as just a loose suggestion.

We introduced the use of borders in Figure 1 to indicate statistical significance instead. See our reply to the in-depth comment below.

Line 193: "extremes observations" -> "extreme observations"

Thank you for pointing out this typo, we have implemented this change in the manuscript

Line 193: How many data points does the 5% most extreme cases leave you with? What is the statistical significance of the scores shown in Figure 2 (and Figure 1 as well for completeness I suppose).

We welcome this suggestion, as introducing some form of statistical significance could help to separate meaningful differences between models from overall noise. However, we should be mindful of the fact that in this case standard tests of significance may tend to overestimate the significance of the differences between models, since the selected extreme

observations may be strongly correlated in space and time. To address this issue, we make use here of a paired t-test with cluster robust standard errors, which takes into account the clustered nature of the selected observations in space and in time. This is an approach issuing from panel data econometrics, first introduced by Liang and Zeger (1986) and Arellano (1987) and recently revamped by in depth treatments by Angrist and Pischke (2009), and Cameron and Miller (2015). The intuition behind the use of clustered standard errors in this case is that since many of our data-points come from adjacent grid points and are clustered in space and time, the effective number of degrees of freedom of our test is much smaller than the number of available paired forecast differences. Thus, we correct for this by inflating the standard errors, introducing a clustering parameter.

Below we provide an example of the new RMSE scorecards figures, where black borders indicate that the performance of the model at a given lead time is statistically significantly different from IFS HRES, at the 5% significance level.

Line 200: Ditto previous comment, but for 1%

See answer to previous comment, we updated the figure for the 1% extremes as we did for the figure for 5% extremes.

Figure 4: I would suggest you try color schemes other than red-white-blue for this figure since there's a value judgement (better/worse compared to baseline) associated with those colours from the previous figures. Since this is a straight model comparison, some totally different color scheme would be good in my opinion. Figure 5: Same comment as for Fig 4.

We understand the reviewer's concern, and have changed the colour scheme accordingly, see example figure (Fig.4) below.

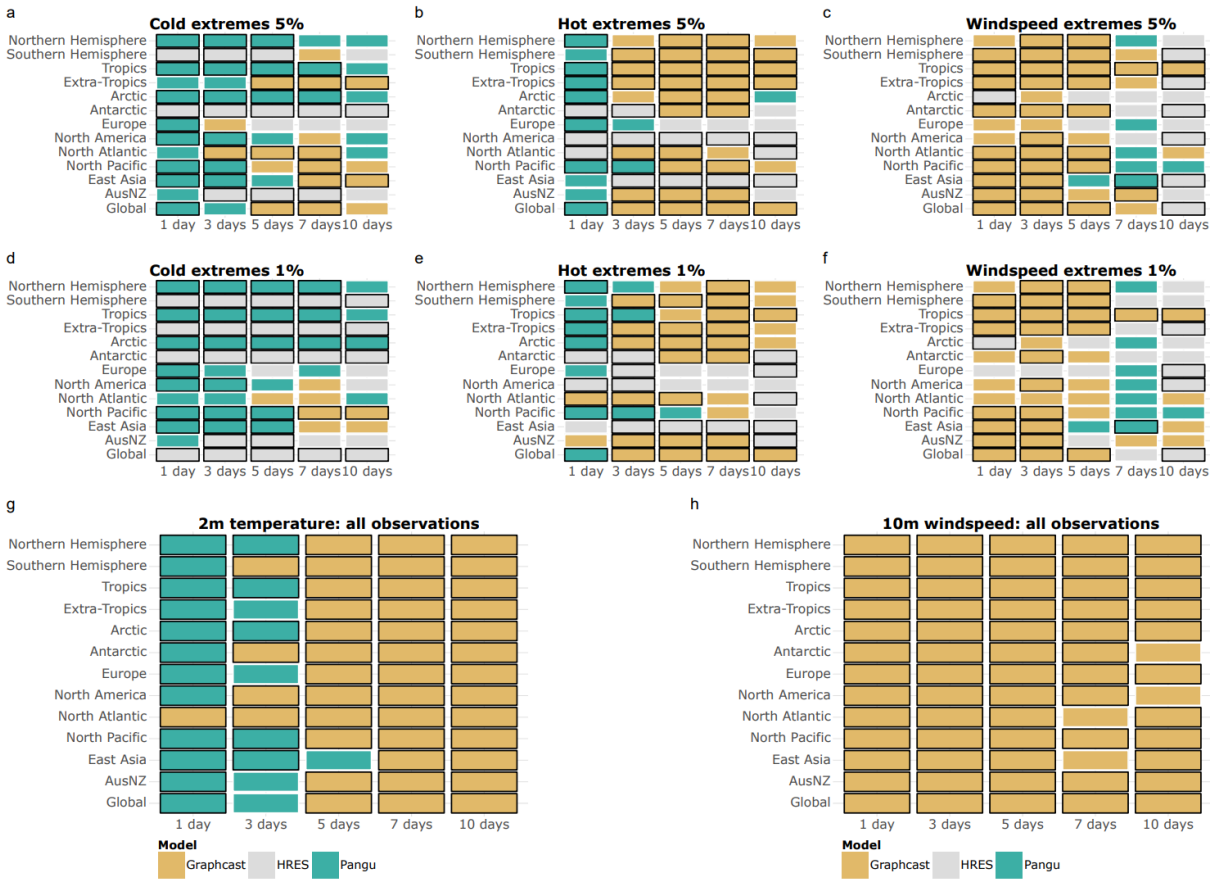


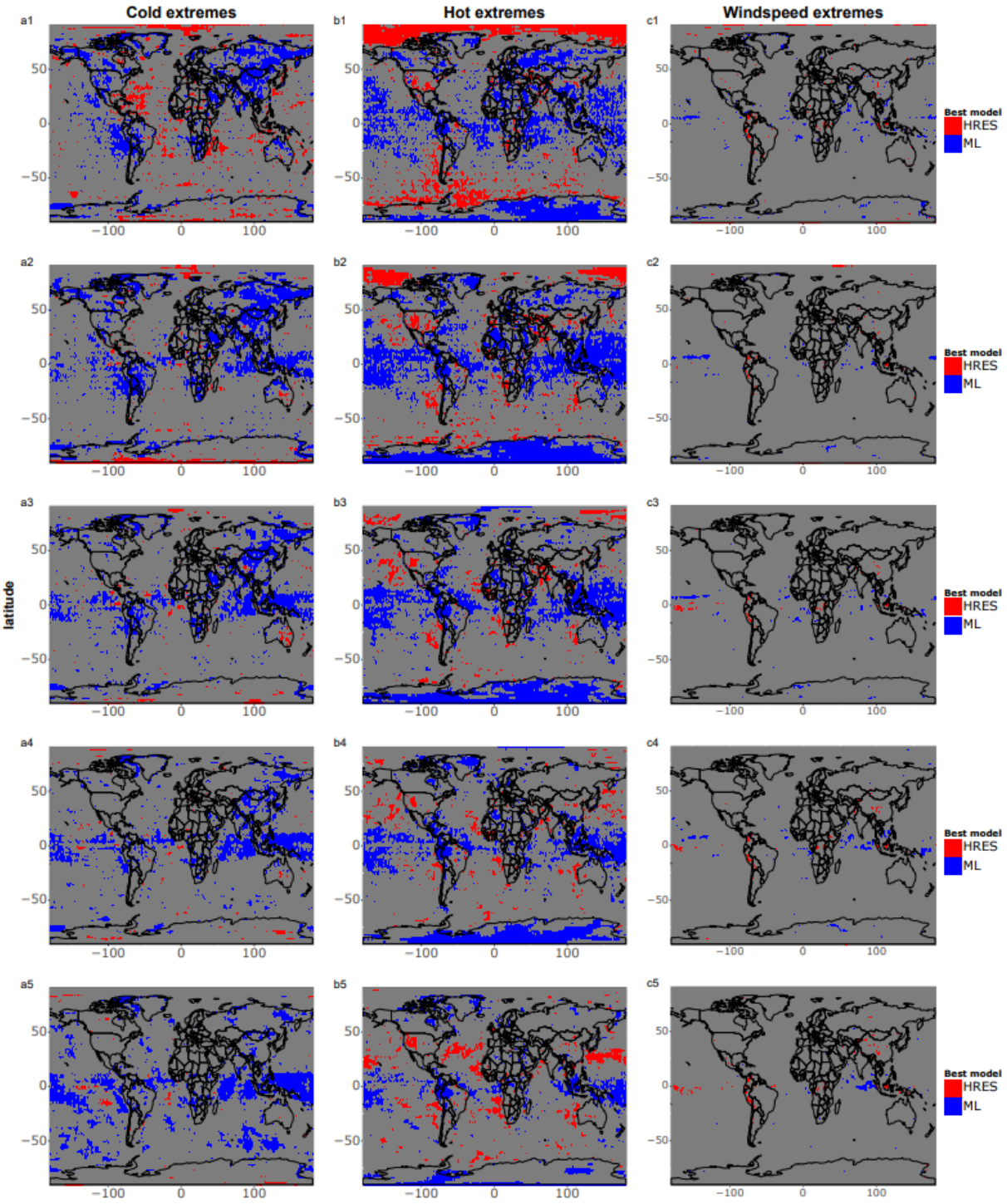
Figure 5: Similarly to the previous comment, it looks to me like there's some tendency for HRES to be better at 2T on the westward side of the continents, and worse on the eastward side. Do you have any thoughts on what this could be due to? Since these are upwelling regions and the feature grows with lead time my intuition is that it could be related to the lack of an ocean in the ML models?

This is certainly a possible explanation. Currently, there are interesting discussions underway within the weather ML community about the possibility of developing coupled ML

models that could tackle this challenge, which is especially significant at longer time scales. However, given the limited data availability and the fact that we have limited evidence to support this claim, we believe it may be best to express ourselves with caution on the topic in the manuscript. Nevertheless, we are going to briefly mention this point in our discussion.

Figures 5 and 6: Some measure of the magnitude of the differences between the models would be very valuable to contextualize what's shown in this figure (maybe maps of the model differences with stippling for statistical significance added to another appendix?) – some of these pixels where one model is shown as best may have a very marginal difference between the models and it would be good to know where this is the case. It would also be good to include an indication of what is statistically significant with this, especially for Fig 6 (where I think your sample is 702 * 0.05 = 35?). This could for example be stippling on Figs 5 and 6 as well as any maps of the magnitudes of the differences.

We agree with the concern raised by the reviewer, and we have added some figures showing statistically significant differences between the performance of machine learning models and IFS HRES (See new example figure below). The significance tests for these figures also use the aforementioned clustered standard errors to account for clustering of extremes in time, and global false discovering rates (Wilks 2016, Benjamini and Hochberg, 1995) to correct for multiple testing and ensure robust statistical inference. As illustrated by Wilks (2016), this approach is also robust for spatially correlated values. In the figure, dark grey areas show lack of statistical significance of the results, namely that the difference between IFS HRES and the best machine learning models is not statistically significant . Indeed, as suggested by the reviewer, many results are not statistically significant in Figure 6, due to the small sample size (n=36 at each grid point). However, some interesting regional patterns still emerge, which we discuss both in relation to this figure and in Section 4 in the manuscript. As in Figures 1-3, blue shades indicate that the machine learning model is better than IFS HRES, whereas red shades indicate that IFS HRES is better.


Cold extremes
Hot extremes
Windspeed extremes
a1
b1
c1
a2
b2
c2
a3
b3
c3
a4
b4
c4
a5
b5
c5
latitude
50
0
−50
−100
0
100
Best model
HRES
ML

To complement these new figures, as suggested by the reviewer, we also plan to add corresponding figures showing the magnitude of the differences between the models, see example figure below.

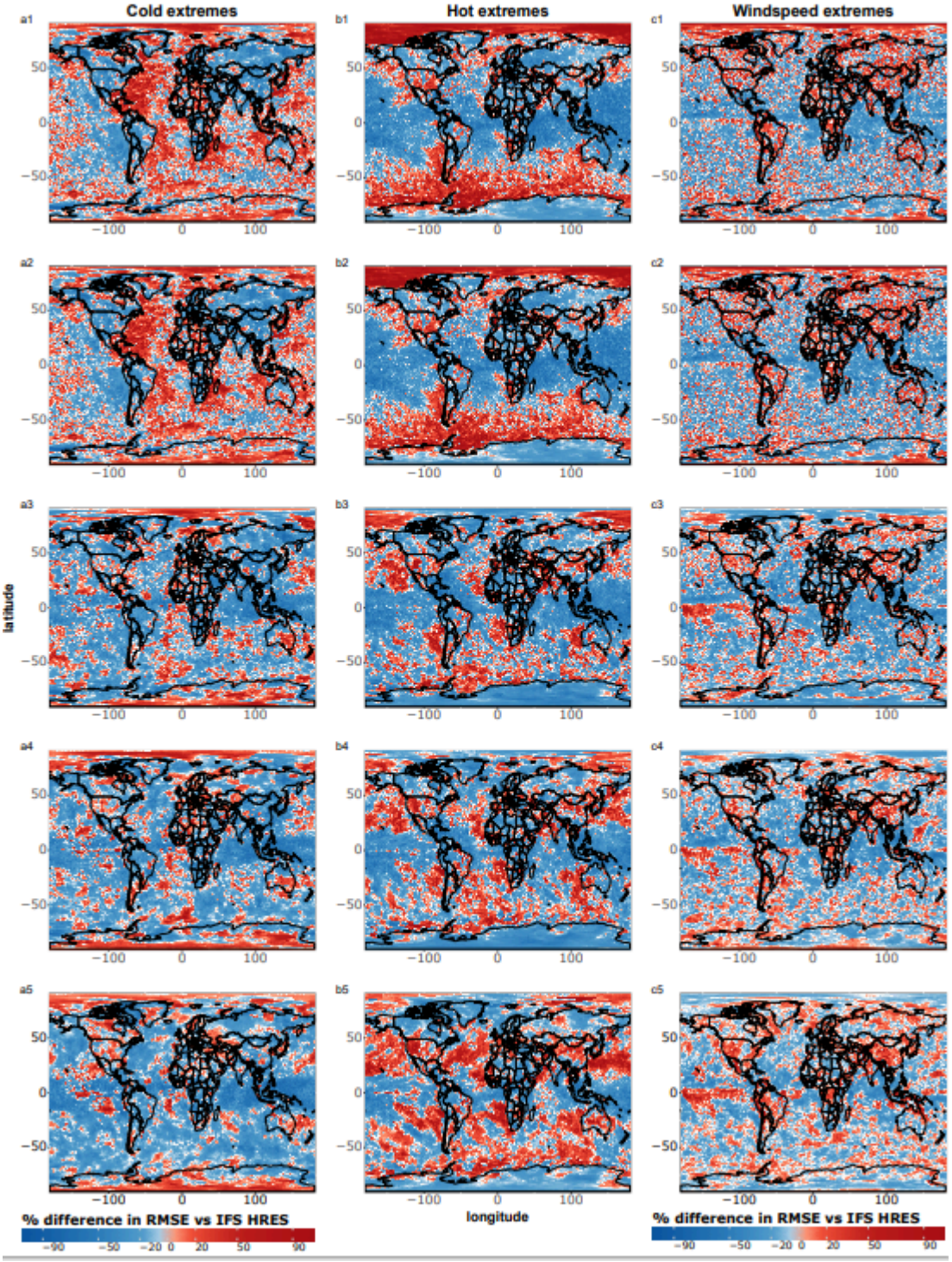


Figure 7: I find these plots pretty hard to read without leaning right in – perhaps you could increase the marker size, or subtract the y=x line from each set or points and display them ad deviations from perfect calibration to increase the visual distance between the markers?

We will increase the marker size as suggested by the reviewer and will also change the colour scheme to reflect changes in Figures 1-3.

Figure 7: Some indication of statistical significance or confidence on these plots would be good

Figures 7-10 are tail reliability plots (qq-plots for the tails of the distribution) that show the calibration of the forecast vs ERA 5. This type of plots does not usually come with confidence intervals or significance assessments attached, since they do not aim to rank the forecasts or show any significant difference between them, namely they do not aim to perform any kind of forecast-based inference. They just aim to provide a rough visual assessment of the calibration of the forecast at different quantiles during the test period vs the (imperfect) ground truth. Any significance assessment here would require making assumptions on the distribution of future extreme observations, on the correctness of the ground-truth (reanalysis distribution), and on the calibration of the models not being improved by the model developers.We see no way of grounding such assumptions given the available data.

Similar plots, without confidence intervals, also appear in recently published publications on this topic, including the quite influential “The rise of data-driven weather forecasting: A first statistical assessment of machine learning-based weather forecasts in an operational-like context” (Ben Bouallègue et al., 2024, DOI https://doi.org/10.1175/BAMS-D-23-0162.), developed by the ECMWF team. We would therefore suggest maintaining Figures 7-10 as they are in this respect.

Figure 8: Ditto both comments from Fig 7

See our two replies above.

Line 261-277: While this is interesting, it feels a little out of place since you haven’t discussed FuXi anywhere else, and in the next section you once again stop referring to FuXi. I feel like you should either move these paras to the appendix so that the FuXi and other reanalysis initialized models are selfcontained in the appendix, or you should add some acknowledgement of their analysis to the opening sentences of the conclusion to make the transition from these paragraphs to the conclusion less jarring.

We agree with the reviewer that these reflections might belong better to other sections in the manuscript. We will therefore shorten them down to one paragraph, and move them to the discussion section, after an overview of the main results. We still think there is some value in maintaining this discussion point in the main body, though, as it might be of interest to a number of readers.

Discussion and Conclusions

Line 295: It also looks to me like Pangu does better at cold extremes, and for hot extremes GraphCast is better over the oceans while Pangu is better over land (based on Fig 6).

Thank you for pointing this out, we will add this information and a more in depth analysis of Figure 6 to our discussion.

Line 298-299: “the choice of best model depends strongly on region, lead time, type of extreme and in some cases even level of extremeness”. I feel like this might not be quite the right way to put it – to me this implies that there is strong variability in the magnitude of the differences between the model’s scores with region, lead, extreme type etc., but the magnitude of the differences between the models is not clear form most of your figures. All we know is that there is a lot of variability in which is best with region, lead time etc., but not by how much it is best. I suspect that you meant that with your wording, but I’m a bit worried it could be misinterpreted and suggest you revise this statement.

We agree that the wording here was not ideal, and that some of our conclusions might have been affected by the noise in the results. By implementing the significance test illustrated above, we have identified some clearer and more robust patterns. Thus, we now place greater emphasis on these robust results rather than on general statements such as the one above. We now highlight, in particular, how the quality of data-driven forecasts declines at longer lead times, likely due to blurring (Bonavita, 2024; Price, 2024). Moreover, we notice a clear trend towards better data-driven forecasts closer to the Equator, and worse at higher latitudes, which we link to the use of area-latitude weights in the loss function (e.g. Lam et al., 2023, Chen et al., 2023). Lastly, we find that IFS HRES displays the best calibration overall in terms of tail reliability, and that the extreme weather forecasts generated by data-driven models might benefit from some form of debiasing.

Line 301-303: “. Ideally, we envisage a hybrid use of physics- and data-driven models to forecast extremes, with physics-based models being supplemented by data-driven models for those areas where data-driven models have been shown to be superior in terms of tail performance.” - are the sizes of the differences in performance enough to justify this approach? I’m taking an operational forecast perspective here, and it feels like the potential gains would have to be more than marginal to justify this.

We agree with the reviewer that this statement was not well-supported in our manuscript. As we have added tests of statistical significance to all our metrics (Figure 1-6) and magnitude figures to the new Appendix A, we believe there is now a stronger support for this statement, at least for some regions. Moreover, several hybrid models have been developed in the last months, including the popular Neural GCM (KochKov, 2024). In general, we do not see many hinders to at least simpler forms of hybrid usage of data-driven and physical modes (e.g. a simple weighted model average between the two), given that the global forecasts generated by the machine learning models require only a single GPU and very limited computational time. Indeed, several meteorological institutes, including ECMWF and NOAA, are already producing pre-operational forecasts both with physical and data-driven models, which could easily be coupled together.

Appendix

The same comments apply to the appendix figures as to the figures in the main text: • Some measure of statistical significance would be useful • For figures A5 and A6, some

accompanying figures showing the magnitude of the differences between the models would help give perspective on the significance of the spatial patterns in the figure. • For figures A4, A5 and A6, a different colour scheme would work better I think – one where each of the models is given a different colour (not just shades of the same colour), and a scheme which is not the same as the one used for showing the magnitude of the differences in scores (since this has a value judgement attached to it).

As suggested by the reviewer, we are going to implement the same changes to the main and the Appendix. Additionally, we will add some figures to the new Appendix D showing the magnitude of the differences in grid pointwise performance corresponding to Figure 5 and 6 in the main.

As an additional step to improving the clarity of our study, we will expand our discussion to address the limitations of our extreme metrics, emphasising that every metric has weaknesses, and that any attempts to make overarching comparisons between models should account for a range of different metrics simultaneously, as well as look at the performance of the forecasting models for the whole distribution of the variables, and not just at the tails. Specifically, QQ-plots and other reliability checks are key here, since they could easily expose attempts to hedge extreme metrics such as the tail RMSE. We will further add some figures related to this point to a new Appendix B.

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