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Interactive comment on "Towards an advanced atmospheric chemistry-enabled ESM with dynamic land surface processes: Part I - Linking LPJ-GUESS (v4.0) with EMAC modelling system (v2.53)" *by* Matthew Forrest et al.

Anonymous Referee #2

Received and published: 9 November 2018

First, apologies for this last-minute review; I appreciate that it doesn't allow much time for online discussion, but other commitments prevented an earlier response. Referee #1 has made a number of very good points and I agree with all, although I have bigger difficulties with many aspects of this paper. I here only give additional comments to those made by Ref #1.

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Major comments

1. As far as I can tell, the authors are running a non-bias corrected GCM together with a version of LPJ-GUESS that uses pre-industrial nitrogen levels, and only considers 'natural' vegetation (i.e. something is non-existent across large parts of the globe). It is mentioned that the GCM has temperature and precipitation biases (though little information is given), and of course it also has biases in other climate variables. The abstract concludes that 'initial results show that the one-way, on-line coupling from EMAC to LPJ-GUESS gives a good description of the global vegetation patterns ...'.

   If a vegetation model which predicts artificial vegetation is fed with wrong data and anyway gives a good description of global vegetation patterns, isn't something seriously wrong? Or is the comparison just not very discerning?

2. The paper states that a human land use and agricultural framework is included in LPJ-GUESS, but not enabled in this study. I cannot understand this. The authors are all from Europe and all the vegetation and land-cover they can see is affected by humans.

3. Similarly, N deposition rates are set for the decade 1850-1859, and seem to be kept constant. Given that nitrogen is a key nutrient, that values have changed enormously since the 1850s, and that LPJ-GUESS can account for this, why proceed with such an artificial assumption?

4. Much of the paper is vague about biases in EMAC and their importance. The authors explain (p5, L21) that 'it is expected that such biases will be reduced at higher spatial resolutions', but no evidence or quantification is provided. This is a serious weakness of the paper, and surprising as I most model groups know the biases of their GCMs pretty well these days.

5. On p6 we read that the simulations correspond loosely to the last couple of decades, in order to gain some insight into biases that may be present when LPJ-GUESS is forced by EMAC climate. But LPJ-GUESS runs over centuries, so how can results from a 20 year simulation (which used constant SST) give much insight into anything?

6. Actually, the LPJ-GUESS setup as given on p6, L1-5, is confusing. Here we read about a 400 year run after the vegetation has been killed off, and with nitrogen limitation accounted for. Does this mean the authors used the N-deposition of the 1850s across some period from 1600 to 2000? I think human populations have increased by more than a factor of 10 over this period, and N-deposition should reflect this to some extent.

   And which meteorology was used for the 100+400 years of simulation. Was this the constant SST, non-bias corrected EMAC, or was it CRU? When trying to interpret e.g. Fig 1 or indeed all results I really missed this information.

7. The fair evaluation of LPJ-GUESS would have been in its 'offline' mode, driven by CRU data. These results should also have been presented in Figs. 1-3, so we see how much influence 20 years of EMAC has on the simulations.

Although I appreciate that GMD is a place to report interim results, I am left with the feeling that this particular work is premature. I think the authors should run their model setup with the various anthropogenic impacts enabled (since they seem to have this capability), and they should properly account for GCM biases, before they compare with today's vegetation maps. Given that they seem to have all the model pieces in place, I cannot see why this wasn't done. And they need to compare LPJ-GUESS+EMAC with LPJ-GUESS+CRU in order to get a better sense of where discrepancies in vegetation cover and characteristics are coming from.

The whole manuscript also needs to be tightened up, with evidence offered in place of speculation. The example above, where impacts of resolution on a GCM were 'expected' is a good example. GCM modellers should know and demonstrate such results, not rest upon guesswork.

Other comments

p2, L11. It would be good to mention some of the key cycles that 'dynamic' vegetation models often lack too, e.g. not all have N-cycle, and few have P-cycles.

p2. Seems strange not to mention the EC-Earth ESM, which seems to have come much further in linking LPJ-GUESS inside an ESM model. Are there any links between the work described in this paper and the EC-Earth efforts? What are the similarities and differences in the approaches?

p3, L11 - refers to a 'companion' publication. As no real reference is given I assume they mean 'future' publication? In my experience these sometimes never appear (even if high-priority), and if one cannot already present an author list and title that can be cited I would re-phrase.

p3, L27. The phrase 'tree-individual' sounds odd and is not helpful. Re-phrase.

p4, L22. Why 'de facto'. Aren't all components of LPJ-GUESS or EMAC de facto components?!

p15, L15. Shouldn't you say 'This will extend' rather than 'This extends'? If the model is already a full ESM I don't see why you are reporting on the very limited and artificial setup you have here.

p5, L26-27. Why keep a constant CO2 when feeding a vegetation model; the seasonal cycle is well known and documented. Any why 367 ppm? Values are over 400 ppm these days.

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p6. Also, isn't LPJ-GUESS a stochastic model? If so, how was the randomness of the results accounted for?

p6. Define and give references for CRUNCEP.

p7, L8. The authors conclude that 'the simulations reproduce the global patterns of vegetation cover well'. There are several point here. Firstly, the Sahara is largely missing, and that is rather a big deal. Secondly, I guess these patterns are mainly determined by the 100+400 years of simulation, rather than the last 20 years, but as noted above I don't understand which climate driver was behind these 500 years.

p7 and elsewhere. The authors sometimes say 'conservation remapping', sometimes 'conservative remapping', and neither version is explained.

p7, L27. Here it states 'The second source of disagreement is the climate biases in the EMAC derived climate, most obviously the underestimation of tree cover ...'. Usually one evaluates climate biases with reference to a temperature data set, not by looking at tree-lines. Again, I really miss any quantification of the EMAC errors going into this simulation, and without that I have no bases to judge the impact of LPJ-GUESS coupling.

p8, L2. The authors claim that biomass isn't directly relevant for land-atmosphere exchanges, but useful for evaluating DGVM performance. Well, canopy height is mentioned, and LAI (and hence BVOC and deposition parameters) could have been, but isn't biomass also one of the key outputs of ESMs? They are supposed to account for C-sequestration, NPP, etc. It is essential that an ESM can predict these outputs very well, but here they seem to be forgotten.

p11. Canopy height is here evaluated, but N-availability is a key driver for this, and here the N-deposition component is from the 1850s.

p11, L25-29. Here again I am not sure what to make of the paper and the setup. For biomass inclusion of a land use correction makes the results worse, but the authors say

that 'this is not a major concern .. as biomass does not directly affect the atmosphere'. How can they say this? Biomass is directly linked to water flows, energy balances, LAI, BVOC emissions, deposition rates, canopy height, momentum exchange, vegetation extent and a host of related parameters. A failure to model biomass reflects a failure to model the vegetation.

p11. Again the authors suggest that LPJ-GUESS needs to be changed to perform better, but since offline simulations perform better (p7, L10), I would look to EMAC first. (I wonder if any co-authors from Lund, Potsdam or Jena would have made the same conclusions!)

p12. The text states that 'scores improve when moving to a higher resolution implies that .. leads to a tangible increase in model performance'. Again, I have trouble with the loose arguments. A change in GCM resolution will result in a change in GCM performance and biases. There is no need to 'imply'. The EMAC bias results should have been presented and analysed to establish problems with EMAC, and the offline LPJ-GUESS results should have been presented as the only true benchmark against which the linking can be assessed.

(This sentence was also rather circular by the way. Higher scores implies better performance? I though that that was definition of the score?)

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