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Interactive comment on "Global Carbon Budget 2017" *by* Corinne Le Quéré et al.

Anonymous Referee #2

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This is a more or less a standard update of the now-accepted GCP data products describing the global carbon cycle and its evolution over time. As such, there's not much to review about most of it other than clarity and completeness as this is a product with proven value and importance. I have enormous respect and admiration for this effort and the community uses this product with confidence in the care with which it is prepared.

All this of course comes before a "but" and the but in this case in GCP's decision to change the terrestrial flux, after estimating land use, to using process models instead of treating it as the residual, and then incorporating an actual separate error term, similar to the innovation in Schimel, Stephens and Fisher (2015). This is a bold move, as the land has been treated as the residual for decades and the terrestrial ecology community has long been concerned about the lack of incorporation of any knowledge of ecosystems, their response to climate and CO2, and other disturbances into the

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standard definition of the carbon cycle.

However desirable this innovation is, the approach used raises some questions. The models used represent the state of the art, but this same ensemble is well-known to diverge dramatically when run into the future, showing that the past is matched with very different sensitivities and feedback strengths. Thus, averaging these different realizations of our knowledge is, on the face of it, a reasonable thing to do, independent of any evidence that the ensemble is biased.

However, there is considerable evidence the ensemble is biased. Papers by Cox, Hoffman, Schimel and especially Lovenduski and Bonan suggest that the mean of the ensemble does not match other themselves highly uncertain estimates of climate or CO2 sensitivity, and there's very little evidence that the central tendency of the ensemble is closer to the truth that some other part of the distribution. Lovenduski and Bonan even argue that the uncertainty of terrestrial models, a state of the art set, can't even be meaningfully reduced with available data. They show that an ensemble, with an extreme weighting against observations so that only a very few models contribute still produces very divergent future carbon budgets. This implies that even the models that best fit data do so for very different reasons, and do not represent a process consensus.

The paper, fatally in my estimation, fails to provide a critical and unbiased assessment of the evidence or lack thereof for the underlying credibility of these models. Unlike ocean models, where O2 and the decadal carbon inventories, as well as a fairly strong theory in the Revelle factor, provide some sense of confidence about the basic processes in the ocean carbon cycle and its integral outcome over decades, there is not yet a similar set of observational constraints for the land. The level of observational constraint required as described in the paper is so minimal as to be almost meaningless in terms of establishing model credibility and based on model benchmarking papers allows models trivially tuned to participate.

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As a result, instead of providing a well-understood if unhappy quantity, the land as residual accumulating all the budget uncertainty, the budget now provides a quantity whose credibility is not only questionable, there is no actual basis for giving it any credence on time scales affected by feedbacks. I would not use this quantity in combination with other data, and I would not consider modeled interannual variability to be of any proven value yet. I understand the desire to incorporate knowledge about the land in parity with land use, the oceans and the atmosphere, as well as emissions, but absent a data constraint, simply using a model mean here is not the right approach.

Speaking (or writing) as a member of the carbon community, I would not use this product or support its use in assessments and policy. I think this is –in all frankness-an understandable but terrible decision and feel it undermines the credibility of the GCP data product, established over many years. I can't recommend this paper for publication prior to a far more thorough analysis of the value of the ensemble mean, which I think, if done honestly, won't support the mean sensitivity as being the most likely given data constraints. Again, I understand the desire to use terrestrial biogeochemical insight, and the limitations of data based products such as FLUXCOM, but this is the wrong decision at the wrong time for such a vitally important product.

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