Biogeosciences Discuss., 11, C6985–C6988, 2014
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Biogeosciences
Discussions
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Interactive comment on "Long term effects on regional European boreal climate due to structural vegetation changes" *by* J. H. Rydsaa et al.

Anonymous Referee #2

Received and published: 26 November 2014

General comments: The manuscript aims to use the state-of-the-art Weather Research and Forecasting numerical meteorological model investigate the impact on regional scale surface variables and mesoscale circulation due to land cover change alone. The experiment is conducted over a sufficiently long enough period to capture considerable inter-annual variation in meteorological drivers. The subject matter is interesting and relevant to a range of researchers, and correctly identified that mesoscale effects have not to date been full addressed, comparatively to global scale or local experiments.

Despite the relevance of the work conducted here I have concerns regarding the model setup used. There are a number of details which need to be explained first.

The authors choose to use the default Noah land surface model (LSM) as opposed to the more advanced Noah-MP or Simple SIB models which are also available (and

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noted in the discussion). The Noah model has a number of, in my view, important disadvantages for this study. The most critical is that LAI in Noah is determine through an interpolation between min and max LAI parameters based on a greenness index provided to WRF as inputs from its geogrid files and it therefore insensitive to changes in meteorological drivers which would impact LAI through changes in terrestrial carbon cycle. Moreover I'm unsure how realistic it would be to use new PFT applied to the existing vegetation greenness? I also wonder how appropriate it is to use a model which is insensitive to the feedbacks it may drive in mesoscale circulation (i.e. no-carbon cycle is included).

An alternative would be to use Noah-MP which includes a carbon cycle and has the option of allowing the LSM to dynamically respond to meteorology. The authors could also then considered the magnitude of these feedbacks by running both with dynamic LAI switched on and off.

Regardless I have number of issues with the approach taken, if the objective is to know the response of the land surface in isolation then this is difficult. Under current climate we don't expect to find these ecosystem so far north therefore their impact will be difficult to interpret as the response of the simulated land surface here will not respond to climate as the real forest would, due to the lack of a C-cycle. Similarly the response of an LSM with a C-cyle may not be informative as to the response under climate change as the vegetation is being exposed to current and may respond differently. I see this as a difficult question to address in comparison to say land cover change experiments when both PFTs already exist within the same climate envelope (e.g. afforestation experiments).

If the intention is to consider structural impacts explicitly then a more logical approach (to me at least) would be to frame the research question as an impact / sensitivity analysis of changing parameters / characteristics. If this is the case then I think that the simulations conducted are sufficient but need to be discussed in the context of the structural changes rather than forest expansion specifically you have not simulated a

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forest that can respond dynamically to meteorology.

If the impact of the PFTs themselves is the targeted objective then offline runs of the LSM may be useful to show the coupled and uncoupled impacts or running WRF with boundary conditions provided by one or more climate change scenarios.

I'm quite willing to accept that I may have misunderstood the authors objectives and methods. Much of my above comment may be irrelevant depending on what further details can be provided as the the model arrangement. I think there is a lot of detail on how the land surface is parameterised and driven which is not specified (i.e. how LAI is derived in this experiment).

Specific comments:

The introduction contains insufficient review of previous works which looked at land cover change and their impact of surface meteorology. I realise that many of these studies have a focus on the terrestrial carbon cycle and land use change but many also consider impacts on surface meteorology e.g. Betts et al., 2007, Arora and Montenegro 2011. The introduction also suffers from some awkward sentences.

Page 15508, line 8: The WRF model number is given but this should be included in the methods as well.

Page 15511, lines 14 – 17. This is poorly written, in the context of the statement the author, I assume, means that complex canopy structures led to a reduction in albedo and an increase in net radiation (?), rather than "...greater affected radiation terms...". As for being closely linked to sensible heat, they are coupled but so is latent heat being that both are driven by net radiation.

Page 15511, lines 21-24. Nothing wrong with the statement but it is incomplete as both sensible and latent heat fluxes are turbulent fluxes and as the author correctly states the partitioning between sensible and latent heat will have an impact on boundary layer processes (although I realise how significantly the impact is poorly defined). But

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why is the impact on 'the overlaying atmosphere' instead of higher LAI impacting soil moisture?

Page 15512-15513, Description of WRF and Noah models, as alluded to in the general comments sections should be extended

Page 15513, lines 5-13. This information is probably better placed in a table.

Page 15514, lines 5-16. Would be good if you provided area or proportional cover estimates to place the cover changes in context.

Page 15515, lines 9-10. How short of time span?

Page 15517, lines 11-26. Some of this could be placed in the methods or is a repetition of the introduction and is not results.

Page 15518, lines 10-15. How is the increase in LAI achieved? This comes back to exactly how was the model setup. Is this purely to do with the max/min LAI parameters or has the 'greenness' index been altered etc?

Page 15525, lines 11-19. Again this relates to how was the model setup. Assuming that greenness index is used to provide the LAI estimates how have you dealt with differences in seasonality for the index? You will have applied evergreen PFTs to locations for which the index may have been deciduous.

Page 15526, line 16. So why did you not use Noah-MP?

References: Betts, R. A., Falloon, P. D., Goldewijk, K. K., and Ramankutty, N.: Biogeophysical effects of land use on climate: Model simulations of radiative forcing and large-scale temperature change, Agr. Forest Meteorol., 142, 216–233, 2007.

Arora, V. K. and Montenegro, A.: Small temperature benefits provided by realistic afforestation efforts, Nat. Geosci., 4, 514–518, 885 doi:10.1038/NGEO1182, 2011.

Interactive comment on Biogeosciences Discuss., 11, 15507, 2014.