Geosci. Model Dev. Discuss., 6, C1186–C1190, 2013
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Geoscientific Model Development Discussions Open Access

Interactive comment on "Dynamic coupling of regional atmosphere to biosphere in the new generation regional climate system model REMO-iMOVE" *by* C. Wilhelm et al.

Anonymous Referee #2

Received and published: 27 July 2013

The authors tackle a worthwhile topic-improving the land module in a regional climate model-but don't deliver on the initial promise. Part of the problem is that unless the land scheme is really horrible, you won't see dramatic results in the weather over a domain of the size being simulated here; the lateral boundary conditions exert too much control over the simulation. This is demonstrated by the fact that the precipitation evaluation does not change significantly from REMO2009 to REMO-iMOVE, and most temperature biases remain following an alteration of albedo.

Where you can see significant results following modification of the land module is in the biophysics and carbon cycle, and in this regard it is somewhat evident that the authors are out of their comfort zone with regard to research focus. We'll return to this later in

the General comments.

The paper also reads as a news article, something I frequently see in student work, and it can be summarized as follows: "I ran a model, here's what happened". I suppose that's adequate, but as a consumer of these kinds of papers what I really want is a story, an article that tells me something about how the world works that I didn't know before. We don't have that here-there's nothing new in this manuscript. For these reasons I am afraid that I must recommend that the paper be rejected for publication.

General comments: 1. Biophysics/carbon cycle

a. CO_2 simulation: The authors state, in Section 5.2, "…influence of atmospheric CO_2…so far not modeled in detail using a high resolution regional climate model." This is not true: I refer the authors to Lu et al. (2005),Wang et al. (2007), Corbin et al. (2008, 2010), Parazoo et al., 2010, as well as the forward 'priors' for any number of CO_2 inversion papers (the authors should look up P. Ciais, T. Lauvaux, P. Peylin, A. Schuh, B. Stephens, M. Uliasz among others).

b. Net Primary Production: One reference (Roy et al., 2001) is not sufficient to validate the model results. Comparison to this one paper demonstrates that the model is not wildly unreasonable, but I would have been very interested to see a regional comparison to temporally-varying metrics. Can the model reproduce annual and diurnal cycles across climatological and vegetation gradients? How does the biosphere respond to extreme events such as the 2003 drought (which was in the middle of the simulation period)? There is a wealth of observational data available with which to confront landsurface schemes. Part of the problem is the use of NPP as the metric of choice. Simulation of Net Ecosystem Exchange (NEE) would facilitate the use of data from dozens of eddy covariance flux towers within the model domain (spatial and temporal), while use of Gross Primary Production (GPP) would allow comparisons to a large body of work on both regional and global domains (global simulations can be subsampled over Europe). Even using NPP, there are multiple ground- and satellite-based estimates of NPP for this region. That the authors only selected one or two references for model evaluation demonstrates either laziness or a lack of familiarity with the topic.

2. Figures: There are too many panels, the figures are very hard to decipher. Figures with 4x6 and 5x5 panels is too busy to be useful to the reader. Figure 12 is unreadable: on the printed page it can't be seen at all, and when I expanded it on an electronic version, by the time the panels were large enough to read the text and lines were very distorted. There are options for analyzing spatiotemporal data of this kind. I might recommend Principle Component/Empirical Orthogonal Function analysis, which can bring out patterns that explain significant portions of the variability in data like this (on one or two maps, even!). PC/EOF analysis may also suggest physical underpinnings to some of the patterns seen, which would enhance the paper's readability and significance.

3. Equations: With the exception of equation 1, the justification of the equations used is extremely vague. If the equation is taken from published work, I recommend placing the citation in parentheses to the right of the equation if space allows. If the equation is not taken from cited work, then the authors must justify its use, either from first principles or empirically.

4. Vegetation type: The method used to determine PFTs from the GLC2000 data was confusing. It appears that a 2-step process was invoked using Holdridge (1964) and unspecified allocation tables. The rationale and method is poorly explained. There are many quality maps of PFT, such as Lawrence et al. (2007), why not use one of these?

Specific comments:

1. colloquialisms: the use of terms like 'nowadays' instead of 'presently' or 'till' instead of 'until' is unacceptable.

2. Spinup: In my experience, 3 years of spinup is not adequate for soil moisture/soil temperature. That the authors basically discount the first year of the simulation sup-

ports this. My experience is that soils (especially a 10m deep soil such as used here) requires 6-10 years to spin up. Also, what is the initial condition? A common IC is 95% of saturation with soil isothermal at mean annual temperature at the site; was that used here? A sentence or more describing spinup would be helpful.

3. There are several mentions of GPCC in Section 5.1.2. Do the authors mean GPCP?

4. Section 5.1.3, latent heat flux: a 50 Wm-2 difference is not very descriptive. Even if not shown, some description of differences on a diurnal basis would be helpful for understanding and interpretation.

5. Figure 9: How much difference does a change in albedo of 10% or less make? I have seen sensitivity tests using RAMS that evaluate albedo changes (tests that can be performed without changing the surface model), these are common sensitivity tests to perform. Has it been done with REMO2009?

6. Section 5.1.5: My understanding of one of the changes from REMO2009 to REMO-iMOVE is as follows: LAI decreases, resulting in a larger Bowen ratio, which decreases precipitation. What I am not clear about is whether or not this is an improvement.

7. Vegetion Ratio (VGR): section 2.3 mentions that the VGR is a function of the LAI and Beers' law, but nothing more. Does VGR correspond to the leaf-to-canopy scaling used in Sellers (1985)? I am unclear what VGR is, and its importance to the analysis.

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Interactive comment on Geosci. Model Dev. Discuss., 6, 3085, 2013.