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.

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.

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, $g_s = g_{max} \prod_i^{n_{constraints}} f_i(X_i)\,, 0 \leq f_i \leq 1$), but does not explicitly gives universal functional forms (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).

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 are 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. In fact, the large model-observation mismatch over ATTO (fig. 5), which the authors attribute to underestimated stomatal uptake (line 327), may also be a product improper parameterization more than inaccurate meteorology.

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.

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 worldwide ozone dry deposition measurements to gauge the performance of ozone over different biomes. Most of the above works have publicized 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.

As both the vertical transfer and canopy resistance schemes are modified, the update should affect not only O_3, 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)

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.

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.

Line 192

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

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.

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.

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