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Geoscientific Model Development Discussions

Interactive comment on "Chemistry and deposition in the Model of Atmospheric composition at Global and Regional scales using Inversion Techniques for Trace gas Emissions (MAGRITTE v1.0). Part B. Dry deposition" *by* Jean-François Müller et al.

Anonymous Referee #1

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The study claims to have developed a new dry deposition scheme for calculating dry deposition velocities (Vd) of trace gases and implemented the scheme in MAGRITTE v1.0. Such a research activity should be encouraged considering the large uncertainties between existing schemes. The authors clearly have done a large amount of work. I have several major concerns listed below for the authors to consider in the revised version of the paper.

The motivation of the study needs to be better justified. Why is a new scheme needed while there are many existing ones, most of which have gone through extensive evaluation? How much improvements will this new scheme make compared to the existing ones? The uncertainties between some excising schemes have been demonstrated, e.g., in North America (Wu et al., 2018) and Europe (Flechard et al., 2011). These studies show that some schemes may provide reasonable Vd values in some circumstances. Will the new scheme have similar uncertainties? Will it provide better Vd values (in terms of mean bias, diurnal and seasonal patters, better correlations, etc., compared to available measurements) than existing schemes? A brief comparison of model performance can be made easily by simply examining published results.

The new scheme combines various formulas from many different existing schemes and ends up to be much more complex than most existing schemes. As is known, a more complex scheme does not necessarily provide better results than simple ones due to the more needed input parameters, which will likely bring in more uncertainties to the model output (Wu et al., 2018). Besides, a more complex scheme will have less attraction to the end-users for general applications.

The effective Henry's law constant (instead of the physical ones) should be used in all the cases (the deposition to leaf surface is controlled by the thin water film on leaf cuticles) (Zhang et al., 2002). This scheme seems to only consider such an effect in some cases (page 4 line 29).

This study is aware of the different conclusions from different measurement studies regarding the VOC Vd. Thus, adjusting some parameters to get a better agreement simply based on one-single site study may not proved reasonable results elsewhere (an approach frequently used in this study). For example, Karl et al. [2010] suggested that Vd of VOCs calculated using existing schemes is about a factor of 2 lower than those based on canopy-level concentration gradient measurements over six sites covering forests and shrublands. The relative magnitudes between Vd(VOCs) and Vd(O3) in Karl et al. [2010] are actually similar to those in Zhang et al. (2003) in that Vd(VOCs) is

slightly small than Vd(O3). While the typical daytime Vd(O3) over vegetated canopies is around 1 cm s-1 in literature from numerous studies (Silva and Heald, 2018), the value in Karl et al. [2010] is much higher (e.g., up to 2.4 cm s-1 at canopy top). Thus, the very high Vd(VOCs) presented in Karl et al. [2010] was likely caused by special chemical conditions, besides uncertainties in measurements and methodology used in estimating Vd, and may not be generalized to other canopies or even to the same type of canopy but in different regions. In addition, uncertainties in field measured fluxes could be larger than theoretically constrained values if the measurement method and instrument detection limit cannot satisfy the flux measurement requirements. For example, different measurement method provide quire different flux at the same site (Wu et al., 2015). By saying this, I do not recommend constructing a Vd scheme producing such high Vd values as shown in Figure 6 for those species before mere field flux data become available to support these high Vd values.

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