Biogeosciences Discuss., 11, C5244–C5256, 2014
www.biogeosciences-discuss.net/11/C5244/2014/

Biogeosciences
Discussions
Open Access

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

K. L. Clark et al.

kennethclark@fs.fed.us

Received and published: 21 September 2014

Author's Reply to Reviewers' Comments

Contrasting Effects of Invasive Insects and Fire on Ecosystem Water Use Efficiency K. L. Clark, N. S. Skowronski, M. R. Gallagher, H. Renninger, and K. V. R. Schäfer

In our reply to reviewers' comments, we first summarize the major concerns that the two reviewers have with the current version of the manuscript. We then address these point by point, and detail how we have revised our manuscript.

Summary of major concerns:

Reviewer #1 was concerned about our definition of "hydrologic fluxes" and how we

concluded that hydrologic fluxes were unaffected during disturbance and recovery. Reviewer #1 asked why we thought that nighttime NEE was reduced at the oak stand relative to the mixed and pine stands during defoliation, and about why LAI was so low at the pine stand in the winter.

Both reviewers wanted us to support our gap-filling strategy; Reviewer #1 was concerned about the use of soil vs. air temperature to gap-fill nighttime NEE values and calculate Reco, and Reviewer #2 was concerned about the use of PPFD data to gap-fill daytime values of NEE.

Reviewer #2 was concerned about the apparent overlap between the current manuscript submitted to Biogeosciences and an earlier publication in Agricultural and Forest Meteorology by our group. Reviewer #2 suggested that the presentation of when disturbances occurred was difficult to determine. Most importantly, Reviewer #2 was concerned about the experimental design, and whether we could detect differences in WUEe using three towers with different forest composition and disturbances through time. An additional concern was the apparent overlap in data presented in the text, tables and figures.

Major comments Reviewer #1

1. One of the main conclusions (p. 9584) is that the carbon dynamics are much more sensitive to these disturbances than the "hydrologic fluxes". Do the hydrologic fluxes include run-off? Or only Et and groundwater recharge? A clearer definition of what is meant by "hydrologic fluxes" would be useful. Looking at Fig. 3 the trends in GEP and Et seem very similar...it's unclear how the conclusion that hydrologic fluxes were unaffected was reached.

KC: Reviewer #1 is correct in pointing out that our use of the term "hydrologic fluxes" is ambiguous in the manuscript. We intended the term to include transpiration and evaporation which were measured using eddy covariance, but we now realize that this could also be interpreted as including groundwater recharge. It likely does not include run-off or overland flow because the topography is flat, and soil (0-20 cm depth) is approximately 94% sand and characterized by very high percolation rates at our three flux sites. Thus, run-off and overland flow are likely minimal, and our primary hydrologic fluxes are Et and groundwater recharge. We have documented these at the oak-pine stand, using a combination of eddy flux, sap flux, and USGS weir and groundwater depth data (Schaffer et al. 2013).

We have changed the text in the revised manuscript to reflect the fact that we are referring to Et in the Introduction, and have defined "hydrologic fluxes" to include ground water recharge where appropriate in the revised version of this manuscript.

We certainly agree with Reviewer #1 in concluding that Et during the summer is reduced during and immediately following disturbance, although typically not to the extent that NEE is reduced. However, we based our conclusions regarding the effects of disturbance on Et vs. carbon dynamics on a number of longer-term observations, and attempted to integrate the disturbance and recovery phases in our analyses. Much of the Introduction and Discussion sections do emphasize the recovery period following disturbance, and we have highlighted a number of specific examples throughout the manuscript, including;

1) Annual Et had recovered to pre-disturbance levels by 2009 at the oak-dominated stand, while annual NEE had not recovered by 2013 (we have added the 2013 value to the Discussion in the revised manuscript).

2) At the pine stand, we focused on Et and GEP pre- and post-disturbance. However, carbon dynamics should also include consumption losses during prescribed fires, and we did include consumption losses when estimating long-term Et vs. carbon dynamics in the Discussion section.

We will further clarify the time frame that we considered to draw this conclusion in the revised manuscript.

2. In the nighttime NEE data shown in Fig. 2 the oak forest is largely affected by the disturbance whereas the other forests show a much smaller effect. Why are these forests acting so differently at night?

KC: We believe that the difference observed in nighttime NEE among the three stands was a result of the extent of defoliation by Gypsy moth at each stand. The oak-pine stand was completely defoliated in 2007, so that from approximately June 1 to July 15th, 2007, foliar biomass in the canopy and understory, and thus respiration from these tissues, was very low. Minimal C assimilation occurred for a six-week period, and this likely limited allocation of photosynthates to the roots and rhizosphere. We observed a progressive reduction in nighttime NEE as this occurred, despite the fact that soil temperature levels were approximately 1.5 to 2 °C greater than pre-defoliation periods, while air temperature was similar pre- and during defoliation. Defoliation by Gypsy moth was less severe at the other two sites. At the mixed pine-oak stand, overstory oaks and understory vegetation were defoliated in 2007 but pines were not. At the pine-scrub oak stand, only understory oaks and shrubs were defoliated in 2007. At the latter two stands, LAI and thus C assimilation during defoliation by Gypsy moth were much higher than at the oak-pine stand.

3. p.9572, l5-10, I think Falge 2001 used T.soil to determine Reco. Why did you choose to use air temperature and how much does that choice affect the results/conclusions?

KC: We used either soil temperature or air temperature to calculate continuous Reco data for each stand, depending on the season. During the dormant season, much of the CO2 flux is a result of forest floor, soil and root respiration, and we used continuous soil temperature data to gap-fill missing nighttime NEE data, and to calculate Reco. During the growing season, foliage and other aboveground tissues are much more abundant, and contribute to nighttime NEE and Reco. We used continuous air temperature data to gap-fill missing nighttime NEE and to calculate Reco during these times.

When summed over the year, this “hybrid” approach typically results in intermediate Reco values that are between those calculated using only soil or air temperature, and all values were within 10 % of each other. For example, in 2006 at the Oak stand annual Reco calculated using only air temperature or soil temperature differed from Reco calculated using the “hybrid” approach by +5% and -8%, respectively. In 2008 at the Oak stand, Reco calculated using only air temperature or soil temperature differed by +4% and -7%, respectively. Other stands and years had similar relationships between Reco values. For example, Reco calculated using only air temperature or soil temperature at the mixed pine-oak stand in 2006 differed from the value calculated using the hybrid approach by +2% and -4%, respectively.

4. p.9567, l20-24, If Reco is relatively invariant to disturbances why does that produce large variations in NEE?

KC: NEE is the balance between photosynthesis and ecosystem respiration. Thus, the large differences in annual NEE that we and other authors have observed during and following disturbances are a result of the relatively large differences in photosynthesis (here calculated as GEP) pre- and post-disturbance, and relatively smaller changes in Reco. We agree that lines 20-24 are not as clear as they could be, and will rewrite this sentence to make the link between variation in NEE and GEP clearer.

5. There are a few other studies related to the effect of beetle mortality on forests and how this affects ecosystem fluxes that you might consider to include in the references (these are listed at the end of this review). These studies typically involve more dramatic disturbances, but perhaps add some insight.

Thank you, we will incorporate these into the revised version of this manuscript.

Minor Comments:

* why does NEE have the subscript "c"? It seems like this is not necessary.

KC: We have used the “c” in NEEc as an abbreviation for net ecosystem exchange of

CO_2 in this and previous publications. We can remove the “c” from NEE if needed, and will discuss this point with the editor.

* p. 9568, l.25, define "SD" first time it’s used. Also, sometimes "SE" is used which should also be defined.

KC: We now define SD (standard deviation) and SE (standard error) at their first use in the text, tables and figure legends.

* sect 2.1, some description of how far apart the sites are would be usefulâĂŤdo the tower footprints have any overlap?

KC: The three flux tower sites are separated by approximately 15 to 20 km, so that footprints do not overlap. We will add this information to the Methods section in the revised manuscript.

* p.5970, l.13 (and other places), for some reason people started to call this company "Li-Cor". It should be LI-COR.

KC: We have edited the Methods section in the revised version of the manuscript to include “LI-COR”

* p.9571, l.5-8 (also, p.9572, l.25), what percentage of data were gap-filled? Was it similar for all three stands?

KC: The percentage of gap filled data ranged from 44 to 52% at the oak stand, 55 to 65% at the mixed stand, and 40 to 62% at the pine scrub oak stand. We will add these details to Table 6, where we present annual NEE, GEP and Et data.

* p.9573, l.1, how big was the fetch?

KC: The fetch at all three stands was greater than 900 m, with the lowest value at the mixed stand, where a managed stand with significant thinning was located approximately 900 meters north of the tower. A low-density housing development with a partially intact forest canopy was located approximately 1500 m to the southeast of the tower in the oak-dominated stand, although wind rose analyses indicated that this was not a predominant wind direction during our study.

* p.9573, l.28, why was 10mm of precip chosen for the cut-off (this seems like a fair amount of rain).

KC: Because we wanted to produce and analyze large datasets for daily WUEe, we retained as many daily values as possible. When we analyzed daily precipitation data to exclude days where we assumed the canopy was not dry, 10 mm day-1 represented an obvious gap between dry days and those with light precipitation, and days with heavy convective precipitation, which were excluded from further analyses. Most events during the summertime were convective precipitation, and were typically brief in duration and then followed by a drying period characterized by clear sky conditions. Long-term events, such as those associated with tropical storm systems towards the end of the summer, were typically excluded from further analyses.

* p.9574, l.17-20, seems surprising that the LAI for the pine forest changed so much going from summer to winter...any explanation for this?

KC: Pitch pine retains needle cohorts for approximately 18 to 20 months. Needles from the current year cohort expand relatively late, and are not completely expanded until July 1 on most years. Needle senescence in the following year starts in late October, and by December and January, many needles from the “older” cohort have already abscised. Thus, during the winter months, only one cohort of needles is present. Nearly all of the hardwood tree species in the three upland forests are deciduous, as are the dominant shrubs and scrub oaks in the understory.

At the oak-dominated stand, scattered Shortleaf and Pitch pines account for some leaf area within the footprint of the flux tower, and occur in the tree census plots, thus LAI is > 0 m2 m-2 even during the winter months.

* p.9576, l.7 (and elsewhere)...there are references to Fig 3a, 3b, and 3c, but in Fig 3

there is no "a", "b", or "c".

We apologize for the omission. We will add “a”, “b”, and “c” to the appropriate panels on Figure 3.

* p.9584, l.3, how do you know this all goes into groundwater?

KC: We believe that run-off or overland flow at our three flux sites is minimal, because the topography is flat and soil (0-20 cm depth) is approximately 94% sand. Percolation rates are very high in these coarse-grained soils, thus our primary hydrologic fluxes are Et and groundwater recharge. We have recently documented these at the oak-pine stand, using a combination of eddy flux, sap flux, and USGS weir and groundwater depth data (Schaffer et al. 2013).

* p.9584, l.13: Does recent data from 2013 show how the recovery has progressed?

KC: NEE at the oak stand in 2013 was only -59 g C m-2. We will add this value to the Discussion section in the revised manuscript, where we report data from years following 2009. The Pine stand was burned in a second prescribed fire conducted on March 15, 2013, thus 2012 was the last “undisturbed” year at this stand. Annual NEE at this stand was -94 g C m-2 in 2013.

* p.9584, l.14, why do you call this "actual" Reco?

We will omit the term “actual” in the revised manuscript. We intended this to mean measured Reco, although this is really an estimated term.

* p.9585, l.7, change "probability" to "likelihood"

We have corrected this in the revised manuscript.

* Table 3, define the columns "df" and "F"

We have now defined these abbreviations in the revised manuscript. We also note that “degrees of freedom” is confusing in the online version of the manuscript, because commas were omitted between values. We have corrected these in the revised manuscript.

A few other papers which may be relevant:

Biederman, J. A, et al 2014: Increased evaporation following widespread tree mortality limits streamflow response. Water Resources Research, DOI: 10.1002/2013WR014994

Levy-Varon, J. H, et al. 2014: Rapid rebound of soil respiration following partial stand disturbance by tree girdling in a temperate deciduous forest, Oecologia, v174.

Moore, D.J.P, Trahan, N.A., et al 2013: Persistent reduced ecosystem respiration after insect disturbance in high elevation forests. Ecology Letters, doi:10.1111/ele.12097. (and references therein).

Major and minor comments Reviewer #2

Major comment #1: To maintain three flux tower sites requires great amount of work and provides valuable 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 pervious AFM paper. It is a very important issue that the authors need to consider and address in general in the revised version.

KC: We appreciate the complement for maintaining the flux tower sites. This was the primarily the responsibility of the lead author.

However, we believe that Reviewer #2 is mistaken in his or her assertion that the paper published in Agricultural and Forest Meteorology in 2012 contains NEE, GPP and GEP data. This paper only contains energy balance and Et data for the three stands, thus the NEE and GEP data, and the analyses of the interactions between carbon dynamics and Et are unique to the current manuscript under consideration for publication in Biogeosciences.

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's 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.

KC: We believe that Figure 1 in the current paper documenting changes in LAI and canopy Nitrogen content is actually not unlike Figure 1 in the Agricultural and Forest Meteorology paper, with the exception that the earlier pre-disturbance years are averaged together. This is designated in the Figures as Pre, D, B and Post, and defined in the legend as Pre = pre-disturbance, D = insect defoliation, B = prescribed burn, and Post = post-disturbance. Throughout the text, we have attempted to provide dates for each disturbance. We will further clarify this information in the revised version of the manuscript.

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.

KC: We fully acknowledge that our experimental design does not incorporate spatial "controls" for each stand within years, where, for example, one oak-dominated stand would be defoliated and a second oak-dominated stand would serve as an undefoliated "control" stand. However, we were careful to pose our research objectives as questions, which we believe can be addressed unambiguously using the current experimental design and appropriate time series analyses.

Our first question, "how do GEP and WUEe vary among oak and pine-dominated stands growing in the same climate and soil type vary?" during pre-disturbance periods can be addressed with the current experimental design. We were cognizant of the differences in photosynthetic capacity among the dominant overstory species when we posed this question (e.g., Renninger et al. 2013, 2014a). We also controlled for a number of important factors; stand age as reflected in the mean age of dominant overstory trees was similar among stands, understory vegetation consisted of similar species among stands (although in different proportions), and soil factors and climate were also quite similar among stands.

Our second question seems to be the major issue that Reviewer #2 is concerned about. We asked "How are LAI and canopy N content linked to GEP and WUEe during recovery from insect defoliation and prescribed fire in these stands?" To understand how GEP and WUEe varied with disturbance, we have used multi-year datasets collected at each stand, which included at least one full year of data pre-disturbance. We used the appropriate statistical test employing time series analyses with adjusted error structures, where appropriate. We were cognizant of the fact that half-hourly data violated the assumption of independence, and developed a program to randomly sub-

sample daytime or nighttime NEE data for ANOVA analyses. Daily data was tested using ANOVA analyses with the appropriate error structure to account for the lack of independence among variables. Correlation analyses were between independent and normally distributed values, although sample sizes were low. We feel that the observed patterns of NEE, GEP, Et and WUEe with disturbance were clear, and that the experimental design did not preclude the drawing of interesting conclusions, especially with regard to the linkages between the eddy covariance data and LAI and foliar N content.

While we do acknowledge Reviewer #2's concerns about the experimental design, we would also like to take the opportunity to point out that some benefits exist to the use of multi-year data at the same sites that would be difficult to achieve using other flux sites (assuming that they were in other areas). For example, climate and meteorological variables were nearly identical across stands, including precipitation amounts and timing. Cloudiness and thus integrated incident radiation was also similar among stands. As discussed above, soil factors are nearly identical among stands, down to 1 meter depth. Instrumentation and data processing were also nearly identical, and operated by the same personnel throughout the study at all three sites.

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.

KC: We were careful not to report any values in the tables that were reported in the figures, with the exception of Table 6 where we provide annual values for NEE, Reco, GEP and Et. Rather, we reserved tables for general stand descriptions (Table 1), energy balance statistics (Table 2), and tests of statistical significance and model parameters (Tables 3-5).

We do report some selected mean values in the text of the Results section that are also presented in the Figures. We do this to emphasize some important points only.

We will consider removing some of these values if they seem to result in unnecessary redundancy in the revised manuscript.

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.

KC: This is a good point and we will include a Conclusions section in the revised manuscript.

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.

KC: It is true that defoliation does occur due to phenological changes, although this would be better referred to as leaf or needle abscission. We will consult with the editor as we prepare the revised manuscript to address this concern.

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.

KC: We have added two references to the revised manuscript to support our use of half-hourly PPFD from the continuous meteorological data to gap-fill missing half-hourly NEE data during the daytime. We note that models developed to predict NEE during the daytime from PPFD data were highly significant for all three sites pre-disturbance, and r2 values ranged from 0.67 to 0.82 for the relationship between NEE and PPFD during the daytime in the summer (from Clark et al. 2010).

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