We thank the anonymous referee #1 for the remarkably extensive and constructive review of our manuscript. We revised the manuscript by accommodating the referee's suggestions as much as possible. In the following, we provide our responses (written in red, *added text to the manuscript italic*) to the referee's comments (written in black).

As currently written, it is difficult to discern the scientific questions the manuscript is attempting to address. While the authors describe in some detail "what" was done in the analyses, it was not clear "why" a particular analysis was conducted in the study. The manuscript indicated that land management for carbon mitigation could potentially have effects on a variety of ecosystem service indicators, but it was difficult to place the results into context to understand the main "take-home" messages that the authors intended to convey with the manuscript. As ecosystem service indicators can be interpreted as proxies for several ecosystem services (as indicated by the authors, see Section 2.4) and models can be applied to address a variety of scientific issues, it is not clear what the simulated effects on ecosystem service indicators are supposed to mean without understanding the underlying scientific questions being . There appears to be several scientific issues that the manuscript seems to be attempting to address along with some potentially interesting and useful information that is worthy of publication if these scientific issues could be clarified. Below, some ideas are suggested to help clarify the scientific issues and improve presentation of the results and discussion.

We agree that the scientific questions and take-home messages could have been emphasised better in the manuscript and thus adopted the reviewer's suggestions to revise the manuscript accordingly.

1) Overall, the motivation for the study in the manuscript appears to be that land management for enhancing carbon sequestration and/or reducing carbon loss (i.e. land-based mitigation) could have "unintended" effects on other ecosystem services provided by land ecosystems including biophysical processes that influence the Earth's energy balance in addition to land carbon fluxes, the ability to provide food and fiber, the ability to moderate water availability, and the ability to improve air and water quality. Land-based mitigation may enhance some of these ecosystem services, but degrade other ecosystem services. Thus, the basic scientific question that the manuscript appears to be trying to address is "What is the impact of land management for carbon mitigation on other ecosystem services?"

The manuscript also recognizes that two general carbon mitigation approaches have been suggested in the past: 1) avoided deforestation in combination with afforestation and reforestation (ADAFF); and bioenergy production and consumption with carbon capture and storage (BECCS). In addition, the manuscript recognizes that instead of one approach or the other, some combination of these two mitigation approaches will most likely be implemented in the future. Thus, two secondary scientific questions that the manuscript appears to be trying to address are "Do the effects of land-based mitigation on other ecosystem services differ based on the mitigation approach?" and "If so, do the effects of one mitigation approach on other ecosystem services have a more dominant effect than the other mitigation approach?"

The impacts of land-based carbon mitigation on ecosystem services and the differences between the mitigation options are indeed the primary research questions of our study. Carbon removal itself is one of the analysed ecosystem service indicators but is to some degree already predetermined by the mitigation scenarios in which carbon removal was the exclusive objective determining LU patterns. We formulated the proposed questions (slightly modified) at the end of the introduction section. In particular, we rephrase the proposed third scientific question:

"The main research questions we address in this study are:

1. *What are the impacts of land management for carbon uptake on other ecosystem service indicators?*

2. *Do the effects of land-based climate change mitigation on ecosystem service indicators differ based on the mitigation approach (BECCS, ADAFF, or a combination of both)?*
3. *If so, can a mitigation approach be identified in which trade-offs between other ecosystem service indicators are less pronounced than in the other approaches?"*

The manuscript also uses output from two land-use models (IMAGE/LPJmL and MAgPIE/LPJmL) to prescribe projections of land use for the study, but it is not clear why the authors are using two land-use scenarios in general or the results from these two models in particular. It may be that the authors simply wanted to examine how uncertainty of land-use projections to a single climate change scenario might influence the effects of land-based mitigation on ecosystem services to somewhat quantify the "noise" associated with evaluating effects. Or, the authors might have been attempting to address the scientific question "How do differences in the implementation of a particular mitigation approach influence the effect of land-based mitigation on other ecosystem services. Besides influencing different parts of the world (see Figure 2), the two land-use models also appeared to differ in the basic implementation of the land-based mitigation approaches (see Figure 1, Table A2). For the ADAFF mitigation approach, the IMAGE/LPJmL land-use projection appeared to gain natural areas mostly from the abandonment of pastures whereas the MAgPIE/LPJml projection appeared to gain natural areas mostly from the abandonment of croplands. Also, for the BECCS/ADAFF option, it was interesting that the IMAGE/LPJmL land-use projetion has more cropland than the baseline whereas the MAgPIE/LPJmL projection has less cropland than the baseline. For the BECCS mitigation option, all of the additional cropland appeared to be derivced from the conversion of natural areas to agriculture in the IMAGE/LPJmL land-use projection whereas less additional cropland appeared to be derived from the conversion of natural areas to agriculture in the MAgPIE/LPJmL projection, but more cropland appeared to be derived from more intensive use of pastures. With the exception of noting that more natural area came from cropland in the MAgPIE/LPJmL ADAFF land-use projection, the authors did not really note these systematic biases in their analysis.

We indeed used land-use projections from the two land-use models to capture the uncertainty arising from different model assumptions related to the implementation of land-based mitigation for a given CDR target, thereby affecting land demand and spatial distribution of mitigation activities. As shown in Figure 2 and Table A2, land-cover patterns by the end of the century are very different for the two land-use models, which to us seems an important aspect to our study. We clarify this in the introduction:

"By using LU patters from two different LU models we explore some of the uncertainty in indicators of ES arising from different model assumptions concerning the land demand of land-based mitigation."

The reviewer points out some interesting differences in converted land-covers which are apparent from the figures and tables but not mentioned in the text. We agree it would be useful to highlight these patterns in the text and added the following text to section 2.1:

"Avoided deforestation and afforestation in the ADAFF scenarios is chiefly located in the tropics (Fig. 2b) and afforestation typically takes place on pastures or degraded forests in IMAGE but on croplands in MAgPIE (Table S2). Bioenergy production area in BECCS is increased mainly at the expense of natural vegetation in IMAGE but taken also from existing agricultural land in MAgPIE. Total cropland area increases in the scenario combining both strategies (BECCS-ADAFF) compared to BASE for IMAGE but decreases for MAgPIE BECCS-ADAFF (Fig. 1)"

The manuscript uses the dynamic global vegetation model (DGVM) LPJ-GUESS to estimate land carbon sequestration/loss and the ecosystem service indicators. However, the land-use models also used a DGVM, i.e. LPJmL in their simulations. It is not clear from the manuscript what potential benefits were derived from using LPJ-GUESS instead of the LPJmL results for the analysis. Perhaps, some of the output for the ecosystem service indicators were just not available from the IMAGE/LPJmL and MAgPIE/LPJmL simulations to conduct the analyses. Or, perhaps there were improvements in the representation of ecosystem processes in LPJ-GUESS than in LPJmL, which might provide other scientific questions that the authors think the manuscript might be addressing, but if so, it is not clear what these scientific questions are.

The main purpose of LPJmL being coupled to the LUMs is to provide C stocks from which LUC decisions can be derived. Consequently, most variables were indeed not reported, or in many cases even simulated (e.g. N leaching, BVOC emissions), by both land-use models. The use of LPJ-GUESS allowed us to address a wider range of ES indicators in a consistent modelling framework. We clarified this in section 2.4:

"With the exception of C storage and crop production these variables were not available from the LUMs."

Additionally, LPJ-GUESS represents some ecosystem processes in more detail compared to LPJmL. As mentioned now in section 2.1 and 4.7, LPJ-GUESS simulates forest re-growth explicitly by the representation of different age classes. LPJ-GUESS also has a coupled C-N cycle, which is not represented in LPJmL.

It is not clear why the authors have quantified carbon sequestration for the various simulations in the manuscript. Did they expect carbon sequestration rates to vary with mitigation approaches or implementation of those approaches in the two land-use change projections? Did they expect the effects on other ecosystem service indicators to depend on the magnitude of carbon sequestration rates? Or, did they want to indicate a level of the potential tradeoffs between carbon sequestration and other ecosystem services if the land management led to degradation of the other ecosystem service?

We consider carbon sequestration as one of the analysed ecosystem service indicators. Our study shows that simulated carbon uptake in LPJ-GUESS is different compared to the LUMs. This was expected: while LPJ-GUESS shares some history with LPJmL the model is in many respects very different, for instance in its coupled C-N cycles and its fundamentally different representation of canopy establishment, growth and mortality. The large uncertainty in carbon removal potential in land-based mitigation efforts should be considered to assess the associated climate benefits and co-benefits/trade-offs with other ecosystem services (see former section 4.2, now section 4.7).

Besides examining overall effects at the global scale, the manuscript looks at how these land-based mitigation effects ecosystem service indicators over time (Figure A1 and A4) and space (Figure 4, A2, and A3). Thus, another scientific question the manuscript appears to address is "Do these land-based mitigation effects on other ecosystem services vary across the globe or change over time.

By clarifying the scientific questions being addressed in the Introduction and/or Methods sections will help the reader to understand the logic behind the analysis.

We added this question to the introduction:

"4. What are the spatial and temporal patterns of the impacts of land-based mitigation on ecosystem service indicators?"

2) The manuscript appears to evaluate qualitative effects of land-based mitigation on other ecosystem services by using directional changes in ecosystem service indicators. In Table 2, the authors nicely indicate how the ecosystem service indicators relate to the various ecosystem services. However, Table 2 is not currently referenced until the Discussion section. As the information in Table 2 does not appear to depend on any study results, it would be better to move Table 2 to section 2.4 (and rename to be Table1) to link how mitigation-induced changes in ecosystem services (i.e. the scientific questions) are being evaluated with the ecosystem service indicators. As several ecosystem service indicators appear to be related to a single ecosystem service and other ecosystem service indicators appear to be

related to more than one ecosystem service, the Results and Discussion sections could be reorganized to be consistent with the information presented in Table 2. Some of this organization already exists in the Discussion section of the manuscript with Section 4.3 describing the effects on water availability and potential implications on flood protection, Section 4.4 describing the effects on food production, and Section 4.5 describing the effects on water and air quality. Section 4.1 also appears to be describing carbon mitigation effects on other ecosystem services affecting climate change mitigation although the section title is described a little differently. Because Section 4.2 appears to be focused on comparing land-based carbon mitigation results of this study to other studies, it might be better to have this section occur (perhaps a new Section 4.1) before discussing the effects of land-based carbon mitigation on other ecosystem services in the later subsections. However, because the focus of the paper seems to be on the effects of land-based mitigation on other ecosystem services rather than land-based carbon mitigation per se, the text in this section tends to distract the reader from those messages so that it might be better to have this text in a section at the end of the Discussion, perhaps under a title of something like "Role of model assumptions on the uncertainty of land-based carbon mitigation and its relative importance to other ecosystem services".

The reviewer rightly points out that Table 2 should be moved to section 2.4 to introduce the relationship between ecosystem service indicators and ecosystem services already at an earlier stage. We restructured the discussion according to the logic of Table 2. We agree that the carbon removal section 4.2. might distract a bit too much from the main message of the manuscript and it is a good suggestion to move the (revised) sub-section to the end of the discussion.

By moving Table 2 to Section 2.4, the current general organization of the Results section would be okay, but it would be desirable that between the Results and Discussion sections, the reader would understand the "take-home" messages. One "take-home" message may be that land-based carbon mitigation, regardless of mitigation approach:

- Reduces crop production

- Potentially improves water and air quality by reducing nitrogen loss

A second "take-home" message may be that the effects of carbon mitigation on some ecosystem services depend on the mitigation approach and sometimes depends on the particular implementation of the BECCS mitigation approach:

- ADAFF tends to enhance climate change mitigation by enhancing evapotranspiration;
BECCS effects depend on land-use projection with IMAGE/LPJmL tends to reduce climate change mitigation by slightly reducing evapotranspiration and MAgPIE/LPJmL tends to enhance climate change mitigation by slightly enhancing evapotranspiration;
ADAFF effects on climate change mitigation by evapotranspiration changes appear to dominate in the ADAFF/BECCS mitigation option.

- ADAFF tends to reduce climate change mitigation by slightly reducing albedo; BECCS tends to enhance climate change mitigation by slightly increasing albedo; ADAFF effects on climate change mitigation by albedo changes appear to dominate in the ADAFF/BECCS mitigation option

- ADAFF tends to reduce water availability by slightly reducing runoff; BECCS effects depend on climate change mitigation with IMAGE/LPJmL tending to enhance water availability by slightly increasing runoff and MAgPIE/LPJmL tending to reduce water availability by slightly decreasing runoff; ADAFF effects on water availability by runoff changes appear to dominate in the ADAFF/BECCS mitigation option

- ADAFF tends to increase flood protection by slightly reducing peak runoff; BECCS effects depend on climate change mitigation with IMAGE/LPJmL tending to decrease flood protection by slightly increasing peak runoff and MAgPIE/LPJmL does not seem to have an effect on flood protection;

ADAFF effects on flood protection by peak runoff changes appear to dominate in the ADAFF/BECCS mitigation option

- ADAFF degrades air quality by increasing BVOCs; BECCS enhances air quality by decreasing BVOCs; ADAFF degrades air quality by increasing BVOCs; ADAFF effects on air quality by BVOC changes appear to dominate in the ADAFF/BECCS mitigation option

A third "take-home" message might be that the implementation of a mitigation approach (or "option") influences the temporal and spatial variability of land-based carbon mitigation and its effects on other ecosystem services.

The reviewer nicely summarised the key findings of our study. A summary of the main results is indeed necessary and was not put clearly in the first version of the manuscript. We revised the conclusion section 4.8 accordingly:

"Terrestrial ecosystems provide us with many valuable services like climate and air quality regulation, water and food provision, or flood protection. While substantial changes in ecosystem functions are likely to occur within the 21st century even in the absence of land-based climate change mitigation, additional impacts are to be expected from land management for negative emissions. In all mitigation simulations, what might generally be perceived as beneficial effects on some ecosystem functions and their services ((e.g. decreased N loss improving water/air quality), were counteracted by negative effects on others (e.g. reduced crop production), including substantial temporal and regional variations. Environmental side-effects in our ADAFF simulations were usually larger than in BECC, presumably reflecting the larger area affected by land-cover transitions in ADAFF. Without a valuation exercise it is not possible to state whether one option would be "better" than the other. All mitigation options reduced crop production (in the absence of assumptions about large technology-related yield increases) but potentially improve air and water quality via reduced N loss. Impacts on climate via biophysical effects and on water availability and flood risks via changes in runoff were found to be relatively small in terms of percentage changes when averaged over large areas, but this does not exclude the possibility of significant impacts e.g. on the scale of large catchments."

Additionally, we aimed to emphasize the implications of our main results when revising the discussion section.

3) The additional amount of carbon uptake related to the simulated land-based mitigation efforts estimated by the study in the manuscript are 40 to 60% less than the 130 Gt C presumed by the studies that developed the IMAGE/LPJmL and MAgPIE/LPJmL land-use projections. This discrepancy where the same land-use projections have such large differences in simulated carbon sequestration rates suggests that there are some major differences in model assumptions between this study and the studies used to develop the land-use projections. The manuscript seems to attempt to address this discrepancy in the Abstract, the Methods section, the Results section and the Discussion section which distracts the reader from what otherwise appears to be the main focus of the manuscript, the effect of carbon mitigation activities on other ecosystem services, and confounds the "take-home" messages to be derived from the analysis in the manuscript. While the discrepancy in carbon sequestration rates should be addressed by the manuscript, the importance of the discrepancy needs to be related to the objectives of the manuscript.

One possibility might be to indicate that if there are trade-offs between land-based carbon mitigation and their effects on other ecosystem services, then decisions would depend on the magnitude of carbon mitigation that might be achieved to determine the worthiness of the mitigation activity. There may be, however, large uncertainties in the amount of carbon sequestration that may be estimated for a particular land-use projection based on assumptions used by various models and give the above example. Then describe some of the potential differences in assumptions that might affect carbon sequestration estimates, such as part of the text in current Section 4.2. As indicated in comment 2), this text may be organized into a section placed at the end of the Discussion with perhaps the title "Role of model assumptions on the uncertainty of land-based carbon mitigation and its relative importance to other ecosystem services". While it is still worthwhile to indicate the assumed carbon sequestration used by the studies used to develop the land-use projections because it affected the distribution of the projected land use, mention of the 130 Gt C in the Abstract is more confusing than helpful and should be deleted. In addition, comparisons of the results of this study to the carbon results of studies used to generate the land-use projections (including the comparisons of crop production) should be deleted from the Results section and restricted to the Discussion section where the results of this study are compared to other studies to provide perspective.

We agree that focusing on carbon uptake, while being one of the ecosystem service indicators analysed in this study, distracts from the main message. The differences in carbon uptake will be the subject of an upcoming manuscript, but we - as the reviewer - think that some information should be already provided in the present manuscript. We removed the 130 GtC target and the crop production numbers reported by the land-use models from the abstract and the results. Additionally, we adapted the reviewer's suggestions about restructuring section 4.2 and placing it at the end of the discussion section:

"4.7 Role of model assumptions on carbon uptake via land-based mitigation and implications for other ecosystem services

Our simulations show that trade-offs between C uptake and other ES are to be expected. Consequently, the question whether land-based mitigation projects should be realized depends not only on the effects on ES, but also on the magnitude of C uptake that will be achieved. However, our study suggests that potential C uptake is highly model-dependent: C uptake in the three land-based mitigation options in LPJ-GUESS…"

4) In the Methods section, the authors describe how bioenergy crops, carbon capture and storage, and afforestation are simulated in IMAGE/LPJmL and MAgPIE/LPJmL, but not LPJ-GUESS. Yet, the carbon dynamics in the analysis of the manuscript is being simulated by LPJ-GUESS using land-use change projections developed with IMAGE/LPJmL and MAgPIE/LPJmL. Thus, it would seem to make more relevant to describe how LPJ-GUESS estimates carbon dynamics for bioenergy crops, the influence of N fertilizer application on bioenergy crop production, carbon capture and storage, and afforestation rather than the land-use models in the Methods section and perhaps move the description of how these models estimate carbon dynamics of bioenergy crops, the influence of N fertilizer application on bioenergy crop production, carbon capture and storage, and afforestation are simulated by land-use models to the Appendix in support of how the land-use projections were developed.

We think that describing the assumptions made in the land-use models is important to understand the resulting land-use patterns in the mitigation scenarios and should thus be part of the main text. How LPJ-GUESS represents carbon dynamics and human management is described extensively in the cited literature and some model features particularly relevant for this study are mentioned in the discussion (e.g. forest regrowth in former section 4.2, now section 4.7) or the Supplement (e.g. residue removal in Supplement A, CCS in Supplement B). However, we expanded the LPJ-GUESS description in section 2.1:

"Vertical forest structure is accounted for by the use of different age classes for woody PFTs…Croplands are represented by prescribed fractions of five crop functional types (CFTs, see Table S1) which are moderately tilled, fertilized, and harvested (Olin et al., 2015a), and are prescribed to be either irrigated or rain-fed (Lindeskog et al., 2013). Specific bioenergy crops are currently not represented."

5) The second sentence of the Abstract is a bit awkward and confusing. "However, land-based mitigation's prospect of success depends on potential side-effects on important ecosystem services." It is not clear what the authors are trying to say here.

We rephrased the sentence:

"However, the acceptance and feasibility of land-based mitigation projects depends on potential side-effects on other important ecosystem functions and their services."

6) The first paragraph of the Discussion seems more appropriate to be in the Methods section (Section 2.4) It is also not clear what the last sentence of this paragraph in the Discussion is attempting to say: "The changes in our mitigation simulations will occur in addition to the changes originating from climate change, increased atmospheric CO2, and non-mitigation related LU/management changes over the century, thereby intensifying or dampening the supply of ES to human societies." Perhaps the message is something like "Ecosystem services will be influenced by changes in climate, atmospheric chemistry and land use even in the absence of land management for carbon mitigation. To separate these non-mitigation effects from those effects associated with a mitigation approach, we compare changes in ecosystem service indicators in the baseline simulations over the 21st century to the changes that occur when a mitigation approach is implemented. Land-based mitigation may potentially enhance or degrade another ecosystem service to human societies."

We moved the paragraph to section 2.4. The reviewer is right about the meaning of the last sentence of the paragraph and we adopted the suggested revision to the sentence to make the statement clearer.

7) In section 4.1, it would probably be worthwhile to note that using an Earth System Model of Intermediate Complexity, Hallgren et al. (2013) found that the unintended biogeophysical cooling effects of biofuels production more than compensated for the warming effects associated with enhanced release of greenhouse gases from the biofuels production at the global scale. This study also found that biofuel production had small impacts on global surface temperatures, but had larger impacts on regional surface temperatures, such as the Amazon Basin and part of the Congo Basin.

We included the following sentence in section 4.1:

"A modelling study by Hallgren et al. (2013) found that while albedo effects and C emissions from deforestation for biofuel production might balance on the global scale, biophysical effects can be large locally."

8) In section 4.1, it seems strange that the authors would discuss changes in BVOCs as part of the climate regulation via biogeochemical effects, but not changes in carbon storage, which would seem to be more substantial. In addition, wouldn't changes in BVOCs and their effects on warming/cooling be included in the calculations of the effects of overall changes in the carbon budget on warming?

The magnitude of C losses from BVOCs is relatively small. We added the following sentence to the paragraph:

"BVOC emissions also impact climate directly by reducing terrestrial C stocks but the magnitude is small (<0.5%) compared to total GPP."

Ideally, one could estimate the total climate effect of all analysed ES indicators but as indicated in the text this is particularly difficult for BVOCs. Additionally, we were only able to analyse effects on some of the many ES indicators that ecosystems provide. A calculation of the overall climate effect of land-based mitigation is thus beyond the scope of our study.

9) In Section 4.2, there are a couple of additional issues that might be influencing the discrepancies between LPJ-GUESS and the target value (i.e. 130 Gt C) used in the land-use models that seem to be missing from this Discussion. First, is the 130 Gt C actually CO2-C or CO2 equivalent C? If the latter, then some of the 130 Gt C could be greenhouse gases other than CO2 so that the discrepancy between LPJ-GUESS and the land-use models may not be as bad as indicated in the text. Second, was there a dynamic linkage between LPJmL and IMAGE or MAgPIE so that information on changes in land productivity and land management were passed iteratively between the two models such as in Reilly et al. (2012)? Or was information just passed between the two models non-iteratively, such as in Melillo et al. (2009)? The first approach would allow feedbacks to potentially influence carbon sequestration

whereas the second approach would not allow such feedbacks. By prescribing land use, the carbon dynamics of LPJ-GUESS would not be influenced by potential feedbacks that might have occurred if the land-use models and LPJmL passed information iteratively to estimate different carbon sequestration rates.

The 130 GtC are CO2-C, not CO2-equivalent. We clarified this in the introduction:

"Each of these target a CDR of 130 GtC (only CO_2-carbon, omitting other greenhouse gases) by the end of the century, which is approximately equivalent to the cumulative deforestation CO_2 emissions from the late 19th century to today, or around 60 ppm (Le Quere et al., 2015)."

Information was passed non-iteratively between the land-use models and LPJmL. We clarify this in section 2.2:

"The LU scenarios were created using harmonized assumptions about climate change, atmospheric composition, and socio-economic development and thus did not include C cycle feedbacks."

10) In the first sentence of Section 4.3, not clear what "replacing grassland, respectively shrublands, with large variability" means. Did the authors mean "replacing grasslands and shrublands, respectively, with large variability". This strange wording associated with "respectively" occurs in several places in the manuscript.

This is indeed what we meant. We changed the wording accordingly in such cases.

11) In the fourth sentence of the third paragraph of Section 4.3, the sentence is awkward and difficult to understand. It might improve if the phrase "They found no longer a statistically significant correlation" became "They did not find a statistically significant correlation".

We changed the sentence accordingly.

12) In Section 4.4, the authors should relate the study results to Reilly et al. (2012) who found higher prices for agricultural products due to mitigation costs of land, energy, and other greenhouse gas controls in their ADAFF-like (i.e. the No Biofuels scenario in Reilly et al. 2012) and ADAFF/BECCS-like (i.e. Energy + Land scenario in Reilly et al. 2012), but did not find higher prices for agricultural products in the BECCS-like (i.e. the Energy-Only scenario in Reilly et al. 2012) scenario because the higher mitigation costs were offset by benefits of avoided environmental damage to other ecosystem services.

We added the following sentence to section 4.4:

"Similar results have been reported by Reilly et al. (2012) who found that afforestation substantially increases prices for agricultural products, while the cultivation of biofuels has little impacts on agricultural prices due to benefits of avoided environmental damage offsetting higher mitigation costs."

References

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