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Biogeosciences Discussions

Interactive comment on "Forest NEP is significantly driven by previous year's weather" *by* S. Zielis et al.

S. Zielis et al.

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–> We would like to thank both anonymous reviewers for their effort to improve the quality of the manuscript by providing critical thoughts and helpful suggestions. We revised the manuscript accordingly by addressing/improving as many of the critical points as possible. In the following we reply to the reviewers' criticism point by point and give information about the revisions made – replies by us are indicated by an arrow at the first paragraph of our response. We hope that the revised manuscript now fulfills the requirements of the reviewers and the journal editor.

Interactive comment on "Forest NEP is significantly driven by previous year's weather" by Zielis et al. Anonymous Referee #2 Received and published: 17 October 2013

The manuscript works on improving performances of regression models that describes inter-annual variability in net ecosystem productivity (NEP) in a coniferous forest. Models' R-square improved after variables of "previous years' weather" (i.e., winter precipitation, spring soil temperature, and autumn incoming radiation from the previous year) are swapped into the initial regression models. Authors then conclude that "previous years' weather" is important in improving predictions of NEP. Although the topic is very interesting and important, the approach of this study is quite questionable.

First, the initial regression models for predicting NEP show the p-value around 0.1 (Table 2). It means that these initial models are not good enough to be considered as a starting point, and it suggests that current-year variables included in the initial models are probably not critical enough to driver inter-annual variability in NEP. Later, the final models by swapping variables from the previous year improve their performance, but it is not necessary to mean that "previous year's weather" does the job, but it could mean that the performance of those initial models is too poor. In fact, the final models have the values of R-square between 0.24-0.53, which is still too low since we are talking about regression modeling. In my opinion, such regression models can perform much better if critical climatic drivers are identified properly. I highly recommend that authors pay more attention how to form a "good" set of starting models that are acceptable.

–> We thank the reviewer for his/her critical remarks since they seem to hint to a misunderstanding and thus to the point that our previous text was not clear enough. As already mentioned in the introduction and discussion of the earlier version, it is quite common that linear regression models a low fraction of variability in NEP is difficult (leading to only low r2 values). Also, we want to point out that our models with only current-year variables did not serve as a "starting point" to develop models with previous year's variables per se. We did not "swap" variables from the previous year into current-year models, yet rather build entirely new models, into which previous year's variables were chosen statistically when they improved the model performance. Thus, models were not forced to include variables of the previous year.

Nevertheless, we appreciate the concerns of reviewer #2 on the "good set of variables" and applied under great efforts yet another statistical method ("method 2") with additional variables (e.g. cumulative degree days, see below) to verify our initial results. Therefore, we calculated linear regressions for all possible combinations of all independent variables without and with previous year's influence (n= 30 and 60, respectively). After calculating linear regressions we accounted for interdependences and dismissed all regression models where correlation coefficients between the independent variables were r > |0.58|, similar to our initial method. Considering all possible combinations of all independent variables and their permutations, this method resulted in up to >200'000 linear models when models were restricted to up to three independent variables. These calculations were only possible using the ETH Zürich cluster computer with several days of computation time.

For comparison, we prepared two tables (see supplementary material): Table 1 consists of all models without previous year's weather computed with our initial method ("method 1") and the new method described above ("method 2a"). The same information is displayed in Table 2, however, for all models with previous year's weather included. We also considered the third comment of reviewer #2, concerning a possibly missing cumulative temperature measure over an important period (see comment below). Therefore, we calculated the cumulative temperature for thawing degree days (days with a mean air temperature > 0) for the periods winter-spring of current year and the previous year (Dec, Jan, Feb, Mar, Apr, May; "ThawDegDays.wispr" and "ThawDegDays.wispr-1") as well as for the remaining period summer-fall of current year and the previous year (Jun, Jul, Aug, Sep, Oct, Nov; "ThawDegDays.sumfa" and "ThawDegDays.sumfa-1") and included these variables into our data sets as possible drivers of annual NEP ("method 2b").We chose this assignment of the important time periods based on results of previous studies at the Davos Seehornwald. Churakova et al. 2013 previously showed the importance of spring for CO2 exchange and tree growth and Etzold et al. 2011 reported the same for the net CO2 uptake, with additional hints towards a dependence of the CO2 flux on winter temperatures and thus soil moisture

(see also comment 3 and our response). The importance of winter conditions is also evident in our models, as winter variables of both the current and the previous year are frequently present (see Table 1 and Table 2. For example, PPFD.winter, T2min.winter, and Precip.winter-1).

The results of method 2a and 2b confirmed our initial results concerning the importance of the previous year's weather for current year NEP in the Davos forest. Although method 2a and 2b yielded significant models with higher adjusted r2's than the models of our initial approach (model 1), the consideration of previous year's weather resulted always in a remarkable increase in explained variability of annual NEP (compare top panel with second and third panel of Tables 1 and 2, respectively. For example, third panel, three variables: adj. r2 increases from 0.49 to 0.63)). Also, current-year-models never reached an adj. r2 > 0.5 as expected by the reviewer #2, confirming all earlier studies published on this issue (see above).

Moreover, spring conditions are still of high importance, although somewhat different variables were chosen by methods 2a and 2b compared to our initial approach. Cumulative temperatures however did not significantly improve the model performance and were only present in one of the models as the variable with the lowest explanatory power (For example: method 2b, without previous year′s weather, ThawDegDays.wispr with beta of 0.6183 vs. beta values of 0.6415 and -0.8512).

Therefore, we adjusted the manuscript as follows: In the revised manuscript, we mention that another method was used to verify our results obtained by our initial approach, but explanations and results of this new method (method 2a) will be presented and explained in an appendix. We hope this is acceptable. The discussion and the conclusion are still based on our initial approach since we strongly believe in the concept of dismissing interdependent variables before calculating thousands of linear regression models (and discarding interdependent variables only after the fact), also supported by the similarity of the results obtained.

Second, what is "previous year's weather"? The concept need clarify and specify in Introduction. The reasons for adding more variables from the previous year are not addressed enough in Introduction. There are some in Discussions (Page 11), which is very good. They could be moved to Introduction to help clarifying why "previous years' weather" is needed, and answering why winter precipitation, spring soil temperature, and autumn incoming radiation from the previous year are chosen.

–> Thanks for this important comment. We introduced and clarified the term and gave more background information on possible ecophysiological feedbacks between previous year's weather and current year's forest ecosystem response in the introduction in the revised version. It now reads: However, forest ecosystems very likely do not only respond to immediately to actual changes in environmental conditions, but can also show delayed responses to legacy-effects or climate-vegetation feedbacks. Such delayed responses to seasonal weather conditions of the previous year (hereafter referred to as previous year′s weather) might include weather induced alterations of the built-up of stored C in the previous year's summer and/or fall used to fuel current year growth and metabolism (Carbone et al., 2013), the formation of buds in the previous year's fall and the accompanied implications for current year leaf area index (LAI), and thus GPP (Zweifel et al., 2006) as well as the compensation of respiratory C losses due to frost damages induced in winter and spring of the previous year.

Third, the conclusion "forest NEP is significantly driven by previous year's weather" is misleading. For the forest with significant amount snowpack in the winter, snow melting in the coming spring is a large water resource to tree growth and all other related ecological processes. Authors also provide ecological explanations on previous-year weather variables. Sure, influences of previous year's weather on NEP exist, but it should not as much as current year's weather. The current-year-weather models could perform much better if drivers are chosen carefully or comprehensively. Probably, some current-year-weather variables, other than spring soil temperature, winter PPFD, and winter precipitation, have not been found and included in the starting models. For

example, since the coniferous forest is sub-alpine, temperature is often the first critical driver. If so, cumulative temperature over the course of year or a critical period in growing season may be worth to be included in current-year models. In addition, winter PPFD is correlated to winter precipitation because more precipitation can means less PPFD due to more chances of cloudy days. Thus, when these two variables should not be included in the same model, the p-value is 0.1887 (Table 2). I would expect that the p-value of the current-year-weather models are <0.05 and the value of R-square >0.5 at least, as the initial models.

–> The reviewer is right pointing to the potential influence of snow melt and its related role as a water resource in the Davos Seehornwald. However, since snow starts to accumulate in winter, sits as snow pack for several months at this elevation and only melts in spring, isn′t this the first clear evidence that previous year′s weather is important for current (next) year′s soil water availability and thus growth? We might misunderstand the comment, but we strongly agree with the reviewer that winter snow conditions of the previous year are indeed important. Although we have precipitation data available, unfortunately, no data on soil water content were available for the entire 15yr period covered in this mss (1997 to 2011). Data are only available since 2006, which is too short of a time period to run the analysis presented here. In addition, we were also surprised to learn what impact previous year′s weather had on NEP, we did not expect this, again, in accordance to the reviewer.

Concerning the correlation between winter PPFD and winter precipitation: Indeed, such a correlation is possible, with less precipitation being associated with higher PPFD. The correlation coefficient between these variables at the Davos Seehornwald is $r=-0.42$, which might indicate such a relationship. However, this correlation is not strong enough to exceed the threshold of $|r|=0.58$, which was used in this study to account for interdependence between climate variables (translating to a r2 of 0.34). Moreover, the correlation coefficient only barely exceeds the lowest boundary of thresholds reported in the literature ($|r|=0.4$; cf. Suzuki et al., 2008). Thus, we believe that the presence of both variables in one model is justified from a statistical point of view. This is also confirmed by a non-significant linear regression for the two variables with r2=0.11 (p=0.123). Thus, the assumption of “winter PPFD is correlated to winter precipitation because more precipitation can mean less PPFD due to more chances of cloudy days” cannot be supported with the data at our site, although maybe valid at other (e.g. tropical) sites. In addition, PPFD and precipitation do trigger different ecophysiological processes, with different potential legacy effects, both negative (frost dryness) and positive (growth enhancement), which warrant their treatment in the same statistical model as well.

The comment concerning the performance of current-year-weather models and the possibility of missing cumulative temperature measures have been addressed above. We could increase the performance of current-year-models using a highly computation intensive method (calculation time of several days), yet, the main conclusion concerning the importance of previous year's weather for current year NEP and the importance of spring conditions remained untouched. Thus, we are confident that our models are robust, based on a comprehensive set of variables tested.

The manuscript need fix above problems before being considered for publication in BG.

–> We hope we could clarify the open points addressed be reviewer 2 and want to thank for a critical voice on the previous manuscript which made our study (i.e., the revised mss) much stronger.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C7595/2013/bgd-10-C7595-2013-supplement.pdf

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