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Interactive comment on "A revised dry deposition scheme for land-atmosphere exchange of trace gases in ECHAM/MESSy v2.54" *by* Tamara Emmerichs et al.

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A revised dry deposition scheme for land-atmosphere exchange of trace gases in ECHAM/MESSy v2.54 Tamara Emmerichs1, Astrid Kerkweg1, Huug Ouwersloot2, Silvano Fares3, Ivan Mammarella4, and Domenico Taraborrelli

The field of dry deposition has had periods of ups and downs in activity and research. Unfortunately algorithms in important models have been fossilized 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

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this paper.

I see the main contributions are

The default dry deposition scheme has been extended with adjustment factors to predict stomatal 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'.

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

Baldocchi, D. D., and T. Meyers (1998), On using eco-physiological, micrometeorological and biogeochemical theory to evaluate carbon dioxide, water vapor and trace gas fluxes over vegetation: a perspective, Agricultural and Forest Meteorology, 90(1-2), 1-25.

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 modeling worth considering in 2020.

Wang, Yujie, John S. Sperry, William RL Anderegg, Martin D. Venturas, and Anna T. Trugman. "A theoretical and empirical assessment of stomatal optimization modeling." New Phytologist (2020).

Medlyn, B. E., Duursma, R. A., Eamus, D., Ellsworth, D. S., Prentice, I. C., Barton, C. V., ... & Wingate, L. (2011). Reconciling the optimal and empirical approaches to modelling stomatal conductance. Global Change Biology, 17(6), 2134-2144.

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.

Jiang, C., and Y. Ryu (2016), Multi-scale evaluation of global gross primary productivity and evapotranspiration products derived from Breathing Earth System Simulator (BESS), Remote Sensing of Environment, 186, 528-547, doi:http://dx.doi.org/10.1016/j.rse.2016.08.030.

De Kauwe, Martin G., et al. "A test of an optimal stomatal conductance scheme within the CABLE land surface model." Geoscientific Model Development (2015): 431-452.

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

Clifton, O. E., Fiore, A. M., Massman, W. J., Baublitz, C. B., Coyle, M., Emberson, L., ... & Guenther, A. B. (2020). Dry deposition of ozone over land: processes, measurement, and modeling. Reviews of Geophysics, 58(1), e2019RG000670.

Clifton, O. E., A. M. Fiore, J. W. Munger, S. Malyshev, L. W. Horowitz, E. Shevliakova, F. Paulot, L. T. Murray, and K. L. Griffin (2017), Interannual variability in ozone removal by a temperate deciduous forest, Geophysical Research Letters, 44(1), 542-552, doi:10.1002/2016gl070923.

Clifton, O. E., Paulot, F., Fiore, A. M., Horowitz, L. W., Correa, G., Baublitz, C. B., ... & Hogg, A. J. (2020). Influence of dynamic ozone dry deposition on ozone pollution. Journal of Geophysical Research: Atmospheres, 125(8), e2020JD032398.

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

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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.

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.

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.

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

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.

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.

Regarding conclusion

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 modeling 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.

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