Responses to the referee comments: Assessing the climate and air quality effects of future aerosol mitigation in India using a global climate model combined with statistical downscaling

Miinalainen, T., Kokkola, H., Lipponen, A., Hyvärinen, A.-P., Soni, V. K., Lehtinen, K. E. J., and Kühn, T.: Assessing the climate and air quality effects of future aerosol mitigation in India using a global climate model combined with statistical downscaling, Atmos. Chem. Phys. Discuss. [preprint], https://doi.org/10.5194/acp-2022-513, in review, 2022.

We thank Anonymous Referee #1 and Anonymous Referee #2 for the efforts made to review our paper and for the valuable comments. Our responses are written below each comment separately. The referee comments are marked with yellow color and italic, and the author replies are marked with gray color.

Replies to the comments made by the Anonymous Referee #1:

This manuscript presents global model simulation results over India from ECHAM for air quality and radiative forcing under present and future emission scenarios (from GAINS) through 2030. For the region covering Delhi, the results were downscaled using random-forest corrections using multiple emission, met, and orography variables.

While the radiative forcing calculations from ECHAM is a known path, the use of the same to bias correct and estimate air quality for a city is new. The later methods have been used, but not for model resolutions at 1.9 degrees. Machine Learning (ML) approach is a new and emerging field and the benefits of using a global model for both air quality and climate applications cannot be overlooked. While the methodology is well explained for correcting the model results with biases from ML, the statistics also improved after the corrections, the gaps between the measured and model-corrected numbers is still significant.

We thank Anonymous Referee #1 for the useful comments.

As the referee mentioned, the RF-corrected PM2.5 does not fully reproduce the station-averaged PM2.5 for the testing phase data, especially over short time scales. This is analyzed and discussed in Section 3.1.

However, if the aim is to capture the long-term trends correctly, the RF-corrected PM2.5 performs as adequately as the PM2.5 obtained from the dispersion model System for

Integrated modeLling of Atmospheric coMposition (SILAM). In the Table below, we show the comparison statistics between modelled and mean of the New Delhi ground stations for the testing phase period 22.8.2018-31.12.2019.

Compared to the mean of the measurement station PM25 for 22.8.2018-31.12.2019, daily average values			
	ECHAM PM2.5	**SILAM PM2.5**	**RF-corrected PM2.5**
RMSE (µg/m³)	113	65	56
MRE (%)	-36	27	33
MAE (µg/m³)	78	46	39
R^2	-0.63	0.46	0.59
Pearson correlation	0.19	0.79	0.79

The error statistics show that the RF-corrected PM2.5 correlates with the measured data at the same level as SILAM PM2.5 does. Furthermore, the mean absolute error (MAE) and root mean squared error (RMSE) are smaller for RF-corrected PM2.5 than for SILAM data. The mean relative error (MRE) is smaller for SILAM than for RF-corrected data, but the difference is less than 10 percent units. This comparison suggests that the performance of the downscaling approach is comparable to air quality models if the aim is to utilize data to applications where long-term trends in air quality are central.

The scenario analysis for air quality primarily hinges on the reproductive capacity of the model and the only question that is not clearly answered is why extract air quality data from such a coarse model (when the problem is known that coarser models have hard time replicating high-density urban areas with very distinct emission characteristics)? Especially, since FMI and IMD (author organizations) are known to conduct chemical transport modeling for air quality at much better resolutions globally and in India.

The referee is correct that the downscaled PM2.5 does not fully correspond outputs from high resolution models when comparing day-to-day variation. However, this was not the aim of this project, and we presume that the concept of our manuscript was misinterpreted by the referee.

The main idea of our study is to expand the possibilities to utilize global model data in additional applications such as local air quality analysis. As we describe in the manuscript text (line 522 and forward), one advantage of using global-scale models for analyzing the effects of aerosol mitigation is that one can simulate fairly long time periods (decades to even a century), and that the simulations typically cover the whole globe. With the help of downscaling, one can "zoom in" to a very specific location and analyze how the global or local scale emission mitigation affects. Up to our understanding, air quality models are computationally more expensive due to the high grid resolution, and therefore the simulated time periods are shorter than with GCMs, and the simulation times can be much longer.

Furthermore, another advantage of using downscaling enhanced PM2.5 from a GCM is that one can simultaneously analyze the effects of aerosol mitigation on various other climatic processes. In our case, we focused on the radiative forcing, but one could, for instance, analyze global precipitation patterns or low-level cloud formation at very distinct regions in the globe.

One plausible application of downscaling PM2.5 could be simply to use it as a "quick tool" to evaluate how a mitigation scheme affects climate or air quality in different parts of globe. The intention of this study is not to present a method which can be used to replace chemical transport models. Instead, the main idea is to provide a relatively light-weight tool that can be used to assess simultaneously the climate and air quality effects of future aerosol emission reduction measures.

Furthermore, many air quality models require simulated data from global climate models for modelling the future climate. Regional modelling with fine grid resolution requires input for boundary conditions for large scale atmospheric dynamics, and therefore a separate global model simulation is needed before the actual air quality model simulation can be performed. Our intention was to explore if we could use the global model data directly also to analyzing surface air quality, especially if there is no need to study the underlying physical mechanisms in a detailed way.

We will update the Introduction text to communicate better our motivation and the core concept of the study.

Why use a city like Delhi with so many stations with 0% data available in the ML testing phase? Why not use a city in Europe or the US with good availability rates and good representation of the sources, to show that the model is capable of replication after the bias corrections?
We assume this is a misunderstanding as the majority of the stations used in this study have a coverage of 80 to 90% for testing phase (see Table S1). To emphasize this point, we will add a couple of statements about data availability in Section 2.3.

The one drawback of the manuscript is the selection of the case study city (Delhi) -- which has strong seasonal trend, strong diurnal trend, and distinct sources (for SO2, BC, and OC) over the months. A city(s) or region(s) with consistent emission loads would cut down some uncertainty in the model and corrections methods and then apply to regions like India and China.

We chose New Delhi as a case study city as it is ideal for this kind of study. The PM2.5 concentrations are at a high level for most of the year in New Delhi. Furthermore, Delhi National Capital Territory (NCT) is densely populated and relatively large in surface area. In addition, the ground measurement station network in New Delhi is very extensive and there was a sufficient amount of data available for our study.

Furthermore, we respectfully disagree with the referee that the unique characteristics backgrounding New Delhi pollution profile would make Delhi unsuitable for this kind of analysis. On the contrary, as we mention right in the first sentence of the abstract text, we aimed to study the potential of the downscaling procedure. As the referee described, the New Delhi PM2.5 is not constant all year around but has a lot of seasonal variation and strong dependence on anthropogenic sources. This makes New Delhi a good target region to explore how well downscaling can capture local tendencies, such as short- and long-term trends in PM2.5.

Anthropogenic aerosol emissions are relatively large in India (~15% of the global BC emissions), and they are not projected to decrease at a same rate as global emissions (see manuscript Table 1). That is why we considered that India and New Delhi are interesting areas to study, as the aerosol mitigation is expected to bring clear benefits to the local air quality, but the net radiative forcing due to simultaneous mitigation of three species (BC, OC and SO2) was unclear. We will update both the Introduction text and the text in Section 2.3 to give readers a clear impression of why New Delhi was chosen as a study of interest.

Unfortunately, it seems that we have not reported clearly enough in our manuscript that our bias correction model is meant to be built separately for each city. Though technically possible, it would require a very large global training dataset representing a wide variety of conditions to construct an ML-based model that would be able to generalize from one region to another. In practice, this type of global dataset does not exist. The characteristics of PM2.5 concentrations trends and how they depend on prevailing atmospheric conditions and the assumed local emissions are unique for each city/region. We used the same city for training and testing the RF Model to ensure representative training data for the model. We will emphasize this better in Section 2.7 and add a description of why there needs to be a separate model for each city.

Line 237-242 and 290: It is not clear if the emissions and other variables extracted and used are still at the ECHAM resolution or further downscaled to support a region of 30km x 30km over Delhi? (L290) is an important observation - When making the bias corrections, besides the model grid variables, are there any variables that are seggregating the Delhi area signatures for a better fit?

We thank the referee for making us aware that this aspect was not clear enough in the manuscript text. The input feature values used in the RF bias correction are all from one specific ECHAM-HAMMOZ simulation, and the data were not further downscaled to represent higher resolution grid data. Downscaling from T63 to L290 would require a large amount of external fine resolution data and would not serve the purpose of lowering the computational costs.

Instead, we post processed the input feature data by extracting values from the original ECHAM-HAMMOZ data of T63 horizontal resolution. We produced input feature data either by extracting data from one single grid box (‘point’), or by summing (‘fldsum’) or averaging (‘fldmean’) over an area surrounding New Delhi (72 – 83 ‘W, 24 - 34 ‘N). This means that some of the input features represent more the regional changes in the simulated atmosphere, whereas the point features show more the local changes.

In our modeling set up, it is assumed that the measurement data from ground stations incorporate information about the very detailed characteristics affecting New Delhi pollution levels, such as urban infrastructure or very specific emission sources. Including detailed external data as an input feature (e.g., info about public traffic routes) would have reduced the feasibility of our modeling approach since external data is easily subject to change and would need to be kept updated constantly, or be based on additional scenarios or assumptions. Then again, the absence of detailed, local information is most likely the reason why our modelling approach is not capturing short-term variations very well. Developing such downscaling bias correction method that could also include external data could be an opportunity for future studies.

We will clarify these topics in the manuscript text in Section 2.6 and describe better the selection of input feature variables. We will also add discussion about potential future studies including very detailed information about local factors affecting air pollution levels.

The results and conclusions of the study in terms of AQ and climate benefits of reducing emissions are as expected. However, since Delhi is the most polluted area/city in the world with not only a complex mix of emission sources, but also a complex mix of political and instititutional setup to manage these emissions. While the manuscript presented % changes (benefits for air quality and RF), the discussion doesn't include any explanation on how these % emission reductions will be acheived in the Delhi area. It is understood that the emissions work comes from a different model (GAINS). Since the manuscript very specifically mentions and analyzes data for one city only, it would be appropriate to also discuss this space.

We would like to highlight here that it is not explicitly evident that future aerosol mitigation would bring a net negative forcing over India. As many studies have shown (for instance, Allen et al.,2020), simultaneous reductions in both scattering and absorbing aerosols are expected to reduce the net cooling effect of aerosols on a global scale.

The aerosol mitigation scenarios used in this study were part of the ECLIPSE V6b emission scenarios, which were designed with the GAINS model. The underlying assumptions in ECLIPSE are idealized in the sense that emission reductions are based on assumed perfect achievement of currently valid legislation (CLE) or full implementation of all currently available technologies to reduce emissions (MFR). In this sense these scenarios could be seen as maximum possible emission reductions from the 2020 viewpoint. Analyzing the underlying political actions needed to achieve the proposed emission reductions is beyond the scope of this study. A more detailed description of the ECLIPSE scenarios can be found from e.g. Stohl et al. (2015), Belis et al. (2022) and Klimont et al. (2017).

However, we will add a brief summary in Section 2.4 about the most significant source sectors reduced in the ECLIPSE MFR scenario. In addition, we will extend the discussion part in Section 3.2. to briefly mention possible source sectors that might have been contributing to air quality improvements the most.

While there is merit to a new methodology to be able to model AQ data along with the climate data, the manuscript lacks punch and I am afraid that these correction results will be hard to replicate in another setting.

While we appreciate the referee's feedback, we respectfully disagree that the proposed method would not be applicable for analyzing the effects in another city or region if trained and applied with suitable data. We do, however, recognize that the performance of the bias correction may be different for each target city.

As mentioned above, there might be a misunderstanding that we would propose to apply the model trained with New Delhi data to correct air quality levels for a different city. In case this referee's comment is based on that kind of assumption, we will elucidate in the updated text that the designed RF models are city specific.

In addition to the already mentioned improvements, we will go through the whole manuscript text and highlight the core concepts to sharpen the key messages of our study.

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Replies to the comments made by the #Anonymous referee 2:

Review of "Assessing the climate and air quality effects of future aerosol mitigation in India using a global climate model combined with statistical downscaling"

The manuscript explores the possibilities of using a global climate model to investigate the effects of aerosol mitigation in India. A machine learning (ML) approach using Random Forest regression is used to downscale PM2.5 concentrations over a polluted city, New Delhi with the help of measured PM2.5 concentrations. Different PM loading future scenarios are projected and compared with the uncorrected and ML-corrected model outputs. The effects of aerosol mitigation are investigated in terms of radiative effects and effective radiative forcing under the PM future scenarios. The authors claim the improvement of global-scale model output in simulating the PM2.5 concentration over a small domain and their effectiveness in estimating the radiative impacts. The study demonstrates the potential of the emerging technique of ML in improving the large-scale model output in the process of statistical downscaling. The study is relevant and unique as mentioned above, and has a significant contribution to the relevant *scientific domain. However, some concerns remain significant and need to be considered before publishing.*

General Comments

The manuscript focuses on two aspects. (1) Demonstration of an ML technique in improving a global-scale model to simulate the PM2.5 concentrations over a small region via statistical downscaling under different emission scenarios. (2) Estimating the radiative effects of future aerosol scenarios using the RF-corrected model simulations. The manuscript structure is difficult to follow until reaching the present 'Conclusions' section which is not a conclusion, but a nice overview/summary of the entire work.

We thank the referee for the insightful suggestions to improve the manuscript structure and content. We will re-organize some of the subsections in Section 2 and rename Section 4 as suggested.

If the authors want to highlight their simulation results regarding the impact of future aerosol mitigation, more discussion is needed with proper references to the existing findings, else it remains as a technical paper demonstrating the potential of ML in statistical downscaling. Currently, the physical mechanisms for some of the simulation outcomes are not given/found, but some tentative reasons are proposed. Many studies are documented the current aerosol-impact scenario using multiple scientific techniques (in situ, remote sensing, etc.) and future projections also for the Indian region.

We will add more discussion and clarification about the potential physical mechanisms underlying the effects of mitigation and will also insert citations to relevant studies in Subsections 3.2 and 3.3. Further information is also included below in our answers to the detailed comments.

To ascertain the second aspect of the current manuscript, the first part needs to be flawless and should be explained confidently. Many parts of the manuscript are confusing which calls for further explanations for the smooth reading.

We thank the referee for bringing up that some parts of the manuscript may be confusing to the reader. We will go through the entire manuscript and improve the general readability of the text.

One of the highlights of the findings is that the improvement of air quality is mostly due to the reduction of OC loading. However, the negative radiative forcing is attributed to the reduction of BC emission. This is an example of confusion arising while going through the manuscript.

This was an excellent comment and helped us to understand which parts of our manuscript are confusing. The reason why OC influences air quality more than BC, though BC has stronger effects on the radiative balance, is related to the optical properties of these aerosol species.

The atmospheric mass load of BC is relatively small compared to the mass load of OC. The radiative forcing per mass load of BC, on the other hand, is much bigger than for OC, as the optical properties differ. That is why in this study the emission reductions of OC influence PM2.5 concentrations more, while BC-reductions have a larger impact on radiative forcing. We will update the text in Sections 3.2. and 3.3 and clarify how different aerosol species might have very different effects on radiative balance and air quality.

The map of India shown in Fig. 3 is not matching with the maps published by the institutions that provided the insitu data nor with one of the authors' affiliated institution. Please correct the map as per the source or remove the political boundaries as per the journal's recommendations.

We thank the referee for pointing out this issue. We will remove the political boundaries from Figures 3 and S3.

Language also needs improvement. The main concerns are listed below.

Methodology:

- *Why does section 2.2 stand apart from sections 2.6 and 2.7?*

We will move Section 2.2 below Section 2.5 and update the numbering of the subsections.

- *The hyper parameters were adjusted using different combinations based on the best error statistics. Can you please show the performance of the validation test data?*

This statement in the manuscript was somewhat inaccurate as the hyper parameter selection seemed not to affect the model performance substantially. The error statistics for testing different RF parameter combinations are presented in the Figure below.

max_depth: None, Max_features = 7	nr of trees	20	50	100	200	300
	RMSE ($\mu g/m^3$)	56.56	56.52	56.43	56.52	56.54
	MRE (%)	32.29	31.30	31.98	32.21	32.26
	MAE ($\mu g/m^3$)	38.69	38.46	38.57	38.57	38.65
	R^2	0.59	0.59	0.59	0.59	0.59
	Pearson correlation	0.79	0.79	0.80	0.79	0.79

max_depth: None, Max_features = None	nr of trees	20	50	100	200	300
	RMSE ($\mu g/m^3$)	56.79	56.81	56.90	56.84	56.76
	MRE (%)	33.15	33.52	33.46	33.08	33.11
	MAE ($\mu g/m^3$)	39.02	39.09	39.16	39.09	39.01
	R^2	0.59	0.59	0.58	0.59	0.59
	Pearson correlation	0.79	0.79	0.79	0.79	0.79

max_depth: None, Max_features = 15	nr of trees	20	50	100	200	300
	RMSE ($\mu g/m^3$)	56.73	56.43	56.45	56.45	56.53
	MRE (%)	32.15	32.54	32.63	33.27	32.62
	MAE ($\mu g/m^3$)	38.57	38.61	38.69	38.81	38.75
	R^2	0.59	0.59	0.59	0.59	0.59
	Pearson correlation	0.79	0.79	0.79	0.79	0.79

max_depth: 10, Max_features = 7	nr of trees	20	50	100	200	300
	RMSE ($\mu g/m^3$)	56.78	56.78	56.59	56.54	56.51
	MRE (%)	31.72	32.39	32.58	31.78	31.91
	MAE ($\mu g/m^3$)	38.70	38.77	38.77	38.56	38.57
	R^2	0.59	0.59	0.59	0.59	0.59
	Pearson correlation	0.79	0.79	0.79	0.79	0.79

max_depth: 5, Max_features = 7	nr of trees	20	50	100	200	300
	RMSE ($\mu g/m^3$)	56.93	57.00	57.12	57.09	57.12
	MRE (%)	31.65	31.18	31.80	31.87	31.76
	MAE ($\mu g/m^3$)	38.77	38.76	38.99	38.96	38.96
	R^2	0.58	0.58	0.58	0.58	0.58
	Pearson correlation	0.79	0.79	0.79	0.79	0.79

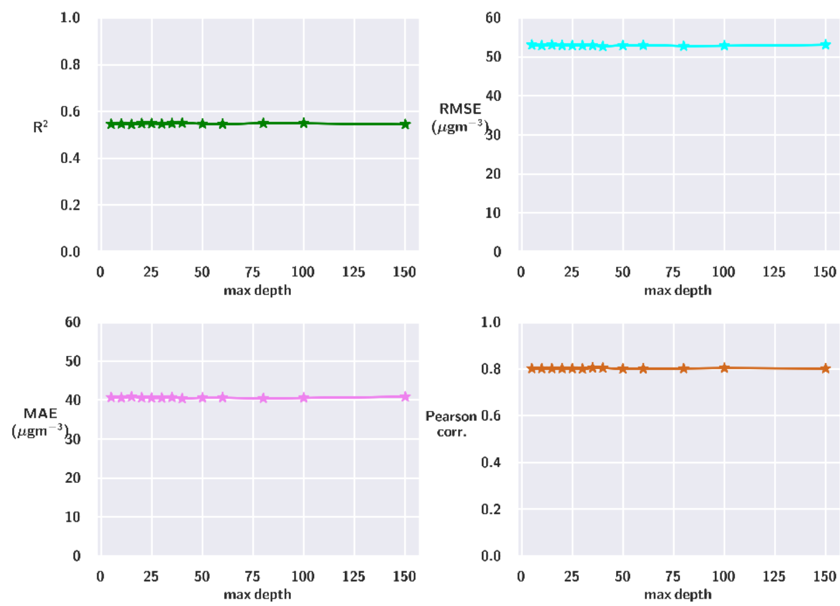
max_depth: 15, Max_features = None	nr of trees	20	50	100	200	300
	RMSE ($\mu g/m^3$)	56.84	56.83	56.85	56.79	56.68
	MRE (%)	34.39	33.11	33.62	33.26	33.19
	MAE ($\mu g/m^3$)	39.33	39.09	39.20	39.06	39.02
	R^2	0.59	0.59	0.59	0.59	0.59
	Pearson correlation	0.79	0.79	0.79	0.79	0.79

As the statistics show, our RF model setup is not overly sensitive to the selection of hyper parameters. Hastie et al. (2009, p. 590) reported a similar tendency. Based on their experiences, random forests require very little tuning.
Furthermore, our modeling approach had 31 RF models (one per measurement station), and we used the same RF hyperparameters in each of them, as we did not tune each station-specific RF model separately. We therefore decided to use the default, recommended values in the RF models. We will update the manuscript text to describe the selection of hyper parameters in a more transparent manner.

• *What do you mean by setting the depth of each tree to infinity? How can you make sure of avoiding over-fit while keeping the depth of the tree as infinity? What are the criteria for fixing the number of trees? It is said that a default value of 100 is taken as the number of trees in the present study. Why 100?*

By setting the depth to infinity we indicated that we did not set a limit to the tree depth. This was a slightly misleading way to report that the “max_depth” parameter in the Scikit Learn Python module was left as default value, “None”. When there is no maximum depth assigned, the algorithm will expand the regression tree nodes until the so-called leaves meet the purity criteria (mean squared error). We will rewrite some parts in Section 2.2 to clarify this point.

We tested how the maximum depth of the trees affects the model performance by altering the max_depth parameter and applying the trained model to year 2020 data (I.e., outside of training and testing phase data). The results are presented in the Figure below.



As Figure above shows, our RF modelling approach is not very sensitive to the max_depth parameter. Therefore, we chose not to fix the maximum depth of a tree in our modelling approach. Note that the root mean squared error and mean absolute error values are slightly larger for the year 2020 as the COVID19 pandemic caused an unusual, long-term drop in the surface PM2.5, which was not accounted for in the ECLIPSE emission inventories.

The number of trees was set to 100 based on a trial-and-error approach. There was no significant improvement if we increased the number of trees (See Table above). Due to these reasons, the hyper parameters were not adjusted based on tight cross-validation routines, but were selected near the recommended, default values.

• *Why the feature importance values are normalized, by doing so what is the chance of smoothing the non-linearity of the dependence of the input variables? Isn't there any criteria to fix the number of input variables? As the authors have pointed out the input variables are* *mutually correlated which is obvious in the atmosphere, including all of them may lead to over-fitting. What is the authors' claim on this point?*

The feature importance values are normalized to make them more comparable to each other. They represent the contribution of each variable on the reduction of the error criteria, and the normalization scales these to a scale from zero to one. Without normalization, the importance values would be more difficult to interpret as they would always depend on the computed total error of the particular RF training setup. Furthermore, the normalizing routine is a default setting in the applied SciKit Python library.

It is true that, since there are correlated input variables, there is a small risk of overfitting. We did preselection for the input variables based on feature importance values. We excluded variables that had an average feature importance value close to zero. Furthermore, for each regression tree, the maximum number of input features to be used when looking for the optimal split was fixed to 7 (See Section 2.2), so that there is a randomized set of features for splits. However, we did not carry out a stringent optimization routine to prune the number of input variables to as low as possible. This was because we had 31 stations and thereby 31 RF models. Each of the stations has distinct characteristics, and therefore the optimal set of input variables is slightly different for each station. We estimated that using the same set of input variables for all RF models could result in a more harmonized outcome. In order to minimize the risk of overfitting, we will re-evaluate the set of input features and analyze the impact of highly correlated input features on the RF predictions.

Furthermore, we will update the Section to describe more explicitly how the input features were selected. In addition, we noticed that there were some inconsistent descriptions of the modelling setup, and we will update those to correspond to our modeling parameters.

We further noticed that the description of the algorithm implementation slightly differed from the actual modelling setup. We will therefore update the sentence describing the "max_features" property. In addition, the bootstrap bagging method was not applied, and therefore we will remove the sentence indicating that. We further tested the model performance with different bootstrapping parameters, and there was no detectable improvement whether the bootstrapping was applied or not.

- *For the global-scale modelling, ECLIPSE V6b emission scenarios are used. How appropriate is this emission inventory for simulating PM over the Indian region or what are the criteria for selecting this inventory for this study? What is the contribution of this inventory to the high under-estimation of PM loading by the model over Delhi as shown in the manuscript?*

A detailed description of previous versions of the ECLIPSE inventories is presented in Klimont et al. (2017) and Stohl et al. (2015).

The ECLIPSE V6b emissions are described in detail in the forthcoming Arctic Monitoring and Assessment Program (AMAP) report. They were especially developed to study emission reductions of short-lived climate forcers (SLCF) on the global and Arctic climate. Unfortunately, the publication of the report has been put on hold due to the current political situation for an indefinite amount of time. However, Belis et al. (2022), von Salzen et al. (2022) and Whaley et al. (2022) provide brief descriptions of the ECLIPSE V6b scenarios.

Furthermore, as we mention in the manuscript text lines 449-451, Whaley et al. (2022) state that in ECLIPSE V6b, the recent declines in Asian SO2 and BC emissions are considered. This suggests that ECLIPSE V6b is a better choice for modeling South Asian aerosols, since some emission inventories, such as CEDS emissions from CMIP6 simulations, might lack this decline (Wang et al. 2021).

We will update the manuscript text to include additional citations for both ECLIPSE inventory and the GAINS model.

• *How the exclusion of the mineral dust component solves the issue related to the PM2.5 peaking? How authors can make sure that this exclusion won't affect the other simulation results?*

Here we want to clarify the RF-correction procedure. Instead of using RF to directly predict surface PM2.5 values, we predict a correction term to the PM2.5 values modelled by ECHAM-HAMMOZ, which has been shown earlier to give better results (e.g., Lipponen et al., 2013). For the computation of the correction term, we use all the ECHAM-HAMMOZ parameters listed in Table 3, which does also include the PM2.5 due to mineral dust component and mineral dust emissions from ECHAM-HAMMOZ. However, because during the RF training phase the mineral dust component shows very large peaks which do not correspond to the measurements, we exclude mineral dust from the PM2.5 value to which the correction term is added. Therefore, the correction term includes an inferred amount of mineral dust as the random forest models get information about mineral dust episodes as an input. In this sense, the mineral dust component is not (entirely) excluded from the RF-correction procedure. We will clarify this in the manuscript.

The difference between PM2.5 with and without mineral dust can be seen in Figure S2. Most of the very high peaking values in ECHAM-HAMMOZ data are during summertime, and due to mineral dust. The daily average of the measurement stations, on the other hand, does not show summertime peaking values that would exceed the winter month maxima. Including dust component in the ECHAM-HAMMOZ PM2.5 might have produced the highest values for the summer months, as our bias correction is additive. We agree with the referee that excluding mineral dust from ECHAM-HAMMOZ PM2.5 when calculating of the error term between station PM2.5 and ECHAM-HAMMOZ might affect results to some level as the very short-term peaks might be suppressed. However, our aim was to model the long-term effects of aerosol mitigation, and that is why we chose to prefer improved seasonal trends over short-term minima and maxima values.

We will add text in Section 2.2 to describe why avoiding dust peaks was necessary.

• *Coming to the radiative forcing calculations (section 2.8), how is the definition given to the radiative forcing is related to the conventional definitions found in the published literature? If there is any difference please highlight and justify those, else give supporting citations.*

Thanks for the suggestion, we will insert additional citations in Section 2.8. For the calculation of RF_{ARI}, we have described in the manuscript text the small differences between our definition and the conventional definition published in the 5^{th} IPCC assessment report. The ERF calculations are according to the latest IPCC assessment report (see answer below).

• *How the effective radiative forcing is estimated?*

The ERF values were calculated as described in Section 2.8, and follow the definition proposed in, for instance, the latest IPCC Assessment report (Forster et al., 2021). The ERF is the difference between the top of atmosphere (TOA) net radiative fluxs of a perturbed (MITIG_2030, CLE_2030) and reference (PRES) simulation. All the simulations have fixed sea surface temperatures (SST) and sea ice cover (SIC), and the meteorology is allowed to evolve freely, i.e., no nudging was applied.

As we mentioned above, we will add more references in Section 2.8. Furthermore, we will clarify the explanations in Section 2.8 to explicitly mention the fixed SST and SIC.

Other comments

L1: This opening sentence is misleading. The study demonstrates the potential of the ML technique in downscaling a global-scale mode output..

We will update the opening sentence to be more precise.

L6: You mean the model output is better than the measured PM2.5 values?

Thanks for pointing this out, the sentence was slightly vague. We'll rewrite this sentence.

L11: This is a highly impactful statement. Better to give caution to the reader by mentioning the associated large uncertainty as seen in Fig. 3(e).

Thank you for the suggestion. We will extend this sentence to also mention the uncertainty related to the ERF values estimated.

L38-40: As per the sentence, the role of ACI in aerosol indirect effects is undermined, hence please modify the sentence. Also, please explain the 'local meteorological dynamics' with references.

We will modify this sentence to as suggested.

L72-73: Cannot find in any of the given references that 'emissions from New Delhi' significantly contribute to the ATAL. Please clarify.

This statement was slightly misleading, as Fairlie et al. (2020) referred to Indian subcontinent and Northern India as significant emission sources contributing to the ATAL. We will correct the sentence to refer to the whole Indian subcontinent.

L94: HAM 'threats' the chemical compounds..?

Many thanks for pointing out this typing error. We will fix this in the manuscript text.

L158: Why do OC emissions increase by 2030 in CLE scenario while all others show a reduction?

In CLE scenario for the year 2030, the *global* OC emissions from waste sector increase approximately by 900 kt (+44 %) when compared to 2015 levels. Furthermore, emissions due to agricultural waste burning increase by ~200 kt (+10%) compared to year 2015 emissions, and the emissions from industry and shipping are also projected to increase by a small portion. These changes are almost as large as emission reductions in the domestic (~ -960 kt) and traffic (~ -230 kt) sectors. The net change in global anthropogenic OC emissions is a small decrease (~ -0.07 %) in the year 2030 compared to the year 2015.

For the *area surrounding New Delhi*, the increasing OC emissions from the waste (~ +90 kt, +62%) and industry (+11kt, +83%) sectors counterbalance some of the emission reductions cuts from the domestic (-38kt) and traffic (-18kt) sectors. Therefore, there is an increase in the net OC emissions in the area surrounding New Delhi. For BC and SO2, the reductions in other sectors (domestic and traffic for BC and energy sector for SO2) are large enough to balance out the increasing emissions from waste sector. That is why the 2030 CLE emissions for BC and SO2 are less than in 2015.

We will add a few sentences to the manuscript text to explain briefly the trends in emissions for different sectors.

L267: What is '2D yearly mean value'?

By 2D, we meant that the yearly mean values were calculated separately for each grid box. We will rewrite this in a more transparent manner.

L356: If that is the case, what is the significance of feature importance values?

The feature importance values describe which input features reduced the modeling error the most in the training phase. Therefore, the importance values reveal information about the RF algorithm priorities during the training. However, the feature importances do not necessarily describe the RF model output sensitivities to input features. We will append this sentence to the manuscript to describe this difference better.

L385: This _ _ somewhat?

We will modify this sentence to clarify the main point, I.e., that the strong negative RF_{ARI} values were not expected when all three aerosol species are reduced simultaneously.

L395-398: Confusing. The aerosol loading in MITIG_2030 is supposed to be lower than the CLE_2030, then how RF in MITIG_2030 is more negative than CLE_2030 at the Himalayan foothills. Bright background due to strong haze is expected to be more in CLE_2030.

The RF_{ARI} describes the change in the aerosol radiative effect between perturbed and reference scenarios. MITIG_2030 RF_{ARI} is more negative than CLE_2030 RF_{ARI} since there is less absorbing aerosol (BC) in MITIG_2030 than in CLE_2030. It is true that the pollution haze is expected to be stronger in CLE_2030 than in MITIG_2030. However, as we see from Figures S3b and d, the change in atmospheric absorption is significantly less in MITIG_2030 than in CLE_2030. This indicates that the absorption due to BC is dominating effect in this area, and therefore the RF_{ARI} is more negative in MITIG_2030 than in CLE_2030. We will modify the sentence slightly to describe this mechanism better.

L434-436: cannot understand. Do you mean that the CDNC burden was more in CLE_2030 scenario?

Exactly. We will rewrite the sentence to make this point clear.

L435: Expand CDNC in the manuscript.

Thanks for pointing out this deficiency. We will add a clarification of the abbreviation and will also describe the term "burden" in Section 2.6.

Conclusions: This section can be renamed as summary and conclusions by adding the significant findings of the study as bullet points.

Many thanks for the suggestion, we have renamed Section 4 and will modify the text to fit the new naming.

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