

Interactive comment on “Air Quality Predictions with an Analog Ensemble” by Luca Delle Monache et al.

Anonymous Referee #2

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The paper presents the use of an analog ensemble (AnEn) technique to improve air quality forecasts. The AnEn is applied to outputs of a numerical model for air quality, specifically on O₃ and PM_{2.5}. It relies on past observations and the corresponding forecasts to draw an ensemble of analog situations. The improvements in the forecast are assessed by means of different scores, and compared to references.

The presented method seems to improve the forecast in different ways and it might be relevant for this kind of applications. However, as I'm not working with air quality, I cannot judge how the method stands against other model output statistics or statistical postprocessing techniques in that context. The paper also gives the impression that the authors do not come from this field, as the provided specific literature on that topic is rather poor. There is no mention of other M.O.S. approaches, whereas there should be

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some. Moreover, I'm not certain about the novelty of this study compared to previous works of the authors.

The whole manuscript is not so well written and is often difficult to follow. It should be rewritten in a more fluent way, and it should better describe the methods used. The frequent use of "we" and "our" is inadequate. A substantial work on the language should be done through the whole manuscript.

The predictors used in the method are introduced very late, in the middle of the results, while they should be introduced earlier. Moreover, there is no justification for the choice of these predictors. Please better explain the choice of the predictors and the method itself.

How does the method perform for extremes ? I suspect that the peaks, which are the most relevant to forecast, might not be well covered by the ensemble due to the very limited size of the observations that can be used as analogs. Additionally, how would the derived deterministic time series (the mean) work for more extreme values?

Specific comments:

- The calibration / verification periods should be clearly explained in the beginning of the manuscript, and the independence of the verification period specifically detailed. It is not clear which results are provided for the calibration or verification period. Is Fig. 2 in the verification period ?
- The number of references are unbalanced. There are too many for some points (e.g. P2L14-17), while some assertions have no reference.
- P3L4-5: not clear
- P3L16-17: issue with the ()
- P5L5: geopotential height at 500hPa is generally a predictor in analog methods, not a predictand (likely the same for 2-m dew point)

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- P6L19: t=1: what is the unit ? days ?
- P7L2: specify the section where the number of analogs is optimized
- P10L8: Figure 3
- P11L2: sensitivities ?
- Figure 2: On the verification period? Is it the best reproduced days, or are they representative of the skill of the method?
- Figures 3, 4, ...: a) b) c) d) not present on the figures
- P14L3: May and Nov or May to Nov ?
- P17L11-14: + they might not sample the observation archive uniformly
- P20L2-5: Not clear how you process it
- P20L12: They are = or very close in some cases!
- P21L18-19: not clear
- P22L2: a slightly better resolution, but not much. . .
- P24L16-18: Not clear which spread you are taking about
- The summary should not contain the details of the periods, but the results should be more discussed.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-1214>, 2018.

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