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Interactive comment on "Plant surface reactions: an ozone defence mechanism impacting atmospheric chemistry" *by* W. Jud et al.

Anonymous Referee #3

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General comments

This is an interesting manuscript with some innovative measurements and analyses concerning surface ozone reactions and their potential impact on atmospheric chemistry. I support publication but since there are some significant "mechanical" issues, the manuscript should undergo some major revision before publication since it could be much improved.

Specific comments

1. Intro: Ozone forecasts for future decades are indicated as being expected to increase (19875, line 15). However, as summarized recently by the IPCC (WG 1 report, 2013), tropospheric ozone is expected to decline in the coming decades accept near

densely populated regions. Meaning, only regionally higher ozone is expected.

2. The intro is too short overall. If I recall correctly, there are numerous previous works using tobacco plants to investigate ozone damage. I see few of those cited. A quick lit search on "ozone tobacco plant" reveals over 250 entries, some of which ought to be relevant. Also, the intro is worded as if contradicting previous works is the most important result of this work. I think not, especially since the authors have not demonstrated that the mechanism they investigated is an important one throughout the plant kingdom. See below.

3. The measured ozone deposition values should be more clearly delineated from the conductance concept. The authors present results and analyses before they introduce the concept. Instead of an Appendix B, which is not referred to, the Methods section should be used (rather than section 3.5) to show how ozone flux was calculated, and adjusted (was it?). Ozone flux as plotted in the graphs is net ozone flux. Day-night differences distinguish surface-dominated losses versus total ozone losses. However, stomatal conductance and associated stomatal ozone losses are not presented although water vapor fluxes were calculated / used. For a proper comparison to previous work, stomatal conductances and photosynthesis rates need to be presented with the ozone fluxes. The current binary format of presentation in Figure 5 is useful but not quantitative. Wording such as "dramatic", and using percentages instead of multiples are symptomatic.

4. The results sections should be rearranged to present the cis-abienol tests first, then the leaf surface extract tests, then the whole plant tests. This appears more logical to me. The current order is confusing and not goal-oriented.

5. Atmospheric implications: The authors make swooping statements such as "plants will hardly lack any reactive surface compounds" and "Semi-volatile, unsaturated organic species are common on various surfaces including soil with plant litter, aerosols, sea surface layers, man-made structures and plant surfaces" without citing relevant

studies. If this is supposedly common knowledge, at least give some examples. As a result, we are made to believe that what was investigated can explain a wide variety of ozone deposition phenomena, but comparisons are missing. The Blodgett Forest studies are cited as one such phenomenon, but the authors ignored that the suggested in-canopy gas-phase chemistry of ozone at that site was corroborated by measurements and a model that consider OH radical formation as result of the ozonolysis reactions. Thus surface reactions cannot be the dominant ozone loss mechanism at that site.

I suggest that, to put the studied mechanism into perspective in terms of atmospheric implications, the authors look at measurements of nighttime ozone depletion at various sites with available data. They should consider ozone-NO and ozone-NO2 reactions, and, together with LAI data, compare ozone deposition rates to their laboratory data to judge whether the mechanism they investigated is likely to contribute widely to ozone losses from the troposphere (and in turn to SVOC emissions from the biosphere).

6. The conclusion of a “powerful ozone protection mechanism” is thus neither quantitative nor justified. It also suggests that it is a purposeful mechanism instead of an opportunistic one. However, the latter is most likely correct since elevated ozone concentrations in the troposphere are highly unlikely to have existed before the advent of increasing fossil fuel combustion after the 2nd world war. Meaning, there was neither evolutionary pressure nor time to develop an “ozone protection mechanism”. Respective wording in the manuscript should thus be altered.

Lastly, since a qualitative or quantitative comparison to real-world situations is lacking, speculation that “some of the ozonolysis-derived products may play important roles in atmospheric processes, influencing the budgets of OH radicals and ozone” should be dropped as well, or at least qualified as speculative.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 19873, 2015.