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Interactive comment on "The trend of the oxidants in boreal forest over 2007–2018: comprehensive modelling study with long-term measurements at SMEAR II, Finland" *by* Dean Chen et al.

Anonymous Referee #2

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Chen et al. presented an inter-comparison study between a 1D model framework and an observational dataset from the SMEAR II site for a decade. The research site is well documented by a number of previous publications. The attempt to compiling 10 year worth of the dataset is obviously quite ambitious thus potentially beneficial to the community. Although the motivation is sound, the scientific merit of presented discussion on the observed data and the model outcomes is not well followed up. The main reason for this in my mind is that manuscript simply contains too much of information to be considered as one paper. In this sense, if the authors' main research interests are in elucidating oxidants at the site,I would recommend to just focus on the topic. Therefore, I will present my comments on the oxidant section (3.4).

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Discussion paper

3.4.1. Ozone The discussion in the current form is not acceptable to be published. Ozone is not like other oxidants such as OH and NO3 presented in this manuscript as it is difficult to simulate using a 1D model due to its long lifetime. In this sense, I would recommend quantitatively analyze the impacts of vertical distribution of monoterpenes to ozone distributions as Kurpius and Goldstein (2003, GRL 10.1029/2002GL016785) presented.

3.4.2. OH Again the presented discussion is not quantitative enough. It comes to me a surprise the attempt to explain long term trend of OH by correlating with the CO trend. Considering the abundance of reactive BVOCs, I would not expect that CO would determine the presence of OH. This section is required to include a detailed description on chemical mechanisms to elucidate the long term trend of OH and its determining factor.

3.4.3 NO3 I would recommend more conducting more work on model evaluations. I understand there has not been any observational attempt of NO3 in the research site. However, for the model evaluation perspective, the authors can certainly use other previous community field campaign datasets open to public. Without the evaluation, it is very difficult to evaluate the validity of the discussion in this section.

In summary, I would recommend major restructuring before considering the publication of this manuscript to Atmospheric Chemistry and Physics.