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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Potential climate forcing of land use and land cover change" *by* D. S. Ward et al.

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REVIEWER: This paper by Ward et al. presents an assessment of the climate forcing induced by past and future land-use and land-cover change. As far as I know, this paper is the most comprehensive assessment of climate forcing from LULCC, and thus it is worth publishing. I, however, have some questions/suggestions on the scientific aspect of the work, as well as strong concerns as to how the work is presented. ï£ij RESPONSE: Thank you for your comments, which we found to be insightful and helpful for revising this manuscript. We have addressed all comments with major revisions to the manuscript including a revision of the overall organization of the text. Please see our responses to the comments below, preceded by "RESPONSE". We have included

a copy of the main text of the paper with added text, and highly revised text highlighted in red, as a supplement to this response.

REVIEWER: 1 On the methods 1.1 Overall In this paper, the authors use historical and future land-use and land-cover change data to estimate the RF induced by these activities and to compare it with the one induced by other anthropogenic activities (mainly fossil-fuel burning and industrial activities). Land-cover change, wood harvesting, agriculture and livestocks are the anthropogenic land-use-related activities considered in the study. The study then follows the "cause- effect chain" to go from anthropogenic activities to emissions of various compounds, to atmospheric burdens of GHGs and aerosols, and finally to the radiative forcing induced by these compounds.

The overall approach is scientifically sound. It appears to be a reasonable compromise between accuracy and efficiency, though it is subject to several shortcomings that, as long as they are explicitly identified and discussed, do not change the qualitative conclusions of the work. All these shortcomings and/or inconsistency have to be mentioned in the text (which is not always the case), maybe even in a specific sub-section of the methods section.

RESPONSE: We address this comment specifically in our response to comment 1.2.2.

REVIEWER: 1.2 More specific comments 1.2.1 This paper is an attribution exercise, and I think it should clearly be presented as this by using the word "attribution" more often, especially in the abstract, introduction and conclusion.

RESPONSE: We made an effort to use the word attribution more often in this manuscript and found several instances, particularly in the introduction, where using this terminology should be much more effective.

REVIEWER: To this attribution exercise is associated an attribution method called the residual method, where the contribution of LULCC is deduced by substraction of two model realizations: one with all drivers (LULCC + non-LULCC) and one without LULCC (non- LULCC alone). This approach is carried out along the whole study, hence the authors implicitly define the contribution of LULCC as the difference between these two realizations (which raises some issues as to non-linearity, but this is not the point here).

This is at least what I understood. But it does not appear clearly in the text: there should be somewhere (introduction, methods section) a clear statement about this attribution method.

RESPONSE: We agree that this basic approach (explained nicely here by the reviewer) was not explicit in the MS text and should be prominently explained. We add the following text to the "Overview of methods" section (first paragraph in Sect. 2) to explain this approach:

"For several forcing agents, including CO2, we isolate the LULCC emissions by comparing global transient simulations of the terrestrial biosphere with LULCC to simulations without LULCC that are otherwise identical, and attribute the difference in emissions between the LULCC and no-LULCC simulations to LULCC. This general approach, attributing the differences between the LULCC and no-LULCC environment to the impacts of LULCC, also applies to our calculations of RFs."

REVIEWER: Along with this comment, I am very very uncomfortable with the vocabulary used to discuss this attribution. Especially surrounding table 2, the authors usually refer to the contribution of LULCC as a "change", or they write that LULCC "increased" or "decreased" emissions of some compound in a given year. This is disturbing. All this should be rewritten, using a more dedicated vocabulary like: "contributions from LULCC", "attributed share", "LULCC-induced emissions". Words like "change", "increase" or "decrease" are misleading when comparing two realizations, i.e. two hypothetical worlds, and they should be reserved for temporal dynamics.

RESPONSE: We have corrected all instances of this kind of language usage as suggested by the reviewer. The majority of the changes were needed in Section 2, often when referencing Table 2 as pointed out. There were several instances where we use

similar language but to refer to changes in atmospheric concentrations of species or changes with emissions over time and in these cases we kept the original language.

REVIEWER: 1.2.2 There is an opposition (and somehow an inconsistency) between using a complex land model to deduce emissions of some coumpounds (CO2, Fire, SOA, Dust), and a simple rescaling for others. I am not asking to redo all the work, but the inconsistency and missing processes should be clearly acknowledged. For instance, a process-based representation of wetlands (both anthropogenic and natural) could significantly affect the quantitative conclusions of this study about CH4. Indeed, drying of wetlands in the past has likely decreased CH4 emissions from wet- lands. Also, if rice paddies were taken over natural wetlands, only the difference in CH4 emissions is attributable to the land-use activity (it is not done this way in the RCP). However, little is known about preindustrial wetlands extent, and given it is also affected by climate, it is understandable that it was not included in the study.

RESPONSE: To address this comment we have added text and moved some existing text into two new paragraphs with the aim of being more comprehensive and organized about communicating the shortcomings and missing processes in this study. We found that we did include some of these inconsistencies in the original MS but they were often scattered throughout the text and not prominently placed. Most of these are now listed in the same place and combined with additional missing processes into the last paragraph of new Sect. 2.1 (LULCC activities):

"While we consider this list of activities to be highly inclusive, several LULCC activities and processes are not included in this study, either because they are difficult to properly model or represent as a forcing, or because of a poor level of current understanding of the process. We exclude the impacts of anthropogenic water use, mainly irrigation, on global water vapor concentrations and the associated RF (Boucher et al., 2004). Changes in water use and land use have numerous other implications for the hydrological cycle including impacts on evapotranspiration, runoff, and wetland extent (Sterling et al., 2013). Related to these effects, the impact of land surface albedo changes may be further moderated by changes in cloudiness (Lawrence and Chase, 2010), which we did not consider in this analysis. Also, emissions of CH4 are tied to the global extent of wetlands, which have likely changed since preindustrial times (Lehner and Doll, 2004), but the scale and distribution of the change is not yet known well enough to be included in our model setup. We assume that natural CH4 emissions remain unchanged from 1850 through 2100 for all scenarios. Finally, there is a potentially major source of CO2 from deforestation and forest degradation in tropical peat swamp forests that has only recently been recognized (Hergoualc'h and Verchot, 2011), although it is thought that contributions from this source to current global CO2 concentrations are small (Frolking et al, 2011)."

We are explicit about the lack of understanding of natural N2O emissions (new Sect. 2.3.2) and kept this in the original location in the text. The second paragraph focuses on the shortcomings of the methodology we use to calculate the RFs. This is the last paragraph in new Sect. 2.4 (RF calculations), and part of this new text that is relevant to this comment is given here:

"For the calculation of the many forcing agents that we do consider, our approach is to treat each forcing separately, which could lead to differences in RFs between agents that are due partly to methodology. For example, land cover changes and agricultural emissions were developed jointly for each of the RCPs, but for use in terrestrial models, including CLM, the land cover change projections were altered (Di Vittorio et al., 2014). This leads to inconsistent storylines between future emissions computed by CLM (Sect. 2.2) and those taken directly from the RCP integrated assessment model output (Sect. 2.3.1). Therefore, it is important to view the future RFs computed here as comprising a broad range in possible outcomes, extended with the TEC, as opposed to precise results corresponding to specific storylines for the future."

We now suggest in the results section (new Sect. 3.1) that since our total anthropogenic RFs match previous estimates, methodology is robust even though there is no single method for computing the RFs of the many forcing agents. The aerosol forcings

of course are highly model-dependent and that is why we make an exception for these and use the IPCC AR5 values instead.

REVIEWER: Given that CLM includes an explicit N-cycle, I wonder why land-related N-fluxes (i.e. NOx, NH3, N2O) were not taken from the model, while the C-fluxes were. Again, it is alright if the authors are not confident enough to take the fluxes from their model, but it should be clearly stated.

RESPONSE: This is a great question and the answer is, unfortunately, that these emissions are simply not simulated by CLM yet. We now note this in the first sentence of new Sect. 2.3.1 (Agricultural emissions):

"Agricultural emissions of important trace gas species, such as NH3 and N2O, are not simulated by CLM. Therefore, additional emissions from LULCC activities associated with agriculture were taken from the integrated assessment model emissions for the different RCPs (e.g. van Vuuren et al., 2011). "

REVIEWER: It is unclear if changes in biogenic NMHCs emission induced by LCC (not by deforestation fires, but by changes in PFT fractions and LAI) are also added to anthropogenic emissions from RCP in the calculation of tropospheric O3 and change in CH4 lifetime. According to figure 2 it is not, but it could/should be.

RESPONSE: Thank you for pointing this out - changes in these emissions attributed to LULCC are included in the CAM-chem simulations and we added an arrow to this figure to note that non-fire changes in NMHCs are also coming from CLM.

REVIEWER: 1.2.3 This study obviously took some time, and it looks like it begun a while ago, thus results are compared to the AR4. It seems to me that updating the paper with the AR5 would not be too difficult, as it does not require further simulations. Also, the AR5 reference year is 2011, which is closer to the year 2010 used in this study. This would be needed when comparison of results is done (figure 5), and for the rescaling of aerosols effects (which are not so much separated into direct and indirect in the AR5).

RESPONSE: This is an excellent suggestion and we have done this. Using AR5 did not require any further simulations but it did require redoing most of the offline calculations since we scale our aerosol RFs to the IPCC central estimates (which are quite different in AR5 compared to AR4). Using AR5 makes this scaling of the aerosol RFs more justifiable as they now report effective RFs, which are what we calculate in this study. The text, tables and figures have been updated to reflect this change.

The change in total anthropogenic aerosol RFs (now scaled to AR5) did not change the LULCC RF from these forcings in a substantial way since they were small to begin with. It does, however, change the uncertainties and perhaps the biggest change is in the proportion of the total anthropogenic RF attributed to LULCC. Since the cooling aerosol RFs are now smaller in magnitude, the contribution to positive RF from fossil fuel burning is larger. The new LULCC portion of total anthropogenic RF is 40% +/- 15%.

REVIEWER: By the way, if such an update is feasible in a reasonable amount of time, I also suggest to change the IRF used for CO2 with the one used in the AR5, chapter 8

RESPONSE: This is also a good suggestion and we were aware of the Joos et al. (2013) work and made a decision to use the older IRF from Enting et al. because they include a scenario where CO2 concentrations are increasing, instead of only scenarios of either preindustrial or present day CO2 concentrations. We consider the older IRF to work better for understanding the airborne fraction of CO2 emitted into an transient atmosphere with respect to CO2 concentrations, such as the historical period and most future scenarios.

REVIEWER: 1.2.4 The way CO2 emissions from LULCC are estimated puzzles me (section 3.2.5). I do not think there should be any downward adjustment! The debate as to what should be included in the CO2 LULCC flux is still open, and very unlikely to be settled any time soon (see doi:10.5194/esd-4-171-2013 and doi:10.5194/esd-5-

177-2014). I'd rather see the authors of this paper choose an attribution method (see above) and stick to it, and not try to correct some biases that only exist for a specific definition of "emissions from land-use change".

RESPONSE: This aspect of our study has now been clarified with references to the categories defined by Pongratz et al. (2014). The changes to the text are in new Sect. 2.2.4 (Co2 emissions). Our approach to computing the emissions with un-coupled terrestrial model simulations puts our net LULCC carbon flux into the “D3” category as defined by Pongratz et al. (2014), meaning we are missing the CO2-fertilization feedback. If we were not interested in attributing changes in atmospheric CO2 concentration to LULCC then we could report the emissions without adjustment from our model and it could be compared to previous D3 studies. However, to get the concentration change right we need to account for all carbon sinks associated with LULCC and therefore need to adjust for the CO2-fertilization feedback.

We have also slightly modified our approach to accounting for this feedback. Previously we reduced carbon emissions from LULCC by a constant PgC amount per year. In light of recent papers we find that it is more appropriate to reduce the carbon emissions by a percentage of the emissions, not a constant PgC amount. We use the same method as in the original MS to arrive at this percentage – 20%. The RF from LULCC-attributable CO2 is unchanged for 1850-2010 but has been reduced for all future scenarios 1850-2100. These changes in RFs are reflected in the tables, figures and text.

REVIEWER: Actually, the strongest bias as to CO2 induced by the way the attribution is done is the inclusion of the loss of potential sink into the CO2 flux (again, see doi:10.5194/esd-4- 171-2013 and doi:10.5194/esd-5-177-2014). But, again, it is only a matter a choice, and I only suggest to state it clearly somewhere in the text, but not to correct it.

RESPONSE: The added references to Pongratz et al. (2014) make our methods in this regard more understandable, in our view. We do mention that we are accounting for changes in the terrestrial carbon sink attributable to LULCC in new Sect. 2.2.4 (CO2 emissions).

REVIEWER: The third paragraph of section 3.4.5, discussing the "aerosol BGC effect", actually discusses the carbon-climate feedback. The author decided to complement the IRF approach for CO2 atmospheric concentration with a simple linear correction to account for this feedback. Although it is a bit crude, I think it is a not-so-bad approach. However, I believe this whole section should be put in the section discussing the atmospheric concentration of CO2.

RESPONSE: We do agree that the C-climate feedback could also be included in the section on atmospheric CO2 changes (now in Appendix B1). However, since we compute the RF from the C-climate feedback separately from the RF from the LULCC CO2 emissions and fertilization feedback we have decided that it is most appropriate to keep this text in the section on BGC feedbacks (now Appendix B7). We have changed the title of this section to read "Biogeochemical and carbon-climate feedbacks" since the previous title referred specifically to aerosols and that may have been part of the reason why the carbon-climate feedback material seemed out of place.

REVIEWER: 2 On the outline The paper is organized following the conventional introduction-methods-results- conclusion outline. Despite being quite complete, precise and accurate, it is rather tedious to read and sometimes repetitive. I believe the paper could greatly benefit from an overhaul! More specifcally, some details should be put in Appendix, to let only the strong message in the main text.

To me, the main results are: 1) the estimation of the contribution of LULCC to RF in present days and in the future; and 2) the use of CLM the derive some emissions that are not well accounted for in other studies. The creation of a WCS is also of interest given the low likelihood of the RCPs' land-use scenarios. Thence, I would recommend puting in Appendix everything else. There would be 3 main appendixes: the way WCS is created (along with figures 3 and 4), the details of the methods and models used

(esp. about atmospheric burden and RF), the uncertainty treatment. Repetitions could be avoided by organizing the paper per process/phenomenon. Currently, the paper first enumerates all the land-use-related phenomena in introduction, then it describes how they are accounted for in the methods section, and then in the results section the values and limits are discussed. This can lead to double/triple citation of phenomena and/or references that renders the paper heavy.

I recommend an outline like this: 1. Brief introduction (quick context, goal of the study, overview of methods: follow causal-chain from activities to RF). 2. Overview of methods 2.1. LULCC activities (should not include Fires) 2.2. Emissions deduced with CLM (should include Fires, one subsection per com- pound, should include brief discussion about table 2) 2.3. Emissions not by CLM (one subsection for N2O, and one for others) 3.4. Radiative forcing (one paragraph for GHGs, one for short-lived species, one for albedo effects; give details in Appendix) 4. Results 4.1. Individual contributions from compound/process (present-day only) 4.2. Overall contribution from LULCC (present-day, RCPs, WCS) 4.3. Enhancement factor 5. Conclusions With a clear reorganization things like table 1 or introductive paragraph of section 3 could be removed.

RESPONSE: We followed the reviewer's suggested outline and find that the manuscript is much improved in readability. We include a somewhat more expanded section on RF in the methods section (new Sect. 2.4) than was recommended in order to communicate the basics of the uncertainties and shortcomings of our approach.

REVIEWER: 3 Specific points p.12170 l.4–20 To complete, one could add the change in VOCs emissions due to LCC (e.g. doi:10.1029/2005GL024164), and a reference to Arneth et al. (doi:10.1038/ngeo905) could be added as they give a comprehensive view of the feedbacks involving the land biosphere.

RESPONSE: In shortening the introduction and moving to the new outline, as described above, we had deleted some of this text. However, we now reference Arneth et al. in our discussion of carbon-climate feedbacks.

REVIEWER: p.12172 l.24–25 This is no longer true (yet not totally false) as for some compounds the AR5 gives the partition between fossil-fuel and land-use.

RESPONSE: This is true and maybe a sign that there is building interest in sector-based studies of forcing and climate change.

REVIEWER: p.12177 l.14 Unclear what happens between 2005 and 2010, as RCPs are not de- fined over this period.

RESPONSE: Thank you for pointing this out, we now include in the text:

"We use historical agricultural emissions from ACCMIP (Lamarque et al., 2010), which covers the time period of 1850-2005 and extend the historical emissions with RCP2.6 projected emissions through year 2010 for computing LULCC RFs in the year 2010."

We now make a similar statement for the extension of the land cover change timeseries for computing RFs in 2010. Also, we had been comparing our calculated total anthropogenic RF in 2010 to van Vuuren et al. (2011) and using RCP2.6 for the comparison (not much change between RCPs in 2010) but now compare to IPCC AR5 instead.

REVIEWER: p.12178 l.4 Section 3.1.1. includes a description of environmental changes (CO2, climate) used with CLM. These are not stricto sensu "LULCC activities". They could be put in Appendix with the details of how CLM is used. By the way, I did not understand how the two different climate projections were used to assess uncertainties...

RESPONSE: This paragraph has been moved to new Sect. 2.2 (LULCC emissions (computed from CLM)), a more appropriate point in the text as the reviewer points out. The use of two atmospheric forcing datasets that are very different from one another was simply used to create a range in future fire emissions that gives some idea for what the uncertainty in future fire might be, given a particular scenario. This is similar to how the RCPs are used to create a range in potential outcomes even though the uncertainty in future climate (as impacted by human activities) can be considered undefinable.

REVIEWER: p.12178 l.16–26 This whole paragraph goes with the way WCS is created (in Ap- pendix).

RESPONSE: This paragraph has been moved to Appendix A (along with the WCS text).

REVIEWER: p.12180 l.9 These are not explicit land-use activities in the study: only implicit as emission data are taken from the RCPs.

RESPONSE: This is correct, we make this clear now in the first paragraph of the section on LULCC activities (new Sect. 2.1).

REVIEWER: p.12181 l.5 Give the precision that N2O is treated separately because sectoral info is not available from RCPs.

RESPONSE: We added the text "N2O emissions are not reported by sector for the RCPs and we compute these separately (Sect. 2.3.2)." as the next sentence.

REVIEWER: p.12182 l.9 "CAM" acronym is used without being defined.

RESPONSE: This has now been defined.

REVIEWER: p.12185 l.14 Davidson (doi:10.1038/ngeo608) did some things about N2O emissions by tropical forest soils. Although uncertainties are not assessed and are certainly high, it is at least an estimate. But this relate to my major comment 1.2.2.

RESPONSE: Davidson (2009) does a really nice job estimating N2O emissions from manure and fertilizer but we still lack a good enough understanding of natural emissions to justify changing these emissions in the future.

REVIEWER: p.12188 l.5 Again, this is an old IRF.

RESPONSE: See our response to the previous comment on this subject.

REVIEWER: p.12196 l.8–13 I would not mention at all the "uncertainty in policies": it is a very different and peculiar kind of uncertainty, not directly comparable with scientific uncertainty.

RESPONSE: We have removed this text.

REVIEWER: p.12199 l.14 I think there is a bias in estimating P and Fe deposition only from fires. More specifically, fossil-fuel burning also emits phosphorus, and I think continental dusts do bear iron as well. Actually, the uncertainty is so high in that field that I would suggest not to account for these two effects at all.

RESPONSE: As suggested by the reviewer we have removed estimates of these forcings from the analysis and the reported RFs.

REVIEWER: p.12202 l.1 The discussion relative to the enhancement factor should be extended, especially regarding the effect of accounting for uncommon land-use-related emissions (Dust, Fires, etc.). Would another model give significantly different enhancement fac- tors? Are there still missing processes/compounds that could change the results, in one way or another?

RESPONSE: We address these comments with an additional paragraph in the section on enhancement (new Sect. 3.3):

"The uncertainties in this factor (computed using the monte carlo method are described in Appendix C3) are large but suggest that the enhancement is unlikely to be less than 1.3 for the year 2010 or any of the given future scenarios. Values above 4.0 for the enhancement factor are within the uncertainty range for the RCP4.5, RCP8.5 and TEC scenarios. The large enhancement factors for the RCP8.5 and TEC scenarios result mainly from the substantial CH4 RF relative to the CO2 RF. For RCP4.5, this is a reflection of the low CO2 RF attributed to LULCC and relatively high total RF with contributions from all other non-CO2 greenhouse gases. The aerosol forcings play a minor role in the sum RF attributed to LULCC but impact the enhancement factor by reducing the non-LULCC forcing considerably. The aerosol ERFs are the source of much of the uncertainty surrounding the enhancement factor. Since the RF

calculations presented here are within uncertainty estimates across many models and estimates (Fig. 3), it is likely that other models or approaches would obtain similar results if the same processes and activities were considered. We do not expect that the LULCC activities and biogeophysical forcings that we exclude from this study would have a substantial impact on the enhancement as these forcings have been shown to be small when considered on a global scale (Lawrence and Chase, 2010). Including model representation of LULCC impacts on soil carbon could increase the CO2 and total RF attributed to LULCC (Levis et al., 2014) and lead to a small reduction in the enhancement factors compared to the values we report."

REVIEWER: table 2 I suggest to add CO2 emissions (maybe cumulative) to this table. Also, see major comment 1.2.1. about vocabulary and the fact that it should be clear that the emissions presented here are the emissions attributed to LULCC following the chosen method.

RESPONSE: We have changed the language used in the caption to be more responsive to reviewer comment 1.2.1. Here we decided not to include the cumulative CO2 emissions in the table since it would introduce a different timescale to the table (currently only showing emissions from one year) and might be confusing. Also, we do not refer to the cumulative emissions from CO2 in the text any more, preferring to note the change in CO2 concentrations attributed to LULCC given the many different ways net carbon emissions from LULCC can be defined.

REVIEWER: table 5 Define AOD.

RESPONSE: Corrected.

REVIEWER: table 6 Given the importance – in the main message – of the enhancement factor, and given the authors assessed the uncertainty in the study, I strongly suggest to show uncertainty ranges of the factor in this table.

RESPONSE: We applied the same montecarlo methodology used to estimate uncertainties in the proportion of anthropogenic RF attributable to LULCC to this question. The uncertainties surrounding the enhancement factors are large but this makes sense since we are looking at a ratio with a number in the denominator (CO2 RF) which can be quite small for LULCC within its own range of uncertainty. These uncertainties are reported in Table 5.

REVIEWER: figures The figures are nice. But again, figures 3 and 4 could be put in Appendix.

RESPONSE: These figures have been moved to the appendix as per the reviewer's suggestion.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C8137/2014/acpd-14-C8137-2014-supplement.pdf

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