Answer to referee comment 2

October 23, 2020

Dear referee,
we are thankful for the detailed review. Our replies are below:

> ***General Comments:***
> *Even there is always a severe lack of direct flux measurement, the sporadic efforts over the past 20 years still reveals a lot of new and interesting environment dependence and inter-site variabilities of gaseous dry deposition after the proposal of the ever-popular (Hardacre et al., 2015) Wesely scheme (Wesely, 1989) and its slight variants (e.g. Wang et al., 1998). Meanwhile, enormous advance has been made over modelling carbon-water exchange, and therefore stomatal modelling. And given that dry deposition has been shown as one of the major uncertainty of modelling surface ozone (Wong et al., 2019), therefore, I largely agree with the position of the first reviewer, that the effort of updating gaseous dry deposition schemes shall be welcomed and encouraged.*

Reply: In the context of developing an atmospheric (chemical) model we chose to extend the common Wesely scheme of MESSy with well-known empirical relationships. The extension firstly captures the dependency on vegetation density, heat and drought which have been shown to be major drivers of inter-site variability's (Wong et al., 2019, Hardacre et al., 2015). Modelling the stomatal behaviour with more mechanistic models, e.g. based on carbon assimilation is a subject of future developments in MESSy. A paragraph on these future developments will be added as manuscript outlook.

> *Yet, I doubt whether this paper is doing a good enough job in "updating" the dry deposition scheme, particularly in terms of modelling canopy resistance. Given the functional diversity of plants on the Earth, I find one of the biggest weakness of the scheme presented in this paper is the lack of biome-dependence of both its stomatal*

and cuticular parameters, especially given that previous works have already addressed this issue (e.g. Emberson et al., 2013; Simpson et al., 2012; Zhang et al., 2003). There is also notable weakness in evaluation of the proposed scheme, but it is much easier to address.

Reply: We understand the reviewer doubts when comparing to the dry deposition scheme of other current models. However, the implementations of stomatal conductance dependence on vegetation density, heat and drought stress as well as cuticular uptake linked to meteorology introduce firstly important functionalities of dry deposition at vegetation to MESSy. Although the scheme is still only based on four different surface types these revision represents a significant advancement for dry deposition modelling with MESSy allowing a more realistic account of an important global ozone sink. Thereby, MESSy still lacks a detailed and mechanistic description of terrestrial vegetation that is evaluated and routinely used by the MESSy community. The documentation, evaluation and publication of the developments presented in the manuscript are important beyond the MESSy community. In fact EMAC participates to the world wide Model Intercomparison Projects (not at least CMIP6), where the full documentation of the models published is essential to understand differences among the different models. To provide a platform for this kind of model description is one of the goals of GMD. Implementing a biome-dependent dry deposition model coupled to CO2 assimilation (White et al. 2004) is planned as a follow-up development in MESSy. Biome-dependent vegetation cover information, required for this scheme, are then provided by global input data which, however, represent only the annual cycle of vegetation. The recently available dynamic vegetation model LPJ-GUESS providing detailed vegetation information with the temporal variability required for a climate model could be a further improvement. By now the one-way coupling of LPJ-GUESS as a MESSy submodel is only in the initial evaluation of the coupling with the atmospheric model (Forrest et al., 2020). A description of these future developments will be added as an outlook section to the manuscript.

Specific Comments:
Starting from stomatal conductance. I agree with the authors, that the simplicity and effectiveness of Jarvis-type parameterizations have its place in atmospheric modelling. Yet this particular ecophysiological theory itself (Jarvis, 1976) only states that stomatal conductance has multiple simultaneous constraint (mathematically, $g_s = g_{max} \prod_{i}^{n_{constraints}} f_i(X_i), 0 \leq f_i \geq 1$) but does not explicitly gives universal functional (i.e. the mathematical forms of f_i) and parameters of all biomes over the world. It has been explicitly shown that improperly parameterized Jarvis-type model can lead to substantial bias (Fares et al., 2013).

Reply: We are aware of the limitations of the Jarvis-type model but among others Fares et al. (2013) showed that the Jarvis-type model captured measured O3 dry deposition fluxes better than a Ball-Berry model based on CO2 assimilation. The criticism of the Jarvis-type model in Fares et al. (2013) concerns the missing ability of the VPD factor in representing the 'VPD driven afternoon depression'. However, we used instead of the proposed drought stress factor by Jarvis the mechanistic factor based on the optimised exchange of CO2 and water by plants (Katul et al. 2009). We will add a section on the uncertainties and limitations of the Jarvis-type model to the manuscript.

> *Earlier works of updating dry deposition schemes with Jarvis-type stomatal sub-models (e.g. Simpson et al., 2012; Zhang et al., 2003) had already been assigning stomatal parameters to each individual biome. Though one may argue that they are neither backed empirically (improperly parameterized), they are probably still working better, especially for global modelling, than one single set of stomatal parameters over all biomes. For example, Hoshika et al.(2018) empirically derive that g_{smax} (maximum stomatal conductance) can vary almost ten-folds across all biomes, and the optimal temperature of stomatal opening (T_{opt}) generally increases as the mean annual air temperature. The Zhang and EMEP parameterizations stated above a re-able to qualitatively capture some features showing in Hoshika et al. (2018) (e.g. higher g_{smax} for broadleaf trees and crops than boreal forests, higher T_{opt} for tropical than boreal biomes), giving them more creditability when applied regionally and globally, which cannot be achieved by one single set of stomatal parameters applied to all biomes over the world.*

Reply: We see the importance of the biome-dependent parameters which however can introduce uncertainties since they are assigned to measurements whereas the absolute values are influenced by multiple factors like genotype and local climatic conditions (Sulis et at., 2015; Hoshika et al., 2018, Tuovinen et al., 2009). Admittedly, detailed parameters are presented in e.g. LRTAP (2009) but for large-scale models with their limitations they have to be simplified like it is done for the EMEP model (Simpson et al. 2012). The most sensitive and uncertain parameter for dry deposition modelling at stomata g_{smax} is not used. Instead, we parametrized the background stomatal behaviour explicitly depending on the photosynthetically active radiation according to Sellers (1985). Regarding the optimal temperature of stomatal behaviour we have to consider that for the maximum and minimum temperature, which are directly related to the optimal temperature, only less measurements under field conditions are available (Hoshika et al., 2018). For these reasons among others, we decided to keep the four-type surface scheme of MESSy for dry deposition modelling in which then biome-dependent parameter sets are not included.

> *In fact, the large model-observation mismatch over ATTO (Fig.*

5), which the authors attribute to underestimated stomatal uptake (line327), may also be a product improper parameterization more than inaccurate meteorology.

Reply: Yes, the discrepancy at ATTO could be due to an improper parametrization of stomatal conductance whereas the neglected chemical within-canopy reactions, however, are also an uncertainty source (Freire et al. 2017). On the other hand the biased meteorology and moisture cycling is a well-known issue in ECHAM (Hagemann and Stacke 2015) and plays a role for dry deposition modelling here as well. In Fig. 5b of the manuscript we can show that modified meteorology and transpiration at least partly improves the modelled dry deposition velocity in the Amazon forest.

The same problem happens similarly, but to a lesser extent, for the cuticular parameterization, as Zhang et al.(2003) did assign different cuticular uptake parameters for different land types. But it is much more difficult to assess whether these parameters make sense than their stomatal counter parts. So this should be a minor issue. However, some discussions on the uncertainty and inter-biome variability of these parameters is important.

Reply: The cuticular parametrization by Zhang et al. (2002) was implemented in order to account for the second important ozone deposition pathway in our model. This pathway was effectively neglected in the previous model version. As well as for the stomatal uptake we built up on the existing resistance scheme in MESSy which distinguish between only four different surface types. Here we also used less generalised parameters. An overall consideration of the uncertainty and limitations of the used model, however, is important and will be added as a separate section to the manuscript.

Another main issue is the model evaluation, which may also stem from the fact that the proposed scheme has no biome dependence. The model evaluation over the four sites is mostly specific and well-thought. However, in most recent work involves evaluating (Silva and Heald, 2018; Wong et al., 2019), developing (Clifton et al., 2020b; Lin et al., 2019) or reviewing (Clifton et al., 2020a) dry deposition schemes, extensive effort have been done to compile world-wide ozone dry deposition measurements to gauge the performance of ozone over different biomes. Most of the above works have publicised their compiled ozone deposition measurements. Adding another part of evaluation that focus on the performance over different land types is necessary in both establishing the credibility of the proposed scheme and identifying its potential weakness, especially given this is a global model.

Reply: The whole data comparison at the four chosen sites account for the most important high vegetation covered biomes on the Earth. For the reason of uniqueness and importance to investigate atmospheric processes in a remote and pristine forest the Amazonian Tall Tower Observatory (ATTO) stands out. In order to include an analysis at this site in our study we adapt the choice of the simulation period to the availability of measurements there, specifically. The used and described measurement data listed in e.g. Clifton et al. (2020a) have been obtained in the late 2000s and early 2010s. However, the analysis period should cover the recent decade which includes most extreme drought and heat events (where the stomatal stress factors are aimed for). Moreover, since we consider the inter-annual differences at the different locations we only compare data which cover the same time period. Including further measurement sites would require a new simulations.

> *As both the vertical transfer and canopy resistance schemes are modified, the update should affect not only O3, but all trace gases. It would be interesting to include a brief description on the changes in some other important trace gases (e.g. NO_2, SO_2, HNO_3).*

Reply: The changes, indeed affect trace gases other than ozone. However, this manuscript focuses on ozone because among it's atmospheric importance the applied Wesely scheme is based on the the dry deposition mechanism of ozone (Wesely 1989). By including the changes in O_x budget, that includes NO_2 and HNO_3, we cover many important tropospheric trace gases. We will further add a figure with the changes for the fluxes of NO_2, HNO_3, HCHO and SO_2 and the respective description.

> ***Technical comments***
> *Line 106:*
> *Let's refer to Fig. 4 of Baldocchi et al.(1987). Linear scaling always produces lower resistance, and therefore higher uptake, than proper canopy scaling. Therefore linear scaling should overestimate uptake instead of underestimate.*

Reply: Indeed, the linear scaling lead to an overestimation of the uptake. Thank you for pointing to this typo.

> *Line 110:*
> *More discussions and acknowledgements on proposed (e.g. Mészáros et al., 2009; Stella et al., 2019) and implemented (e.g. Clifton et al., 2020b) soil deposition schemes are need.*

Reply: We can add discussions and acknowledgements on existing soil deposition parametrizations.

Line 192:
How is wetness and snow-covered fraction calculated? How is it related to LAI? These should be clarified.

Reply: The wet skin fraction is calculated from the wet skin reservoir (wl [m]) and Leaf Area Index (LAI [m^2/m^2]:

$$cvw \sim wl/(1 + LAI)$$

whereas the snow covered fraction depends mainly on the snow at the surface (h_s [m water equivalent]):

$$cvs \sim \tanh(h_s)\sqrt{h_s}$$

The detailed description can be found in the documentation of ECHAM5 (Klimarechenzentrum 1992 eq. 3.3.2.4; Roeckner et al., 2003 eq. 6.45)

Line 235:
There are also other important long-term measurements (e.g. Blodgett Forest, Harvard Forest). Why do you choose these particular four data sets out of all available ozone flux measurements for detailed evaluation? Additional justification is needed.

Reply: We reviewed and ask for several data sets. The chosen data sets were the best available of ozone dry deposition (flux data and ozone mixing ratio or velocity data) with the required temporal resolution and coverage which also represent different parts and biomes of the world. As examples, for Harvard forest data of O3 dry deposition flux and O3 mixing ratio is only available until 1997[1]) whereas at Blodgett forest the total measuring period (2001-2007) doesn't match the chosen simulation period. Like described above we didn't use data with non-matching time coverage since we consider inter-annual differences at the measurement sides.

Line 254:
Non-stomatal deposition does not only include cuticular, but also soil uptake. Other terminology (e.g. total cuticular conductance) shall be used in placed of non-stomatal conductance to avoid confusion and imprecision.

Reply: Yes, at the points where the uptake to the leaf surfaces is meant the term cuticular conductance should be used. However, some cited studies report measurements (partioning) of non-stomatal dry deposition which captures among others the removal at the cuticle. We will clearly distinguish this terms.

[1] data coverage of 'O3.mlb' and f.o3 is shown in plot 3 and 5 in https://harvardforest1.fas.harvard.edu/sites/harvardforest.fas.harvard.edu/files/data/plots/hf004-01.pdf

References

Forrest, Matthew, et al. "Including vegetation dynamics in an atmospheric chemistry-enabled general circulation model: linking LPJ-GUESS (v4. 0) with the EMAC modelling system (v2. 53)." Geoscientific Model Development 13.3 (2020).

Freire, L. S., et al. "Turbulent mixing and removal of ozone within an Amazon rainforest canopy." Journal of Geophysical Research: Atmospheres 122.5 (2017): 2791-2811.

Hagemann, Stefan, and Tobias Stacke. "Impact of the soil hydrology scheme on simulated soil moisture memory." Climate Dynamics 44.7-8 (2015): 1731-1750.

Hardacre, C., O. Wild, and L. Emberson. "An evaluation of ozone dry deposition in global scale chemistry climate models." Atmospheric Chemistry and Physics 15.11 (2015): 6419-6436.

LRTAP: "Mapping critical levels for vegetation, in: Manual on Methodologies and Criteria for Mapping CriticalLoads and Levels and Air Pollution Effects, Risks and Trends." Revision of 2009, edited by Mills, G., UNECE Convention on Long-range Transboundary Air Pollution. International Cooperative Programme on Effects of AirPollution on Natural Vegetation and Crops

Klimarechenzentrum, Deutsches. "The ECHAM3 atmospheric general circulation model." Techn. Rep 6 (1992).

Hoshika, Yasutomo, et al. "Global diurnal and nocturnal parameters of stomatal conductance in woody plants and major crops." Global ecology and biogeography 27.2 (2018): 257-275.

Roeckner, Erich, et al. "The atmospheric general circulation model ECHAM 5. PART I: Model description." (2003).

Sellers, Piers J. "Canopy reflectance, photosynthesis and transpiration." International journal of remote sensing 6.8 (1985): 1335-1372. Wong, Anthony Y., et al. "Importance of dry deposition parameterization choice in global simulations of surface ozone." Atmospheric Chemistry and Physics 19.22 (2019).

Sulis, M.; Langensiepen, M.; Shrestha, P.; Schickling, A.; Simmer, C. Kollet, S. J. Evaluating the Influence of Plant-Specific Physiological Parameterizations on the Partitioning of Land Surface Energy Fluxes J. Hydrometeor, American Meteorological Society, 2015, 16, 517-533

Tuovinen, Juha-Pekka, Lisa Emberson, and David Simpson. "Modelling ozone fluxes to forests for risk assessment: status and prospects." Annals of Forest Science 66.4 (2009): 1-14.

Wesely, Ml. "Parameterization of surface resistances to gaseous dry deposition in regional-scale numerical models." Atmospheric environment 23.6 (1989): 1293-1304.

Zhang, Leiming, Jeffrey R. Brook, and Robert Vet. "On ozone dry deposition—with emphasis on non-stomatal uptake and wet canopies." Atmospheric Environment 36.30 (2002): 4787-4799.
White, P. W. "IFS documentation CY23r4: Part IV physical processes." (2004).