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Comment

Interactive comment on “Environmental change impacts on the C- and N-cycle of European forests: a model comparison study” by D. R. Cameron et al.

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We thank the reviewer for their constructive comments. In the following, we outline how the paper will be improved in response.

General comments

The paper presents a study of the application of four different dynamic models to quantify the future carbon and nitrogen balance in two forest types (pine and beech) through-

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out Europe. The models have different inputs and different outputs. It is very difficult for the reader to grasp the differences and similarities between these models. Furthermore they have different initializations and outputs from one model are used in another. Thus the complexity of the methodology is very high and the quite lengthy text does not help much. It would be of great advantage if all these model initializations, parameter inputs, and outputs could be put into a table or some other kind of diagram.

We thank the reviewer for their constructive criticism of our Methods section. We agree that the section could be written more clearly. We do want to point out that the way the models in our study were used was more standardized than the was apparent from our text. The models had many factors in common such as the weather driving data, forest and soil initialisation data which were not clear to the reviewer. However, the initialisation data were used in different ways by each modelling group. This reflects differences between models and also different choices made by each group reflecting our current uncertainty about how to initialise forest models.

Therefore we agree to shorten and clarify the model descriptions by including a table which will include information such as

- model initialisation approach
- timestep
- number of soil levels
- method of calculating GPP
- weather driving data used etc.

which will aid model comparison. The table will make clear the differences and similarities between the models.

The text in the Introduction could also be shortened.

In recent years there has been a lot of activity aiming to quantify the present and future contributions of forests to the GHG balance of Europe. Indeed there has been considerable controversy over the importance of nitrogen for this balance. Our study is one of the first in which N effects are included in a range of models, ranging from simple to complex, also considering the influence of thinning and harvesting. This is in contrast to previous studies (eg Luysaert et al. 2010, De Vries and Posch 2011) where these processes were largely missing from the modelling. Indeed in most cases our modelling results were closer to those of the observations found in Luysaert et al. 2010 than the models included in their study. We therefore think that a detailed review of previous work helps to set our work in context. This will aid the reader, unfamiliar with this work, with understanding what has been done before and how our work contributes towards this important continuing debate.

The output results of the models does not converge very well, so it is very difficult to judge what could be right or wrong. The authors are of course aware of this, e.g. p.11060, l.1: “There are some similarities . . .” and p.11079, l.22-25: “The large differences found between the models . . .”. So the question is what is the merit of this study other than demonstrating that the models disagree in very many respects?

It is true that the models in this work did give different predictions. The reviewer questions the merit of work where models differ in very many respects. However, there are currently well known large uncertainties in the GHG budgets with respect to the contribution by terrestrial vegetation. Models are crucial tools in helping to understand these large uncertainties and where the key problems lie. Indeed as well as stating differences between models we go on to make suggestions of underlying reasons for those differences based on our analysis of C and N budgets. For example, the lack of equilibrium in the initial conditions of LandscapeDNDC leading to large N-emissions in the first decade. Larger NPP in BASFOR but smaller NEE than LandscapeDNDC was shown to be due to a larger litter flux. Further analysis is in the paper. The models used in this study are widely used in many previous and current C and N modelling

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studies in Europe. These models have however generally been applied to individual forest sites rather than to the whole of Europe. Modelling studies such as ours are required to identify problems with spatial upscaling. Where large differences indicate deficiencies these need to be reported to aid model improvement. In some cases one model was clearly identified as poorer than the others; for example the accumulation of growth in DailyDayCent was unrealistically high and was pointed out in the text. Due to the length and density of our text the reviewer may have missed that we compared the results of our models with results from a number of observational studies (for example Luysaert et al. 2010, Pilegaard et al. 2006, Butterbach-Bahl et al., 2011 and others). Thus the predictions from the models have indeed been evaluated against data from the current literature.

Specific comments

p.11043, l.16: “sink” rather than “source”

This has been corrected

p.11044, l.3-4: The last sentence of the abstract is partly redundant

While the values of the average values of the N₂O source for pine and beech are stated earlier in the abstract we don't agree that it is redundant to state that:

“N₂O emissions were found to be larger from beech than pine forests and were found to be particularly sensitive to forest growth.”

/p.11045, l.11: should read: “...since it has been ...”/

This has been corrected

p.11046, l.26: N deposition is actually decreasing in some areas due to pollution abatement. However, N-availability in soils might increase due to increased mineralisation.

This useful point will be added into the Introduction.

p.11048, l.12: "... due to increased nitrogen deposition ..."

"increased" has been added to the sentence.

p.11050, l.25: The calibration procedure might not be obvious for everyone. Could you give a reference?

A reference giving our MCMC procedure will be added to the text for example van Oijen et al. 2005.

p.11053, l.27: Should it be "soil water content" rather than "drought"?

A clearer sentence has been added to the text: "The decomposition rates are controlled by temperature and precipitation minus potential evapotranspiration between May and September".

p.11058, l.13: Is "CDO" some kind of software?

CDO stands for Climate Data Operators which is now explained in the text.

p.11059, l.11: Should read: "... than for pine forests."

The missing word "for" has been added to the text

p.11062, l.14: Maybe I missed it earlier in the text, but what is the argument for an optimum temperature of 10C for beech?

The low optimum for beech of 10°C is in error and will be corrected in the next version.

p.11063, l.13: Why was Spain averaged for this model?

The INTEGRATOR modelling group only had data for the whole of Spain, not divided in provinces like in other large countries.

The sentence will be rephrased as ..are likely to be due to Spain being averaged for this model, caused by a lack of more detailed spatial data.

p.11065, l.20: Why does N deposition decrease over pine but increase over beech?

The increase and decrease of N deposition of the beech and pine forests just reflects their geographical location (see Fig. 5).

p.11071, l.19: I do not see that it is meaningful just to take the average of the output from the four models. The models gave very different output and the paper does not try to validate the models. I think it would be much better just to give the range.

We disagree as the standard deviation was given in addition to the average value which gives a measure of the variation across the models. Also as stated above, the model averages were compared against values from the literature in the Discussion section and were found to be in good agreement with observational values.

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