Interactive comment on "Historical CO$_2$ emissions from land-use and land-cover change and their uncertainty" *by* Thomas Gasser et al.

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Comment 2.1

CO2 emissions from land use and land cover change (LULCC) are uncertain as theyare not directly observable. In the annually updated Global Carbon Budget, two methods are used to estimate the emissions: a book-keeping approach based on LULCC data and empirical response functions, and results from process-based dynamic global vegetation models. T. Gasser and co-authors present results from a model (OSCAR) which combines book-keeping approaches with process-based modelling. There are several benefits to this approach, including an additional, constrained estimate of annual CO2 emissions from the land and a method for evaluating sources of uncertainty.

C1

This manuscript describes some of these results. Overall I think this is a very strong manuscript, it is well written and forms an important contribution to the Global Carbon Project. I appreciated having an explanation of the model in the Appendix (although I have noted a few questions about this below). My primary concern is about the choice of constraint and sensitivity of model results to the choice, which I explain below. There a couple of other 'substantial' comments, and a few minor comments.

Response 2.1

We thank the referee for the detailed comments and for her support.

Comment 2.2

The constraint:

First, what is the constraint used in reducing the ensemble of simulations (as in, what is the value)? I see it is given at Line 112, but I think this is an important piece of information to include in the Methods and in Section A.5.

Response 2.2

This value is now given in section 2 (overview of the methodology) and in section A5.

Comment 2.3

The choice of constraint seems very important. It's not clear how robust the results are to the choice of constraint (which is important given how uncertain the constraint is). Although this is not discussed in the manuscript, some information about sensitivity of results to the constraint is provided within the figures and tables. From examining Table 2 – in general there is overlap between the annual and cumulative emissions when comparing the two methods of constraining OSCAR. The biggest differences are for cumulative land carbon sink (which I think makes sense given that the original constraint mostly impacted the land carbon sink in Figure 1c, indicating this is a large source of potential uncertainty), and for LASC (which has a strong dependence on the

C2

Response 2.3

Thank you for this comment. We added the following paragraph in the discussion section to discuss the sensitivity and explain our choice. We note that our choice of constraint was also motivated by simplicity, as we see this part of our work more like a proof of concept that can be refined at a later stage, within the whole GCB process.

Table 2 shows that the choice of constraint does not drastically impact our results, as there is a large overlap between the estimates obtained with both old and new constraints. More precisely, LULCC emissions do not show a large impact, whereas the land sink and the LASC do. This is however somewhat artificial, as both our constraints are aimed at constraining the processes that dictate the land sink (such as the fertilization effect), which is visible in Figure 1c where the unconstrained distribution of the land sink exhibits a large spread that is reduced after constraining. Other (or additional) constraints focused on LULCC emissions, such as constraints on carbon densities, could be envisioned – although we deemed it unnecessary for this study. Because the constraining is done after the simulations are actually run, it is indeed possible to decide ex-post on the best constraint (or combination thereof) depending on one's ultimate goal. Our choice of constraint was driven by our will to make our estimates of LULCC emissions compatible with the overall GCB2019, our scientific conviction that it is preferable to use physical (i.e. observable) variables as constraints, and our own expert judgement as to which parts of the GCB are the most reliable. Our choice can be debated, however, and we invite the community to download our raw estimates and apply their own constraints if they so wish (see data availability). Ultimately, a Bayesian synthesis framework could be used at the GCB scale (Li et al., 2016) to avoid having to make such an arbitrary choice.

Comment 2.4

C3

A related question I have (although maybe not for this paper): The uncertainty analysis revealed that biogeochemical parameters contribute to large proportion of the model uncertainty. This is attributed to the carbon densities (Lines 175-176), so it makes me think an additional constraint could be carbon stored in soils and vegetation. Have the authors considered using present-day carbon stocks as an additional constraint?

Response 2.4

It is entirely possible to add constraints to this setup. As written in the added text of response 2.3, It is even fairly easy to do so, because the constraining happens ex-post (i.e. after the simulations are done).

Two very much interlinked points to consider, however. First, using carbon stocks or carbon densities as constraints likely implies to do it at the regional scale, which means adding at least 10 constraints. Second, the more constraints are used, the more "dissolved" their effect, which can lead to relax all the constraints in the end. This explains why we were reluctant to do it in this study.

Ultimately, we decided to use our unique constraint as an example of the setup, leaving the question of which constraint or combination thereof is best open for a discussion within the GCP or possibly a follow-up study. Since we will provide our raw outputs, the referee or anyone can actually look into it. (Another constraint, along carbon stocks, could be NPP.)

Comment 2.5

Other comments:

Lines 435-438: The pre-industrial steady state carbon pools are determined using average climate variables from 1901-1930. I'm not sure how much IAV occurred during this time period, but using average climate, rather than looping through years (as in GCB), will neglect the impact of IAV on the pools. Is it possible this has an impact on the steady state pool values, and would cause spurious transient responses when

C4

Response 2.5

Thank you for this comment.

First, there may be a slight misunderstanding: the preindustrial steady-state is immediately obtained after calibration on the DGVMs (on the S0 simulation of TRENDY), i.e. we do not spin-up our model. This is now explicit in the main text, following comment 1.4. Therefore, our model simulates a deviation to that preindustrial steady-state, as discussed in (Gasser et al. 2017).

It is however true that in our transient simulations, the climate variables before 1901 (i.e. before reconstructions are available) are assumed constant and equal to the average over 1901-1920 (and not 1930; this was a mistake in our text – it is now corrected). We believe the referee's concerns are about this second aspect, as DGVMs actually recycle the first 20 years of the climate data, for this period over which we do not have data.

We made a quick test, using 2000 unweighted Monte Carlo elements, and produced the attached figure. The first line of panels shows the simulations results when using our 'flat' climate assumption or the recycled climate. The second line of panels shows the absolute difference (i.e. recycled minus flat simulations). One can see that the impact on the land sink is as expected: a cycle with >1 GtC/yr of amplitude appears before 1901. However, this virtually does not affect the LULCC emissions (which are the focus of our paper). The cumulative net change in land carbon stock is also extremely limited: it is about 1 GtC in 1901 – which is very low compared to the 40 GtC already emitted at that point – and it slowly decays to be nearly nothing after 1950. We note that this behavior is likely specific to OSCAR: it is not a process-based model, it has a yearly time-step, its sensitivities were calibrated to reproduce the effect of climate change (and not climate!), and it has a fairly linear formulation.

Therefore, we believe this confirms that our experimental setup is sound, if not exactly in line with the GCB's. And thus we will not redo our simulations. We also note that in future simulations (e.g. for future GCBs), this could easily be changed and aligned with the TRENDY exercise. Nevertheless, we added to the relevant appendix mention of this test and the fact that it does not impact our results.

Response 2.6

This is mainly caused by a lower carbon density that is itself caused by the absence of fertilisation effect (as atmospheric CO_2 remains constant to preindustrial levels in this extra simulation). This was added to the corresponding section of the main text.

Response 2.7

Done in both places.

Response 2.8

Corrected throughout section 3.3.

Response 2.9

Those definitions are introduced in the respective referenced papers: (Gasser and Ciais, 2013) and (Pongratz et al., 2014). This sentence was just to make it clear which definition the structure of OSCAR corresponds to, for anyone comparing our study and those two papers. It feels unnecessary, however, to detail the definitions and add another layer of complexity, especially as the definitions are somewhat dependent on other definitions introduced in their respective papers...

This sentence was slightly rephrased to make it clearer that the definitions are introduced in the cited papers:

This bookkeeping approach corresponds to the "definition 3" introduced by (Gasser and Ciais, 2013) and to the "definition B" of (Pongratz et al., 2014).

Alternatively, that sentence could be deleted entirely.

Response 2.10

Simply deleted.

Response 2.11

Agreed.

Response 2.12

Done.

Response 2.13

Done.

Response 2.14

They are calibrated on 1pctCO2 experiments and their variants (esmFixClim1 and esmFdbk1). Information added. Note that the details of the calibration are given in Gasser et al. (2017).

Response 2.15

The value of tau_shift is 15 years, taken as the average turnover time of shifting cultivation according to Hurtt et al. (2006). We acknowledge this is an old value that could be reevaluated.

It was indeed a typo: Eq. 19 is derived from Eq. 3, i.e. the differential equation describing the biomass growth (that is exponential in OSCAR). We added these two pieces of information to the text.

Response 2.16

The referee is right: the slash only impacts initialization, since it is then considered to be part of the litter or soil pool. We do not treat it separately. We added the following sentence:

The slash is not accounted for separately in OSCAR. Therefore, slash fluxes only appear at the initialization step, since this carbon is added to the litter and soil pools, and then follow the biogeochemistry of these pools.

Response 2.17

It is indeed missing! We added the following short paragraph at the end of section A7:

In OSCAR, the LASC (F_{LASC}) is naturally a subcomponent of the land carbon sink. It is deduced from Eq. (42) by difference to a case without transient land-cover change (i.e. with fixed preindustrial land-cover, noted $A_{\mathrm{land},0}$):

$$F_{\mathsf{LASC}} = \sum_{i,b}(npp^{i,b} - e_{fire}^{i,b} - e_{harv}^{i,b} - e_{graz}^{i,b} - rh_1^{i,b} - rh_2^{i,b})(A_{land}^{i,b} - A_{land,0}^{i,b})$$
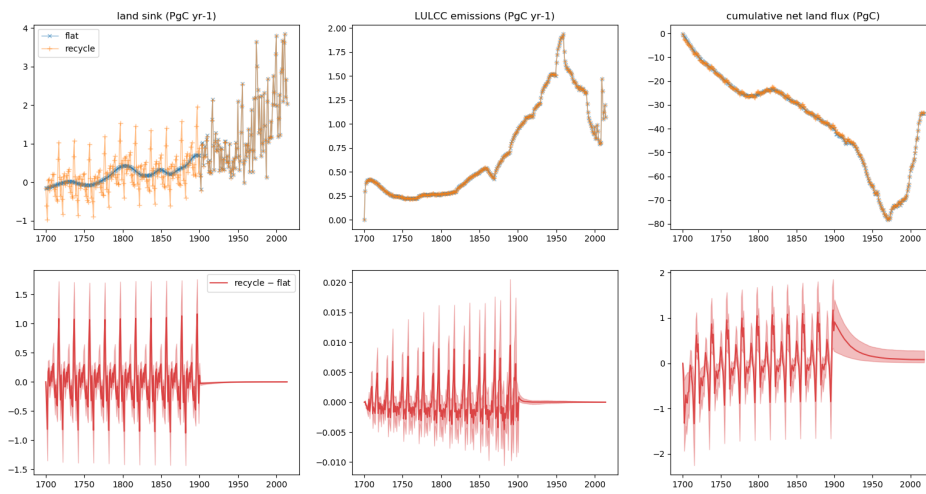
Response 2.18

Yes. Units added.

Response 2.19

Yes. Corrected.

Fig. 1. Figure for response 1.5

C11