Response to reviewer #1

The authors developed a comprehensive land model of intermediate complexity for long-term simulations and paleoclimate studies. The model descriptions provided enough details for readers to understand the model, and the overall global offline model evaluations using a variety of datasets demonstrated the adequate performance of the model. This manuscript can be accepted after addressing the following concerns.

Specific comments/suggestions:

L186: Does eq. (5) converges to eq. (4) (bare soil) when canopies (e.g., LAI+SAI approach zero) disappear? It is also unclear how the stomatal resistance rs is dependent on LAI in the model formulations Eqs. (33), (69), (70).

The dependence of r_s on LAI is implicit in the dependence of g_{can} on carbon assimilation, which depends on the absorbed photosynthetically active radiation, which in turn depends on LAI (Eq. (63)). What was not mentioned in the text was that g_{min}, the minimum canopy conductance, depends on LAI (and soil moisture). This has now been added to the text.
If LAI tends to zero then rs tends to infinity and therefore the first term on the rhs of Eq. (5) tends to zero. The canopy evaporation term (last term in Eq. (5)) also tends to zero when LAI tends to zero. The only term remaining when LAI+SAI approach zero is the term representing evaporation from the ground below the canopy (second term on the rhs). In the original formulation of Eq. (5) this term does not converge to Eq. (4) when LAI+SAI are becoming small because the undercanopy resistance $r_{a,can}$ remains finite. This has been changed by introducing a simple LAI+SAI dependence in the $r_{a,can}$ formulation (Eq. (30)):

$$r_{a,can} = \frac{L_{ai} + S_{ai}}{C_{can} u_*}.$$

There only difference that remains between Eq, (5) and Eq. (4) in the limit LAI+SAI approaching zero is the use of two different temperatures (skin and ground) in the computation of the saturated specific humidity. However, when vegetation is disappearing, the difference between the skin temperature and the top soil layer temperature becomes negligible.

L432: The effects of snow metamorphism and snow melting on snow density are neglected in the model. Is this the main reason for the model's deficiency in the snow simulation (e.g., Figs. 13-15)?

The model performance in simulating total snow mass is comparable to the performance of state-of-the-art CMIP5 models, which also tend to melt snow too late in spring compared to the GlobSnow dataset (see Figure 5 in Shi and Wang (2015) and http://www.earsel.org/SIG/Snow-Ice/files/oral_ws2014/Luojus_2014_EARSeL_CMIP5.pdf). This is now discussed in the text.

The main limitation of our model is probably that it includes a single snow layer, which makes it in general more inert to fast changes in atmospheric conditions.

L753: While the equilibrium spin-up mode is fine, the authors should at least test it against the actual spin-up for "at least 10,000 years" (say, 50,000 years). If global testing is too time-consuming, the authors could pick up a few model boxes over different climate regimes for the test.

The idea of introducing an equilibrium spin-up mode in the model was mainly to have a very efficient way to bring the model to a quasi-equilibrium state to allow fast testing and tuning of the model. For all practical applications the spin-up mode is of limited use since an additional transient model simulation is anyway required to bring the model in equilibrium with the fast seasonal processes. Also, because the model integrates one year in approximately one second, a proper transient spinup of 10,000 years can be achieved in only a few hours.
In the simulations presented in the paper we therefore slightly changed the experimental setup by removing the equilibrium spinup phase and substituting it with a 30,000 years transient spinup simulation.

L834: For biogeochemistry, the authors should compare the model LAI with MODIS LAI (e.g., the seasonal cycle over different latitudes with limited crop coverages). After all, MODIS LAI is one of the most reliable vegetation datasets.

We added a comparison of modelled LAI with MODIS LAI as suggested by the reviewer. We included a figure comparing annual maximum LAI and a figure comparing the zonal mean seasonal LAI variations for different latitudinal bands.

L845: The value of comparing potential vegetation from one model to another in Fig. 19 seems to be limited. It may be more useful to do a comparison similar to that in Zeng et al. (2008, e.g., their Fig. 10).

In addition to Figure 19 we included also a figure similar to Fig. (10) in Zeng et al. (2008) in the revised version of the paper. In the new figure modelled PFT coverage as a function of annual precipitation is compared to MODIS land cover data.

L762 (Section 8: Evaluation): The authors did an excellent job in using comprehensive datasets in model evaluations. However, most of the discussions were quite qualitative. The authors should compute some simple quantities (e.g., root mean square difference, correlation coefficient, mean bias, : : :) for some of the comparisons for two purposes: to back up the qualitative statements, and for other groups to compare their models' performance against the PALADYN v1.0 model in the future. For instance, there seems to be years with opposite anomaly signs in Fig. 18 (L840), and the authors should at least compute the simple correlation coefficient to quantify the agreement.

As suggested by the reviewer we computed root mean square differences and correlations for some of the model-data comparisons (in particular for global maps of different quantities) and reported the values directly on the figures to allow the reader to get a quick quantitative measure of model performance.
Additionally, the discussion of the evaluation results has been extended to include more quantitative analyses.

Minor comments:

L52: It is appropriate to cite Dai et al. (2013) here

It is not clear how a citation to Dai et al. (2003) would fit into the mentioned sentence. Dai et al. (2003) give an overview of the CLM land surface model, which uses the Ball model to link leaf photosynthesis and stomatal conductance. The Ball model is cited in the sentence, but CLM is just one of many models using the Ball model and we think that a citation to these is not appropriate here.

L418: Where and when does "frozen water runoff" occur?

Frozen water runoff occurs if the snow water equivalent of the snow layer exceeds 1000 kg/m2, as mentioned in the text. In practice in the model simulation for the 20th century it is limited to areas around Greenland, where annual snow accumulation exceeds snow melt.

L815: Another reason is the assumption of global uniform soil porosity in the model.

This is now mentioned in the text.

L820: The agreement of wetland areas between the model and multi-satellite data is not very good in spatial distribution (Fig. 11). Please comment.

The paragraph where wetland extent is evaluated has been extended including further discussion: *"The mean annual simulated wetland area is 3.2 mln km2 . Maximum monthly wetland extent is reasonably well captured by the model (Fig. 11). Compared to the multi-satellite product from GIEMS (Prigent et al., 2007; Papa et al., 2010) the model simulates larger wetland extent in tropical forest areas and northern peatland areas. However, if compared to other wetland products based on data other than from satellite, GIEMS is underestimating wetlands below dense forests (e.g. the Amazon forest (Melack and Hess, 2010)) and in peatland regions of northern Canada and Eastern Siberia (Stocker et al., 2014). In south-east Asia, the GIEMS wetland extent also includes extensive rice cultivation areas, which are not represented in the model. The modelled seasonal variation in global wetland area is in very good agreement with GIEMS (Fig. 12)."*

L852: Explain "flux weighted discrimation"

Has been replaced by: *"GPP-weighted isotopic discrimination during photosynthesis"*.

Table 5: Canopy diffuse snow-free albedos (0.005 for vis and 0.154 for nir) for needleleaf trees seem to be too small. In addition, does the model consider evergreen versus deciduous trees for LAImin = 1 and LAImax = 9 (broadleaf) or 7 (needleleaf)?

The diffuse canopy albedo values are taken from Houldcroft et al. (2009) and are based on MODIS data. The values seem also to be in agreement with site level observations reported by Betts and Ball (1997).
All PFT specific parameters listed in Table 5 are the same for evergreen and deciduous trees.

References:
Dai, Y., and 11 coauthors, 2003: The Common Land Model. Bull. Amer. Meteor. Soc., 84, 1013-1023.
Zeng, X. D., X. Zeng, and M. Barlage, 2008: Growing temperate shrubs over arid and semiarid regions in the Community Land Model–Dynamic Global Vegetation

References:

Shi, H. X., & Wang, C. H. (2015). Projected 21st century changes in snow water equivalent over Northern Hemisphere landmasses from the CMIP5 model ensemble. *Cryosphere*, *9*(5), 1943–1953. http://doi.org/10.5194/tc-9-1943-2015

Houldcroft, C. J., Grey, W. M. F., Barnsley, M., Taylor, C. M., Los, S. O., & North, P. R. J. (2009). New Vegetation Albedo Parameters and Global Fields of Soil Background Albedo Derived from MODIS for Use in a Climate Model. *Journal of Hydrometeorology*, *10*(1), 183–198. http://doi.org/10.1175/2008JHM1021.1

Betts, A. K., & Ball, J. H. (1997). Albedo over the boreal forest. *Journal of Geophysical Research*, *102*(D24), 28901. http://doi.org/10.1029/96JD03876