

## ***Interactive comment on “Contrasting effects of invasive insects and fire on ecosystem water use efficiency” by K. L. Clark et al.***

**Anonymous Referee #2**

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General comments: This manuscript aims to use the multiple-year eddy-covariance tower data at three sites in the temperate forests of New Jersey to quantify the water-use-efficiency (WUE) change before and after the disturbance (insect-induced defoliation and prescribed fire burning). The topic is within the scope of the special issue “ecosystem to climate extremes” of Biogeosciences. As a site-specific flux tower study, the authors have done a good job in explaining the technical details of many data that used in this work. Though the science part of the paper is relatively weak (see details below), considering this work also carries a function of documenting important patterns for a typical temperate forest under the situation of different disturbances, I think it is publishable in BG with some revisions.

To maintain three flux tower sites requires great amount of work and provides valuable

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data for the scientific community to use. I have many respects on this type of effort. However, I see a previous paper by the same list of authors in Agriculture and Forest meteorology in 2012, where they essentially used the same data and address a similar question related to the disturbance impacts due to fire and insects for GPP and ET. For the current manuscript, it seems to me that the authors, instead of focusing on GPP and ET previously, focus more on the ratio between the two ( $WUE=GPP/ET$ ). I am thus worried about the added value of the current manuscript compared with the previous AFM paper. It is a very important issue that the authors need to consider and address in general in the revised version.

One major thing that the authors need to clarify for the current manuscript is a figure similar as Fig 1 in their AFM paper, which clearly inform the readers what disturbance types have happened for the three different sites. In the current manuscript, it is hard to find this information in the methods section. At least for me, I have to rely on the Fig 1 in AFM paper to clearly know the disturbance history of the three sites. What strikes me is that two sites have two different disturbances within two continuous years from Fig 1 in AFM, which I don't find any such information in the current manuscript. Please add the necessary information to clarify the natural history and disturbance of the three sites during the study period.

Another major comment that I have is related to the hypothesis testing. Ideally control experiments and treatment experiments should go on parallel, and their difference provides the possibility to test the hypothesis. Here the confounding factors related to WUE change under disturbance at least include: (1) different species or types of forest; (2) different disturbances; (3) recovery length. Only using the three sites data, it is almost impossible to fix two conditions while testing WUE variations caused by the third condition. I totally understand that it is almost impossible to do this type of control/treatment experiments using flux towers (only one or two examples that I know have done this). That is being said, the authors need to reconsider their science question, as the current data may not possibly tease out different factors in the current hypothesis.

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The manuscript has quite a big redundancy of showing the similar information in three places: (1) the results section by directly citing the numbers, (2) in the tables, and (3) in the figures. I strongly recommend the authors to simplify their presentation by reducing this redundancy.

Finally, a conclusion section is strongly recommended, as the discussion is very long and a better summary of this study is needed in a concise manner.

Specific comments: 1) I suggest to use "insect-induced defoliation" instead of "defoliation" whenever possible. "Defoliation" could happen as an internal phenology rhythm of plants themselves, or be caused by disturbance. Only using "defoliation" alone causes confusions. 2) Page 9574, Line 5-9: using PAR and NEE to gap fill needs some references to support. I am not quite convinced about this gap-filling approach.

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Interactive comment on Biogeosciences Discuss., 11, 9565, 2014.

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