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

Anonymous Referee #2

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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.

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.

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

Interactive comment on Biogeosciences Discuss., 10, 15587, 2013.