

Dear Reviewer,

We sincerely thank the reviewer for the thoughtful and constructive comments. We greatly appreciate the careful reading of our manuscript and the valuable suggestions. A few comments identified specific errors in the original manuscript, which we have now corrected. The remaining comments helped us recognize places where the text could be made clearer and more accessible. As authors, we can easily become “blind” to our own writing, particularly when we are so familiar with the underlying ideas that we may assume certain points are self-evident. The reviewer’s comments helped us see the manuscript from the reader’s perspective and have therefore been very helpful in improving its clarity and overall presentation.

To make it easier to review our responses and identify the corresponding changes to the manuscript, we have included a diff version of the manuscript. This version clearly highlights the changes we have made in response to the reviewer’s comments, with additions and modifications marked in red and blue.

Review of “Effects of model resolution on predicting atmospheric ducting conditions with Harmonie-AROME” by K. Hamalainen, L. Rautiainen, J. Tyynela, and L. Laakso

This manuscript discusses the limitations of mesoscale weather models in detecting low altitude ducts in relation to their vertical and horizontal resolution. High and low resolution, in both vertical and horizontal directions (four total combinations), weather predictions are compared with observations made on a meteorological tower. Specifically, probability of detection and false alarm rate are examined and impact of the model resolution on these statistics is discussed. Findings show all resolutions examined were unable to detect near-surface ducts (<15 m), but ducts at higher altitudes (>30 m) were well predicted by all the model predictions.

The results presented in the manuscript are not particularly novel, as it is generally well known that mesoscale numerical weather models are not sufficient to resolve low altitude ducts, especially evaporation ducts, which is why they are often blended with surface layer models to resolve the lowest altitudes (as performed in some of the citations included in the paper). The paper does demonstrate these points using relatively unique data for direct evaluation of the numerical model predictions. For a topic area that has been studied extensively, the references are somewhat narrow. Also, more information about how the comparisons with the ground truth observations were performed is needed.

Below are some specific comments, but the manuscript is rated low mostly because of the lack of novelty and modeling advancement:

1. L41: Atmospheric stability influences the properties of an evaporation duct, but stable conditions are not solely needed for the formation of an evaporation duct. An evaporation duct (especially over the ocean) can form in unstable conditions as well as stable conditions. Furthermore, the first condition (high SST) is consistent with unstable conditions. Consider rephrasing.

This part of the text has been revised and rephrased.

2. L78: Eqn (1) is usually credited to Bean and Dutton 1966 – it is suggested to add this reference.

We have replaced the citation.

3. In general, the manuscript relies heavily on Turton et al., 1988 in section 3; but there are many more papers (as I’m sure the authors are aware) including the book by Bean and Dutton 1966 that discuss these points. In that sense, the manuscript should include more citations.

We have added more citations, including Bean and Dutton 1966.

4. L80: It is important to denote that q (specific humidity) is in units of kg/kg (the reviewer assumes that is what was meant by “[unitless]”).

Thank you for pointing this out. This was a clear mistake.

5. L85: “The best way to estimate signal propagation in the atmosphere is to use modified refractivity M ...” This statement needs to be rephrased – the best way to simulate signal propagation in the atmosphere is using a parabolic equation solution to Maxwell’s 1 equations. I think what the authors meant was: “The best way to model the atmosphere for simulating signal propagation uses modified refractivity, M ,...”

Yes, you are correct. We are purely concentrating on atmospheric conditions and modeling, excluding the signal propagation from the scope of this manuscript.

6. L86: “Modified refractivity takes into account the natural bending of the signal in the atmosphere due to curvature of the ground.” This should be rephrased – the signal doesn’t bend, the Earth does, e.g., “Modified refractivity takes into account the curvature of the Earth when determining whether refraction is bending signals towards the Earth or not.”

We agree that the original wording was inaccurate and may have caused wrong image to the reader. We have rephrased the sentence to clarify how the curvature of the Earth effect on refraction and signal propagation.

7. L101-106: There is a specific definition for duct strength – referred to as the M -deficit, which is the difference between M at the surface (or bottom of duct for an elevated duct) and M at the duct height. If this discussion is not referring to this definition of duct strength, then that should be clarified or reworded to avoid confusion. Furthermore, Table 1 is misleading because it suggests that refractivity (N) and duct strength are related – it is vertical changes in N that dictate the duct strength. So, for example, a uniformly cold and humid environment does not mean a strong duct is present as Table 1 suggests.

We agree that the original table and discussion could be misleading regarding the physical factors governing duct strength. We have therefore removed the table and substantially revised the corresponding text. The revised version emphasizes that ducting is governed by vertical variations in atmospheric properties, and particularly by the vertical gradient in modified refractivity, rather than by the absolute temperature or humidity of an individual layer.

8. L124-125: Duct formation, as the authors noted earlier in the manuscript, is very sensitive to sea surface temperature (SST), especially low altitude ducts. The use of a constant SST over a “whole forecast cycle” could cause issues with the identification of low altitude ducts. How long is the “whole forecast cycle”? This assumption of constant SST could influence forecast errors for ducting outside of upwelling situations, which should be discussed/mentioned.

As already mentioned in the manuscript, the use of a constant sea surface temperature (SST) throughout the forecast period may introduce errors. We have rephrased this part and added text to identification and prediction of low-altitude ducts, particularly outside upwelling situations. The forecast period considered in the test cases presented in this study was 24 h, and we have now clarified this in the manuscript and added a discussion of the potential impact of the constant SST assumption on ducting forecasts.

We are aware of this limitation and the potential errors it may introduce. Our current numerical weather prediction system is not coupled to an ocean model, and therefore the SST is not dynamically updated during the forecast. The operational forecasting system itself provides forecasts for longer periods, but the 24 h period was used for the test cases considered here.

The importance of this limitation has nevertheless been recognized, and we have an ongoing project aimed at developing a coupled atmosphere–ocean modelling system. Such a system would

benefit our operational weather, ocean, and wave forecasting systems and would allow for a more consistent representation of air–sea interactions and their potential effects on ducting conditions.

9. L133-138: Was there any rationale behind the selection of the four different model resolutions tested? Please add a brief explanation.

Thank you for this question. The selection of the four model configurations was primarily motivated by their relevance to our operational numerical weather prediction system. These configurations have previously been tested and evaluated within our forecasting system, although not specifically with respect to the prediction of atmospheric ducts. We therefore wanted to assess how the different configurations perform in predicting ducting conditions and whether increasing model resolution provides benefits for this application.

This study represents a first step towards assessing the capability of our operational weather forecasting system to predict atmospheric ducts. We recognize that the current model configurations have limitations for this purpose. However, establishing their present performance provides a useful basis for identifying areas where the modelling system can be further developed. We have now added a brief explanation of the rationale for the selection of the four configurations in the manuscript.

10. L146-149: More information on how the “ground truth” or observations were used to compute refractivity gradients is needed as well as more specificity in how those are compared with the model outputs. For example, are M-gradients computed discretely between each pair of mast measurement levels and compared to M-gradients computed between the same pairs of model height levels? Are the observations averaged over the forecast period? If model levels and observation heights don’t align in terms of heights (which is presumably the case), is one interpolated or averaged? In other words, when comparing values at a specific height level (e.g., labeled as 2m, 30m, or 59m) are mast measurements averaged over an equivalent height range as a single model level represents, or is the closest model height to the observation levels compared (meaning more than one observation height could be compared with the same model level)? Additionally, for the observations, could a noisy measurement cause the appearance of a duct, or are the observations time averaged before the gradient is computed? Given the importance of the ground truth to the presented statistics, much more elaboration is needed on how this comparison is carried out.

Thank you for this detailed comment. We agree that the original description of the observational data and their comparison with the model results was not sufficiently clear. We have therefore substantially revised and largely rewritten this section of the manuscript. The revised text provides a clearer description of how the observations are processed, how refractivity gradients are calculated, and how they are compared with the model results. We believe this gives the reader a more accurate understanding of the methodology and the basis for the presented statistics.

11. L234-235: “...: the closer the source is to the duct, the higher the likelihood of signal trapping.” This statement might be a bit misleading/unclear, if the signal is transmitted w/in the duct then it is more likely to be trapped depending on wavelength of the EM wave, but when outside the duct, then the closer the source is to the duct then the more likely it is to trap via leakage into the duct. Please clarify and rephrase.

We agree that the original statement was too general and could be misleading. We have therefore rephrased the statement.

12. L243-245: Ensemble forecasts of refractivity have been published and should be cited where discussed in the discussion section. (Zhao et al. 2016)

We have added new citations related to ENS-forecasting.

13. Figure 2: There is inconsistency in the use of dM/dh versus dM/dz throughout (this is one example). Suggest making the vertical coordinate a single variable either z or h but not both.

We have gone through the whole manuscript and unified the vertical coordinate to be h in all the plots.

14. Figure 2: The gray dots are not where $dM/dz < 0$ as denoted in the legend (which would be just negative values on the y -axis if they are labeled correctly). There are no units on any variable in this figure; are these normalized somehow, please explain or provide units.

This picture has been replotted and labels unified with the rest of the manuscript.

15. Figure 7: It would be more helpful to see three lines on three subfigures rather than 9 subfigures.

We replotted the figure as was succeeded.

16. Figures (generally): Should have labeled subpanels (i.e., a, b, c, etc.) and a description for each. All axis labels should include units unless the plotted quantity is unitless.

We have fixed the figures to include subpanel names/indexing and the units.