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Interactive comment on "Global distribution of hydrologic controls on forest growth" *by* Caspar T. J. Roebroek et al.

Anonymous Referee #1

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In this interesting work, the authors classify the land surface into patterns of dominant effects on forest growth by precipitation or water table depth (wtd) using satellite imagery of fPAR as a proxy for forest growth, modelled wtd, and measured and interpolated precipitation. The analysis is based on spatial correlations between long term averages of the high resolution data sets. They find that the relationship between precipitation and fPAR is prevalent, but the effects of wtd are still widespread and important. The authors also illustrate variations of the relationships as the result of local climate conditions and landscape characteristics. As the authors convincingly explain in a paragraph in the discussion, the results prove that in current modelling approaches of the land surface using exclusively precipitation as a hydrologic control on forest growth is not sufficient and the work is therefore timely and relevant. The

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Discussion paper

paper is very well written, the analysis steps are clearly explained including underlying assumptions and the results are logically structured and interpreted. I was wondering, however, why the authors used full correlations all the way through their analysis when the scope was to actually isolate the hydrological control/ contribution. As mentioned in the description of several ecohydrological classes and sometimes in the interpretation of the results, spatial covariates like temperature play a role and will explain some of the patterns of correlations found, especially those with precipitation. So, why not remove at least the contribution of spatial gradients in temperature as a known important control on forest growth by partial correlations to narrow down the contributions of the hydrological controls? I might pose a similar question regarding the relationship between precip and wtd, which might also be split more rigorously. However, the authors take this into account in the interpretation and explain well in the paper, so I do not pose this a major point of discussion. Overall, the work the authors present in their paper is scientifically interesting and relevant, methodologically mostly logical (next to the one major point stated above, I pose some minor methodological questions below that need clarification or justification in my opinion), and is presented in an excellent way regarding both text and figures. I see the need for revision and minor clarifications before publication.

Minor aspects that need clarification/ discussion and potentially changes in the manuscript:

- Consistency of the long-term averages of the data sets: As shown in table 1 of the main text, the length and the periods that they represent differ by 10 years and more between individual data sets. How might this affect the consistency of the long-term averages that are the basis of the analysis? Secondly, the data availability of at least the fPAR dataset will vary seasonally due to snow or cloud effects. Has this issue been considered and taken into account in some way in order to prevent the longterm averages to be seasonally biased?

- Is the scope to analyse hydrological control of trees or of forests? From the title I expected only forests, but basically all results are based on any pixels having a canopy height>3m independent of any definition of a forest, eg tree density. The assumption that the ‘translation from fAPAR values to photosynthetic activity are homogeneous’ (l. 91) in each moving window appears strong when only the threshold of 3m is used as a filter criterion and in reality several vegetation types might be mixed in the pixel. A slight rewording in the first and a clarification in the second case are appreciated.

- Are only those correlations displayed and evaluated that were tested as significant? Have you tried whether the results strongly change of you apply other criteria in addition, such as a (higher) correlation threshold? A threshold of 0.11 for a significant correlation for fully available spatial windows (l.101) is quite low as to have a strong meaning for the interpretation.

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