Answer to referee comments GMD

July 2020

Comment by D. Baldocchi

Dear referee, many thanks for your review. Here are our replies:

> *The field of dry deposition has had periods of ups and downs in activity and research. Unfortunately algorithms in important models have been fossilised to consider the Wesely model of 1989. While that was a very good and appropriate algorithm 30 years ago, we know more about land surface fluxes, how to model stomatal conductance and have been datasets and parameterization information in 2020. So, I was excited to see this paper. I see the main contributions are The default dry deposition scheme has been extended with adjustment factors to predictstomatal responses to temperature and vapour pressure deficit. Furthermore, an explicit formulation of the non-stomatal deposition to the leaf surface (cuticle) dependent on humidity has been implemented based on established schemes. Finally, the soil moisture availability function for plants has been revised to be consistent with the simple hydrological model available in EMAC. The authors make a good case for this work and its significance as 'the revision of the process parameterisation as documented here has the potential to significantly reduce the overestimation of tropospheric ozone in global models'.*

Reply: The article documents a revision of the existing dry deposition scheme in EMAC not a complete new implementation. The idea is to improve the existing scheme based on the already available information in the model (i.e. without detailed phenology information etc.) because model results show that a more precise representation could lower the overestimation of ozone by models. With the current model version, these developments can only draw on limited vegetation information without details on cover and phenology. Dry deposition of trace gases is represented by the "resistance-in-series" scheme of Wesely (1989). The stomatal uptake was firstly only based on the response to incoming solar radiation developed by Sellers (1985) which is known to be an important fluctuation factor (Dawson et al. 2010), and a soil moisture stress factor. The further developments were build on this common dry deposition scheme. For the extension with additional stress factors, we adopt the multiplicative principle and the temperature stress factor by Jarvis (1976). This principle is commonly used in second-generation LSM schemes due to its computational efficiency, adaptability and simplicity (Pitman et al. 2003, Clifton et al. 2020) and has been shown to capture 95 % of the observed variability of stomatal conductance (Dawson et al 2010). The stomatal sensitivity to vapour pressure deficit is calculated according to the optimisation framework by Katul et al. (2009) which maximises the use of carbon under a minimal cost of water inside the plant. This concept accounts for the water cost of carbon without specifying the stomatal response to VPD and CO_2 in advance and agrees well with experimental data (Katul et al. 2009). Hence, by adding also the stomatal response to temperature and vapour pressure deficit within this study, the key responses of stomates are represented (Pitman et al. 2003).

> *This paper is a steps in the right direction, but revolves around the over parameterized Jarvis stomatal model that was used in the 80s with more adjustment factors. Many of us, including Piers Sellers, have abandoned the Jarvis model in land-surface modeling of water and carbon fluxes because it lead to stomatal suicide. Others have adopted the Ball-Berry approach, with better fidelity.*

Reply: Comparing to measurement data, several studies found that Jarvis-type models can compete with Ball-Berry models in explaining observed stomatal conductance and stomatal ozone flux to vegetation (Hoshika et al. 2017, Ran et al. 2017) whereas both have different limitations and advantages (Lu 2018, Farquhar et al. 1980). The performance of both models depend certainly on the choice of parameters (Sulis et al. 2015, Lu 2018). The mentioned "stomatal suicide" as major critique to the Jarvis model has been experienced in EMAC and is attributed to the lack of soil moisture storage in some regions. It is solved currently by adapting the soil moisture stress factor to the used soil representation. Moreover, the stress factor dependent on VPD (Katul et al. 2009), that we use, exerts a stronger control on evapostranspiration that the original factor proposed by Jarvis. For comparison, at VPD = 5 kPa stomatal conductance is predicted to decrease by about 50% and $< 10\%$ according to Katul et al. (2009) and Jarvis (1976), respectvely. A further amelioration of the EMAC model dry bias in the Amazon is brought by the use of VPD factor by Katul et al. (2009) only in simulations without meteorological nudging (not shown in the manuscript). The usage of the Ball-Berry approach is constrained by the availability of detailed information on plant microphysics which determine the parameters. Due to the current limitations of EMAC in this regard, described above, an implementation would build on many assumptions concerning the representation at global scale.

> *"I don't view this 'new' model as an improvement by going back to the Jarvis model for stomatal conductance. There has been many advances in stomatal modelling worth considering in 2020."*

Reply: With regard to the developments of stomatal conductance models in the last years the approach used here is dated but in EMAC this represents a significant improvement compared to the existing parametrization.The adaptability, simplicity and computer efficiency makes it attractive for the use at global scale and the usage of parametrizations for radiation response and VPD stress are different from the one used in Jarvis (1976).

> *Personally, I'd like to see some connection with ecosystem photosynthesis scaling with stomatal conductance. There has been excellent advances modeling both that could be coupled with a stomatal and dry deposition model, for instance.*

Reply: We agree with the Referee but unfortunately these developments are limited by the minimal ecosystem representation in the EMAC model. Implementing a mechanistic approach which connect stomatal conductance to plant photosynthesis is definitely intended for EMAC once a vegetation model with the sufficient details and well-constrained parameters will be available.

> *In writing the introduction, there has been some recent workshops on dry deposition, newer long term studies and a very good review that should be cited and considered.*

Reply: We will add a paragraph on the current research status of dry deposition to the introduction considering this studies .

> *I am of mixed feelings of this work. I find the model algorithm dated and not an improvement. On the other hand there has been a dearth of long term flux measurements and use of those data to test the performance of a model, as it done here. To my opinion this would be much better paper by using modern, better state of art stomatal models that couple carbon and water fluxes and test the performance against a year of flux measurements. Then I would feel the work is new, novel and a significant improvement over the past work.*

Reply: With regard to the mentioned limitations and the current status of the dry deposition parametrization in EMAC, our development can be seen as an intermediate stage on the way to a "state-of-the-art" dry deposition scheme. For the stomatal part, major dependencies to meteorology have been established whereas the implementation of the cuticular pathway contributes to a global enhancement of dry deposition especially of soluble organic species that are ozone precursors. Furthermore, the study has a significance for the MESSy community as first technical description and evaluation of the vertical exchange submodel VERTEX.

> *I also like the use of 4 contrasting flux datasets. This too is an advance in model testing. For example regarding performance, we learn 'As seen from the comparison of stomatal resistance values (Fig. 4d) the model underestimates the stomatal uptake. This*

> *is because the irrigation of the Orchard leads to cooling sustained evapotranspiration and keeps f(T) low.Thus in the model, a too high temperature stress act on the stomata'. My alternative hypothesis is that this bias may disappear with a coupled carbon-water stomatal conductance model.*

Reply: Concerning the model evaluation at Citrus Orchard, we cannot exclude that such a model might remove the bias. However, if it did, it would do it for the wrong reasons. The absence of soil water stress at Citrus Orchard (due to irrigation) is artificial and not represented in the global model. Thus, the site cannot be representative for the mostly non irrigated 1.1°x1.1° grid box including Citrus Orchard. In fact, removal of the water stress from the model greatly reduces the model bias at Citrus Orchard (see Fig. 4d).

> *If I have learned anything over my career it is the power and importance of multiple constraints. Sadly, the Jarvis model does not deliver. It was great circa 1976 and helped us think about the role of stomata on dry deposition in the 1980s, but that is its extent of being good enough.*

Reply: We are aware of the limitations of the implemented model parametrization. But regarding that the developments for a global model which has only a minimal ecosystem representation available, we see the current implementation as the best achievable in EMAC without having to embark on the coupling with a dynamic vegetation model that would provide the desired constraints.

> *Fig 3 would be better if error bars were added, given these are monthly means.*

Reply: Error bars can be added for all sub figures.

> *I do like the global upscaling. It helps address the 'so what?' question and does produce some multiple constraint with regards to getting pollution right, as we see in Fig. 6.*

Reply: Thank you for mentioning this aspect which addresses the actual motivation of this model study. EMAC is an Atmospheric Chemistry Model which explicit chemistry and misses on the other hand details for e.g. the vegetation representation.

> *My bottom line is that this paper can be remedied. It has lots of strengths worth keeping. And the spirit of the work is good.*

Reply: Regarding all the arguments mentioned above we can not be sure that implementing a simple 'Anet-g_s' stomatal approach relying on the scanty vegetation information available in the model could improve the representation of dry deposition in EMAC.

"The seasonal variability of the simulated dry deposition velocity could be further improved by using as model input the time-series of vegetation cover from an imaging products which also capture land use changes and vegetation trend that are known to impact dry deposition significantly. Connection to phenology modelling or observation is key to getting the seasonality in LAI correct and the fluxes right. So Yes this is an important aspect of the model. I'd like to see it in the 'new model'. If the model had already coupled water and carbon phenology should be part of it."

Reply: The usage of the time-series of vegetation cover from the Moderate Resolution Imaging Spectroradiometer (MODIS) is in preparation as one of the few available means to represent ecosystem phenology in the current model. However, so far only LAI data from MODIS is available in the model and remaining data like canopy height still have to be acquired. Water and carbon phenology is unfortunately not yet part of the model and will be added as part of a future planned vegetation model for EMAC.

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