

***Interactive comment on* “The Yale Interactive terrestrial Biosphere model: description, evaluation and implementation into NASA GISS ModelE2” by X. Yue and N. Unger**

Anonymous Referee #2

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This manuscript presents a new land surface model to be coupled with the NASA Model E2. The model is a combination of previous land surface schemes and functional modules – TRIFFID, CASA, Biome-BGC, MEGAN, and the ozone damage scheme employed by Sitch et al (2007). The paper is generally well written, though often a little dense. The research represents a great deal of work, model development and evaluation are substantial research efforts, especially for such a small research team. However, this research would benefit from well developed objectives and strong arguments for the model units that were chosen to form the basis of the model.

It appears that the goal of this research was to develop a state of the art LSM that terrestrial ecosystem processes that are important for interactions with atmospheric

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chemistry, e.g. BVOC emissions, O₃ damage to plants, etc. Though this objective is never clearly stated. Without a clearly stated objective the need for another LSM cannot be justified and the choice of how to represent processes within the model are apparently ad hoc. Why not use a complete, state-of-the-art LSM that represents the most processes relevant to your purpose and then include a state of the art BVOC and O₃ model. For example, the authors acknowledge their model does not represent N and P cycling. This could have been achieved by collaborating with CABLE, CLM, or JSBACH modelling groups. Furthermore, in my limited understanding of land ecosystem – atmospheric chemistry interactions, gaseous N species are essential to many reactions and so an N cycle would be essential to accurate coupling of terrestrial ecosystems with atmospheric chemistry. Other state-of-the-art LSMs which represent an N cycle such as LM3 or O-CN could have formed the basis of this model. It is as if this model has been developed in isolation from the land surface and terrestrial biosphere modelling community, and the many advances in process representation from that community over the last decade.

The decision making process about which parts from which model to include is not at all discussed and seems ad hoc, there is no evaluation of competing alternative schemes. So much has been learnt over the past decade about LSMs and how to improve them, for example Zaehle et al (2014) is cited but no information from that detailed model evaluation is used to inform the development of this new model. The model purpose, and therefore the criteria to assess how to build the model are not apparent. This research needs, and would really benefit from a clear statement of purpose which would then provide a decision criteria for how constructing the YIBs model.

Minor comments:

The physiology references are incorrect, the Collatz et al (1991) equations for photosynthesis are presented (equations 2-4), but Farquhar et al (1980) are cited. Why cite Collatz for the Ball-Berry model of stomatal conductance? Cite Ball et al (1987). And

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why use this model, later versions are available, e.g Medlyn et al 2011.

The general definition of PFT is 'plant functional type', this should be unified through the manuscript as 'plant functional type', 'plant function type', and 'land cover type' are used interchangeably.

Figure 4 are not histograms, they are bar charts. To be histograms each x-axis should be on a single scale. For example plot 4c, intervals (from left to right) of 42, 17, 33, 50, 50, and 396 are all given the same distance on the x-axis. Also, there should be less or no space between the bars, the x-axis is a continuous variable. I suggest using box and whisker plots to represent these distributions.

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