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Interactive comment on “A modified micrometeorological gradient method for estimating O_3 dry deposition over a forest canopy” by Z. Y. Wu et al.

Anonymous Referee #1

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General Comments.

The paper is a useful contribution to the issue of dry deposition over a forest. It describes a new method- the modified micrometeorological gradient method- which is in better agreement with eddy-covariance-EC- observations than the more traditional gradient methods

Specific comments

On page 782, line 15, the authors make clear that the method is still based on the flux-gradient theory. This remark is repeated at several places in the paper, as f.e on page

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785, where it is mentioned that the flux-gradient method is questionable within the canopy. The question arises how serious this is, what is the impact of this restriction. It is recommended that the authors write some sentences about this.

On page 785, line 13, the height-dependent Flux is introduced. What is the impact of this assumed height-dependency on the obtained results. Does this mean that EC-observations at the different height as they are performed now-which is 29 m, would lead to different values at f.e. 18.3 m? A similar issue arises with the remark made on page 786, line 3, where it is stated that again the constant flux approach is used. It is recommended that the authors write a short paragraph to comment on these issues.

On page 787, formula (15), u^* is introduced, without clarification. Is this the shear stress velocity at the surface, or the "effective" one at the displacement height, and how is it calculated. It is recommended that the authors clarify this issue.

Page 789, lines 18-21 it is discussed that in about 70 % of the observations counter-gradient profiles occur. No remark is made about what is happening in these cases, which phenomenon is present, and what is the impact on the fact that in only 30 % of the cases "real" dry deposition seems to occur? It is recommended that the authors write a short paragraph on this.

Page 790, line 18-25. It is mentioned that the AGM method gives much higher values than the EC-observations. Could the authors please give a possible explanation to this finding?

Technical corrections

-Page 782, line 1: field instead of filed -Page 788, line 28 states: not available whose. Should that be not available, these -Page 792, line 4 states "experiments" I would prefer to write "calculations".

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 779, 2015.

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