

Interactive comment on “The influence of soil properties and nutrients on conifer forest growth in Sweden, and the first steps in developing a nutrient availability metric” by Kevin Van Sundert et al.

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Here, we provide a first short reply to the main points raised by Referee #1. Point-by-point responses to all General, Specific and Technical Comments and changes in the manuscript will be presented in a later stage of the peer review process.

Sincerely,

Kevin Van Sundert, on behalf of all co-authors.

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REPLIES TO GENERAL COMMENTS

We thank the reviewer for his/her supportive and constructive assessment of our manuscript. We agree with most remarks made and believe incorporating the suggestions will further improve our manuscript.

One issue we would like to clarify upfront, and that may not have been sufficiently clear yet in the manuscript, is that the primary aim of the paper is not to improve predictions of forest growth (although our work may also help in that regard), but to investigate which soil properties and nutrients are the most critical determinants of conifer forest productivity. This as a first step in developing a metric of nutrient availability based on soil data. Tree productivity (normalized for climate) here serves as the most straightforward indicator of nutrient availability.

COMMENT: “The central goal of this paper is to evaluate the utility of individual and combined soil parameters for predicted forest growth. The study leverages soil and forest growth data from > 2500 pine and spruce plots spanning much of Sweden’s forest land. Soil x forest growth relations are partitioned along a N-S gradient with productivity normalized for corresponding climatic conditions. The strength of soil x production relations vary between and within regions. Soil moisture class (site wetness) is identified as another critical category that regulates both soil parameters (SOC %) and forest productivity. High SOC (wet sites) with low forest productivity (Fig 5a, 6a) stand out from N-M-S regional gradients. Owing to the strong influence of site wetness on SOC, it is advisable to stratify and analyzed soil fertility x productivity relations by wetness class, not just along N-S gradient.”

We fully agree with Referee #1’s recommendation to perform our analyses for the separate soil moisture categories (based on the observation that soil moisture is a critical factor (Figs. 5 and 6)). We therefore performed a new analysis where we distinguished the four moisture classes represented in the database. This analysis confirmed the results and parameter estimates obtained in the previous analysis (Table 2 vs the at-

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tached Table in the supplement). Hence, these results indicate that the observed patterns are very robust across the database. We will include this additional analysis in the revised manuscript.

COMMENT: “N inputs are 4-5 times higher in Southern than Northern Sweden (Binkley and Hogberg 2016) and thus present a further factor that should be considered with evaluating nutrient and forest productivity across these sites (de Vries et al. 2014).”

Referee #1 correctly states that considering variables such as N deposition may further improve growth predictions. However, exploring the influence of N deposition is outside the scope of our study. Including N deposition as an extra predictor would even complicate our understanding of the influence of soil properties, because for example pH and soil C:N ratio are gradually altered by N deposition. Nonetheless, if the editor so wishes, we are willing to test the influence of N deposition separately and present it in the revised manuscript.

COMMENT: “In addition to examining productivity and soil data from Swedish forests, the authors also evaluate the value of an existing global approach for assigning nutrient constraint metrics. The author’s general intent to validate or modify an existing approach makes good sense, but the fact that the selected approach was developed for crops not forests and is ‘yet unvalidated’ is a bit counterintuitive. Interpretations of the utility of IIASA are reliant on some level of understanding about the strength and limitations of crop-focused IIASA approach. For example, how robust is the approach for predicting plant growth across various soil types? Based on the conditions under which IIASA was developed, how well might it be expected to perform on forest soils and with tree growth? Without clear description of the IIASA (currently some found in Intro, but lacking from Abstract), the paper presents a ‘house of cards’ based on a relatively unknown, and possibly weak foundation. Finally, please justify why the simpler approach of originating with ‘no’ tool and creating one from the ground up with existing data, as outlined in Question 1, is not both adequate and preferable.”

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We used the IIASA-metric of constraints on nutrient availability because the structures of its formulas (Eqs.(6-9)) reflect general mechanisms that link soil properties to nutrient availability. Soil pH for example shows a typical optimum effect on nutrient availability, while SOC and TEB have a direct positive non-linear influence (IIASA and FAO, 2012). The final weighing of the four partial scores (Eq. (10)) finds its rationale in the idea that if a certain soil property is particularly suboptimal, it will be the most important determinant of productivity, with less influence of the other soil properties that are within the optimal range. This way of weighing can be considered as a type of interaction, but one that cannot be implemented in a simple linear regression model. Hence, our main reason for adopting the IIASA-metric as a starting point is that, in spite of its simplicity, it is based on theoretical considerations. The multiple regression equations we obtained, on the other hand, would be of less use for building a nutrient availability metric because such an entirely empirical approach would not allow for later updates of parameters or model structures based on data from other ecosystems.

We understand that the choice to start with the IIASA-metric in particular is somewhat counterintuitive, as this metric was initially developed for evaluating the soil fertility of agricultural ecosystems. Species and soil conditions of such ecosystems indeed greatly differ from the boreal forests investigated in the present study (e.g. we anticipated in advance that N availability would not yet be sufficiently explicitly implemented, given the absence of variables such as C:N). However, we argue that this metric still offers the best option to serve as a starting point, because of (i) the reasons mentioned in the previous paragraph (simplicity, inclusion of mechanisms and interactions, and potential for updates), and because (ii) it is, to our knowledge, the only attempt so far to develop a generic nutrient availability metric (except for an older productivity index presented in Riquier et al., 1970, which however only considers linear effects without interactions). In other words, we had to rely on a metric originally developed for arable land, because nutrient metrics for other ecosystem types simply do not exist. In the revised manuscript, we will further clarify the reasons to start with the IIASA metric.

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Citations:

IIASA and FAO.: Global Agro-ecological Zones (GAEZ v3.0), International Institute for Applied Systems Analysis, Laxenburg, Austria and Food and Agricultural Organization of the United Nations, Rome, Italy, 2012.

Riquier, J., Bramao, D.L., and Cornet, J.P.: A new system of soil appraisal in terms of actual and potential productivity, Food and Agriculture Organization of the United Nations, Rome, Italy, 1970.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2017-372/bg-2017-372-AC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-372>, 2017.

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