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Geoscientific Model Development Discussions
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Interactive comment on "GOLUM-CNP v1.0: a data-driven modeling of carbon, nitrogen and phosphorus cycles in major terrestrial biomes" *by* Yilong Wang et al.

Anonymous Referee #2

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This manuscript presented a simple model for coupled terrestrial carbon-nitrogen-phosphorus cycles. A model-data fusion approach was applied to estimate steady-state values for selected model state variables, fluxes, and parameters. In particular, the estimated quantities allowed for computation of carbon, nitrogen, and phosphorus pool sizes, the openness of the N and P cycles, characteristic turnover times, and nutrient use efficiencies.

In principle, I think that estimation of these quantities using model-data fusion is a worthwhile and interesting idea. However, I think there is substantial room for improvement in both the presentation and the approaches used. Here are a few general

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comments:

1. The paper contains no statement of objectives, questions, or hypotheses. Including any or all of these features would help to orient the reader. But without them, the paper is difficult to evaluate because the goals of the paper are unstated.

2. The model is meant to be applied to natural ecosystems, and so the model-data fusion approach should be valid in grid cells that are dominated by nature ecosystems. However, the problem is that very few such grid cells remain (Ellis and Ramankutty 2008). I would therefore expect biases to emerge when the natural ecosystem model is applied to grid cells that are biogeochemically influenced by humans. I am bothered that anthropogenic effects are not accounted for in the global and (natural) biome scale averages computed by the authors.

3. In a way, not enough information is presented. I would be interested in seeing spatial maps of at least some quantities and some confirmation that the model-data fusion approach is satisfactory on the grid cell level. I am skeptical of the biome- and global-level results without having a better idea of the grid cell level information used to construct them.

4. In another way, too much information is presented, making the paper confusing (see Figs. 3, 7). It would help to have a better-defined narrative arc. While many numbers were presented, I think the authors could do a better job in drawing attention to new, qualitative insights.

5. I would like to know approximately what level of bias the steady-state assumption incurs. These ecosystems are unlikely to be at steady state for many reasons (CO_2 fertilization, changes in nutrient deposition, climate extremes such as droughts, etc.).

6. The model is simple, which is okay for a first attempt. But there does not seem to be any substantial discussion of how to make the model more realistic by including other known fluxes and feedbacks (especially appropriate in 5.3, Future research and data

needs).

7. The manuscript needs a careful proofreading by a fluent English speaker. Starting with the title, “model” would make more sense than “modeling”.

Specific comments Page 2, lines 17-21: Not clear if you are talking about N cycling, P cycling, or both Page 2, lines 28-30: Too bad that this is not discussed further Page 3, lines 17-19, state that an inorganic P pool was added to the model, and it is assumed that this P is accessible to plants. However, in reality, very little inorganic P is accessible to plants. Page 3, line 31: What do you think about N from rock weathering (Houlton et al. 2018)? Page 3, lines 37-38: N fixation is not entirely controlled by plants. In fact, the authors mentioned asymbiotic fixation a few lines above. Page 4, lines 21-22: much inorganic P is neither strongly sorbed, nor immediately available to plants. See, for example, Yang et al. (2013). Page 5, lines 2-6: why the biome-scale analysis? Soil parent material is perhaps the most important factor governing nutrient limitation (Augusto et al. 2017) and has substantial sub-biome scale variability. Page 5, lines 12-13: I am concerned at the ECMWF soil moisture product. Is it any good? Doesn't it have fixed layer depths? I'm not sure if that is appropriate here. Page 8, lines 3-4: There is not much take-away here. I guess it is not too surprising that the number falls within the large range. But what is the range of your computed number? Is the ranage reduced or increased compared to CARDAMOM? Page 8, lines 16-19: All of this seems like speculation, more appropriate for a “discussion” section than for “Results”. Page 11, lines 4-18: There are also a fair amount of “results” in the “Discussion” section. Furthermore, these are some very ad hoc ways of making corrections when unexpected results are obtained. Page 11, lines 40-41: I do not see where this was shown.

Augusto, L., D. L. Achat, M. Jonard, D. Vidal, and B. Ringeval. 2017. Soil parent materialâĂŤA major driver of plant nutrient limitations in terrestrial ecosystems. Global Change Biology.

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Ellis, E. C., and N. Ramankutty. 2008. Putting people in the map: anthropogenic biomes of the world. Frontiers in Ecology and the Environment 6:439-447.

Houlton, B., S. Morford, and R. Dahlgren. 2018. Convergent evidence for widespread rock nitrogen sources in Earth's surface environment. Science 360:58-62.

Yang, X., W. M. Post, P. E. Thornton, and A. Jain. 2013. The distribution of soil phosphorus for global biogeochemical modeling. Biogeosciences 10:2525.

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