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Interactive comment on "IntelliO3-ts v1.0: A neural network approach to predict near-surface ozone concentrations in Germany" *by* Felix Kleinert et al.

Anonymous Referee #2

Received and published: 22 September 2020

The authors present a data-driven forecast model for maximum daily 8-hour ozone (mda8O3) concentrations based on multiple convolutional neural layers and apply it to the network of rural ozone monitoring sites in Germany. The manuscript is well written, and the model presented and its application make a valuable contribution to the field. I ask the authors for a few clarifications and specifications detailed in my specific comments below.

Specific comments:

1) The authors include several meteorological variables as input but not radiation. This is a little odd given the importance of radiation for photochemistry and thus ozone formation. Cloud-cover is used by the authors together with temperature as a proxy, however I am not surprised that this surrogate variable shows limited influence on

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Discussion paper

mda8O3. It seems also that direct and diffuse irradiance are available at various time steps (https://reanalysis.meteo.uni-bonn.de/?Download_Data___COSMO-REA6) although otherwise stated on page p4, L93.

2) Related to my previous comment I am a bit puzzled that all meteorological covariates show such limited added skill. Have the authors assessed simple brute-force correlations between meteorological covariates and mda8O3, are they as low as the skill score would suggest? Also I wonder if the limited influence stems from the joint consideration of all seasons and if a cleaner picture would emerge on seasonal or monthly basis.

3) Along these lines, I am wondering if pooling of observations in the samples might cause some spurious effects. Given the relatively low VOC abundances during fall-early spring pooling might explain to some degree the relative low skill obtained for NO and NO2.

4) On p.5., L103 the authors state that they include stations if they have at least one year of valid data in one of the sets. I wonder if an unequal inclusion of observations from different time periods affects the robustness of the training and tuning. How much would the sample reduce if a more stringent criterion would be applied say e.g., more than 80% or 50% coverage over the considered time period, or a high fraction of available data per month, season and year?

5) Also O3 and NO and NO2 show substantial changes and trends in Europe over the time period considered. The authors do not address this in their manuscript, thus I assume the data has not been detrended before use in model training and validation? What magnitude of effect would we expect by considering non-stationary time series training of a CNN model?

6) The authors use dma8 for NO and NO2. What is the motivation to use here dma8 instead of the daily mean or maximum value, which would be the more common quantities?

7) P6, L135, a batch size of 512 samples is used, how do the authors derive this number?

Minor comments:

1) I suggest to use ‘training set’ instead of ‘train set’ throughout the manuscript.

2) I suggest grouping several figures to multi-panel figures to increase accessibility (7a-d, A3a-d, A4a-d,A5a-d, A6a-d).

3) Figure A2 is incredibly hard to read even when zooming in to 400%.

4) Axis labels of Figure A2 are hard to read.

5) P7, L174 a reference for the Adam optimizer is missing

6) I was wondering if the section on joint distributions and skill scores could not be moved to the Appendix

Spelling and typos:

P6, L118: hyperparameters

P9L226: replace ‘model with ‘models’

P10, L235: replace ‘observation’ with ‘observations’

P10, L237: replace ‘multi-valued’ with ‘multi-value’ and check throughout the text

P11, L295: therefore more credible

P11, 297: replace ‘the network under-forecasts’ with ‘the forecast is underestimating’

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