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Interactive comment on "The carbon cycle in Mexico: past, present and future of C stocks and fluxes" *by* G. Murray-Tortarolo et al.

Anonymous Referee #1

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This is an interesting study and I fully agree on the statement "that more comprehensive understanding of the C balance in Mexico is needed, to aid in policy formulation and to identify regions that may provide important ecosystem services like C sequestration". One question is about which scale is adequate for a "comprehensive" understanding of the C balance of a country: is 1ox1o gid enough or we should aim to higher spatial resolution? This manuscript has a lot of potential but at the current stage is a numerical exercise where methodology is not fully described nor results have been validated.

As I said, this is an interesting study, but I think the methodology has to clearly define the datasets used and account for the uncertainty in upscaling of information and the quality of the data (beyond standard deviations on the estimates from models). This country has produced and updated gridded data and newer datasets (produced during the last 6-10 years), but these have not been used in the current study (e.g., de Jong et al 2010, MexFlux, Mex-LTER, CONAFOR, INEGI, and CONABIO information). Newer datasets (and those revised for AQ/QC) could be more accurate than those used by the authors; mainly those that describe vegetation types, C flux data, LUC, and soil C. I understand that the authors used datasets that were freely available, but I want to bring to their attention that this country is doing a large effort on standardization and updating of datasets. That said, I recommend being cautious with the interpretation of the results. I suggest rephrasing the title, results, and discussion to include the main goal of this study: This study is a "numerical experiment with currently freely available data". These results, may or not, change if new and revised country-specific data is used from vegetation types, to ecosystem fluxes, to soil carbon, but need to be tested with other datasets. The main limitation of the study is that the results are not systematically tested with available "land-truth" data, and in several cases cannot be corroborated (as stated in the manuscript).

I call for transparency, reporting of uncertainties and assumptions, and to be careful on the interpretation of the results. A simple way to address these issues is to mention that is a numerical exercise that needs to be validated with future data and that there are X uncertainties associated with this work. This is of critical importance because there may be convergence on the results from the approaches because all of them give the answer because of the "wrong" reason. Therefore, error propagation and uncertainties must be included in this study.

I also encourage the authors to include hypotheses into their questions of the past, present and future. We cannot test the future but we can test the hypotheses for the past and the present. I am mainly interested in reading about why the effect of CO2 fertilization is larger than LUC during the last 100 years. During this period there have been policies and practices that have mandated and changed landscapes for a large part of the last century for Mexico. How the authors include this socioecological influence in the LUC past and future estimates in this country with large spatial hetero-

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geneities? Finally, there is no attempt to include disturbances that are common in this country and substantially influence carbon stocks (e.g., fires, hurricanes, land slides) in the past, present (e.g., the 2005 hurricane season) and future. Here, I list several comments that I hope the authors find useful to improve the manuscript.

Comments

The authors discussed that previous studies that have placed Mexico as a source of C may be biased as they have derived this conclusion for estimating Carbon fluxes form biomass change only. The authors also state that these previous approaches did not take into account soil C dynamics, the effect of CO2 fertilization on GPP or the impacts of climate change. How certain are the calculations of the authors for LUC and soil carbon that make this study less biased than previous ones? As the current results stay there is a large mismatch with soil C estimates, so how this approach reduces bias?

There is a large mismatch in scales representing the past, present, and future. The past are 99 years, but the present are just 5 years (2005-2009), and the future takes into account years 2010 to 2015 and extend into 2100. I think the authors need to reconsider the time scales presented here; mainly the present that I would argue at least should consider 20 years (1995-2015) which are the years that Mexico has had forest inventories (see de Jong et al 2010, CONAFOR 2015) and could be used to validate the study. Therefore, the future should be at least after 2016. It will be very interesting to see if the results for question 1 change if the analysis is done with the last 20 years rather than with 5 years. This is very important because during the last 15 years Mexico has implemented several policies and programs that have affected carbon dynamics and available measurements to corroborate modeling efforts (see de Jong et al 2010, Vargas et al 2012).

The questions are definitively interesting, but as results cannot be corroborated then several issues should be consider:

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A) First, I think it will be crucial to disclaim the assumptions and the uncertainties of the study in the introduction and then in the results section. This is important because the current results contrast to several past publications and the authors state that previous results may be biased (page 1205).

B) Second, I think it is important to clearly state the hypothesis of this study. For example, why CO2 fertilization on GPP could be more important for carbon dynamics that land use change? This issue is of critical importance in a country with large heterogeneity and with high land use change that was motivated by government policies; mainly during the first part of the past century (i.e., the "past" period in this study). It is critical not to ignore the history of land use change in this country that precedes satellite information and "present" inventories.

The authors wrote in the introduction that doing this study for Mexico is important because is a country with large heterogeneity, but in the methods section (page 1206) the authors reduce heterogeneity from 10 categories (derved from Ramankutty and Foley 1999) to five. Furthermore, a 1ox1o grid largely reduces and combines heterogeneity. I understand that this was done for simplicity, but it somehow contradicts the motivation of the study in this heterogeneous region. This will add uncertainty that it is not accounted for and will need explanation. Also why the authors did not use country-specific information of land cover (see CONABIO and INEGI information).

I encourage the authors to clearly state the assumptions and include calculations of uncertainty in their estimates. I totally agree that there is limited available information, but assumptions on the methodology must be transparent when doing a comprehensive country-specific estimate. There are several issues that need to be disclaimed and taken into account.

a) DGVMs, Earth system models, atmospheric inversions: I fully understand and agree on the application of these approaches, and the numerical experiment for DGVMs (Simulations 1-3), but the authors need to clearly explain that these are simulations that are hard to evaluate in this country. For example, there is the issue of past LUC that I discussed earlier and it will be very interesting to read about how this was addressed. The product MOD17 has not beet tested in ecosystems of these country. It is unclear the performance of the VOD to calculate aboveground biomass in this country (as I understand that the authors used the same algorithm that Liu et al 2011, 2013). The MTE was parameterized with worldwide towers, but not country-specific information, this is important because the authors state that they use available flux tower data (see MexFlux for country-specific data).

b) The source of the soil carbon data set is unclear. I was not able to find these data in the cited reference (SEMARNAT 2002). To the best of my knowledge it seems that the cited dataset is derived from road inventories (maybe provided by Carlos Ortiz) and the scale of 1:250,000 is not correct. To the best of my knowledge this data could have been taken from a 1:1,000,000 scale from Cruz-Cardenaz et al (2014) but transparency and clarity is needed. De Jong et al (2010) sued a systematic nation-side grid (see de Jong et al 2010 for comparison using INEGI data from a systematic grid) and there is INEGI systematic data for nation-wide purposes.

Second, there is no information about the quality assurance and quality control for that data as the cited reference is usually used for data on soil degradation (SEMARNAT 2002). Finally, the dataset is in %C but the authors used the FAO map to extract bulk density and transform the data set values to mass. This creates several issues: a) which soil depth was used?, b) which is the uncertainty added when using this generic information?, c) how the authors upscaled the information from 4000 sampling point to the national scale?, How does the authors combined the soil C field data with DGVMs and FAO dataset (page12508)? These challenges influence the fact that there are large differences for estimations on soil C across the used approaches, but is difficult to evaluate as methods do not explain in detail this approach.

c) Finally, how point-measurements and dataset were re-gridded to a 1ox1o grid? Was this done by simple linear interpolation? How errors were propagated into this ap-

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proach?

The spatial correlations between the numerical products are high (between 0.91 and 0.97). Are these correlations high because of the "correct" reason, or they are high because all these products make the same "mistakes". I think that the authors need to discuss this issue and also test for spatial correlations with the available measurements (e.g., inventory data). These analyzes applied for all the grid-point data and the total socks (eg., aboveground biomass, soil carbon, ecosystem fluxes (GPP)) but not for the site available information.

Page 12512: There are estimates in situ measurements of GPP using flux towers across Mexico (MexFlux). Page 12513 on the fact of NPP measurements the authors only cite two studies (one by Martinez-Yrizar 1996 and another by Garcia-Moya 1992). These are two relatively old studies and following them there have been multiple studies on NPP, NEE, biomass and soil carbon across the country. Just a few examples are works by Masera et al 2003, de Jong et al 1999, Masek et al 2011, Saynes et al 2005, Jaramillo et al 2003, Hughes et al 1999, Vargas et al 2008, Lawrence et al 2002, de Jong et al 2013, Delgado-Balbuena et al 2013, and many others). The authors partially review the information available across some ecosystems in Mexico, but I encourage them to review the wealth of information that has been contributed by many researchers.

I think that the manuscript needs to end with a section of limitations and considerations. For example, some of these points are emphasized on the challenges that models have to represent drought responses (page 1217) considering that water limited ecosystems cover 40% of Mexico. How this uncertainty is incorporated for past, present and future? Other issues that should be discussed are QA/QC of available data and error propagation to include uncertainties that goes beyond standard deviations of modeling approaches. This section will be of critical importance because the authors end the manuscript stating that "the methodology proposed here can be used to analyze the full-C cycle of regions elsewhere". This is important because the authors

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are not systematically testing the results with “ground truth” information across Mexico.

Finally, to add transparency and to ignite research on the “comprehensive understanding of the C balance in Mexico” all datasets used (biomass, soil carbon, climate, etc) must be published along this manuscript. This will be the only way the scientific community can move towards testing approaches and comparisons to reduce uncertainties in the regional-to-global carbon balance.

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