Referee #1, Dimitar Nikolov National Institute of Meteorology and Hydrology 1784 Sofia, Bulgaria

*We would like to thank Dr. Nikolov for the in-depth review and detailed comments. His comments are in **bold** and our replies to the comments are in normal font. We included here and replied only the critical comments.*

…

Specific comments

I would recommend the authors replace the references Rauber et al., 2000; Carrière et al., 2000 for the warm rain process (page 2) with (or add) the following two:

- **Bocchieri, J., 1980: The objective use of upper air soundings to specify precipitation type. Mon. Wea. Rev., 108, 596–603.**
- **Huffman, G. J., and G. A. Norman Jr., 1988: The supercooled warm rain process and the specification of freezing precipitation. Mon. Wea. Rev., 116, 2172–2182.**

The references recommended by Dr. Nikolov are indeed the ones to first describe the phenomenon, and are better than Carrière et al. (2000) in this context. We suggest keeping Rauber et al. (2000) and replacing Carrière et al. (2000) with Bocchieri (1980), and Huffman and Norman (1988).

I disagree with the decision of the authors to exclude most of the stations from eastern Europe. It seems that they have erroneously interpreted the explanations of Bezrukova et al. (2006) for the different definitions of FZRA events in these countries. Indeed, sometimes the icing due to supercooled clouds or fogs may deposit as glaze (wet growth process) and then the symbol for glaze is written down, but such a case will never be reported as freezing rain or freezing drizzle in the WMO weather codes. This ambiguity concerns mostly the local meteorological archives where additional control is needed to distinguish between both events. The weather codes 24, 56, 57, 66 and 67 are not affected at all. By this reason the authors (of Bezrukova et al., 2006) have decided to restrict only to the WMO codes.

Indeed, it seems to be likely that we misinterpreted the methodology of Bezrukova et al. (2006). However, based on our results, it is clear that when all stations from the domain (i.e. including eastern stations) are used in calibration of the FMICLIM algorithm, we get a prominent overestimation of algorithm-based total number of FZRA events in eastern parts and underestimation in other parts of Europe for some reason. When the eastern stations were excluded from calibration, the validation results enhanced slightly (and the underestimation of the number of FZRA events disappeared elsewhere, because of the unbiasing nature of the calibration procedure).

We agree that using all relevant data should be preferred. On the other hand, prior to further studies, it would be very important to explain why we get so different results in eastern Europe compared to other areas. Because the method (FMICLIM) and data (ERA-Interim) are the same over the domain, the only imaginable reasons, we think, are (1) local conditions favouring/inhibiting the formation of FZRA and/or (2) differences in how FZRA is observed in different countries. Because the eastern area we excluded contains both flat terrain and mountains, (2) might be more likely than (1). Moreover, it is perhaps unlikely that several totally independent observational data sets of FZRA would exist: maybe at least partly same data (or same observers) were used in datasets presented by Bezrukova et al. (2006). However, we admit that our reasoning here is rather speculative. Additionally, the division to ‘eastern’ and ‘other’ stations is rather coarse and should be refined in future.

We suggest modifying the text (Page 4, lines 5 -- 20) so that the misinterpretation of the paper of Bezrukova et al. (2006) is corrected, but keeping the selected set of stations as it is.

The authors of the manuscript have also filtered the data outside the interval –30oC +10oC, which seems to be too wide. Most often FZRA occur in the interval –10oC 0oC, so an appropriate interval for filtering, in my opinion, would be –15oC + 5oC. This would prevent to a certain extent from misclassification of ice pellets as FZRA or FZDR.

This is true. We suggest (1) testing how much data is filtered out using the proposed stricter interval, and if a large proportion is filtered out, (2) testing how much validation scores are altered. If major enhancements in validation scores are found in (2), we should perhaps also consider rerunning the calibration procedure.

The finding that the altitude does not contribute to the explain variance is somehow surprising for me. One would expect that the number of FZRA and their duration would decrease with the altitude because of the decreasing of the depth of the near-surface cold layer and FZRA aloft should be even more rare event

than the FZRA at the ground. However, mountain ranges mostly caused cold air damming which is difficult to be recognized in data sets with coarse resolution.

Actually, altitude is, as well, correlated with the total number of observed FZRA events, but slightly less strongly than the distance to the coastline. Since the altitude and distance are themselves quite strongly correlated, including both variables to the model did not enhance the result much compared to using just one of them. And because the distance was slightly better in explaining the variance, we decided to use it.

We suggest adding this clarification to the text.

The vertical resolution of the FMINWP seems to be not very appropriate for detailed representation of the vertical profiles of the relative humidity and the air temperature, which would affect the correct estimation of the near-surface cold layer and the melting layer above. It can be seen that an increasing of the resolution is foreseen as future work and this would be very helpful.

Using a coarse vertical resolution, despite the drawbacks, was partly selected because output data from climate models, which we are going to analyse next, is commonly available in a rather coarse vertical resolution as well, and one purpose of this paper was to show that some kind of results can be achieved also by that way.

We suggest adding this clarification to the Introduction and Conclusions parts.

The minimum acceptable cold layer depth has been significantly increased by the calibration procedure – from 130 meters up to 400 meters. This seems very reasonable because of the large size of the investigated area and variable weather conditions. For example Bernstein reported values of the near-surface cold layer in USA between 100 and 1400 meters, the minimums being between 100 and 300 meters.

- **Bernstein, B., 2000: Regional and local influences on freezing drizzle, freezing rain, and ice pellet events. Wea. Forecasting, 15, 485–508.**

We thank Dr. Nikolov for this information. We suggest completing the text based on this comment, and adding the new provided reference.

…

Very interesting results are presented in the paragraph 3.3 Climatology of freezing rain in Europe. However, the finding for a maximum in the annual number

of events over the Carpathian mountain sounds surprisingly for me. It would be useful if the altitude of these regions is given.

This is an important comment, and reveals one weakness in the results: the FMICLIM algorithm cannot detect FZRA reliably at high altitudes (perhaps >2000m or maybe >1750m), because there the pressure levels used are too few to represent the cold layer -- melting layer structure. We suggest replotting the maps (Figs. 8 and 9) using a mask which hides the suspicious high altitude results. Also, analyses including the high altitude results should be recalculated (e.g. Fig. 7) by excluding the high-altitude data.

We also suggest trying to include elevation information to Fig. 1, but there is a risk that readability of the figure deteriorates. If it gets too low, we perhaps should not include elevation.

Technical comments

I have encountered only two small misprints – on page 9, third row – the FMICLIM is written wrongly and on page 11, third row is written "The Carpathian...".

We suggest correcting this.

Conclusions

…

The size, quality and readability of each figure is adequate to the type and quantity of data presented, except for figure 5 which could be a little bit larger.

We suggest enlarging the figure, or replotting it so that markings and details are clearer.

The authors give proper credit to previous and related work with a small oversight of two references for the warm rain process.

We suggest adjusting the references as proposed previously.

…

The authors should only take into account that they could use for future investigation the stations in eastern Europe with no restrictions, as far as they utilize the international weather WMO codes.

We agree that using all relevant data should be preferred, but we suggest keeping the restrictions as they are in the paper. We admit that the evidence to support that decision is not as solid as we would like it to be, but it is the ‘least bad’ choice in our opinion.

…