"**Regional variation in the effectiveness of methane-based and land-based climate mitigation options**"
Hayman et al. (ESD-2020-24)

Response to Reviewer Comments

The reviewer comments are in normal font, with our responses in ***bold italics and indented***. All line numbers refer to the originally-submitted manuscript.

Reviewer 1

Summary

The authors present and apply a method for examining the contributions of methane and land-based mitigation to meeting 1.5 and 2 degree warming targets. They give a thorough description of their modeling framework and present the results of an ensemble analysis of individual and combined contributions of methane vs land mitigation to emissions reductions and how these reductions can allow for complementary fossil fuel emissions. They conclude that methane mitigation contributes 2-4 times more reduction potential than land-based mitigation, depending on the BECCS assumptions. They also show that there are regional differences in how effective BECCS is compared to afforestation/reforestation, and estimate that bioenergy crop productivity must be fairly high in some places (with low transport losses) for it to be an effective strategy. They also show that water usage for BECCS may impose limits to BECCS deployment in some regions.

Overall Response

This is an interesting and well-thought-out paper that examines key uncertainties in how to reach 1.5 and 2 degree targets. While I am not an expert in methane dynamics, the description of the framework is detailed enough to convey that the approach is reasonable for this analysis (assuming the methane references this is based on also have adequate methods). My main concern is the framing, and there is also some clarification of the experimental design that is needed. I elaborate on these two things here, with more detail below.

1) While I can appreciate the goal of presenting alternatives that allow for some fossil fuel emissions in these strict scenarios, this goal is not clearly articulated, and I am not sure that it is the most reasonable framing for this issue. Given that these are idealized scenarios and that there is considerable uncertainty on the adoption of mitigation policies, the actual extent of implementation of mitigation strategies, the assumptions and efficacy of mitigation strategies, and the modeling method, the estimated reduction levels here indicate that these approaches are more at the level of additional measures that would help ensure meeting particular targets under certain fossil fuel emission scenarios, rather than allow for more emissions to occur. Stating that doing these other mitigation actions allows for more fossil fuel emissions simply shifts responsibility away from the primary cause and increases the risk that these targets would not be met (the probability of exceedance is not particularly low to begin with). I suggest re-framing the study as additional mitigation potential or “insurance” mitigation potential. Barring a complete rework of the framing of the study away from allowing more fossil fuel emissions and toward additional mitigation potential, there at least needs to be more discussion regarding the magnitude of these results in relation to the large uncertainties inherent in mitigation approaches, idealized scenarios, and modeling.

> ***<u>Response:</u> It was certainly not our intention to suggest that methane mitigation is an alternative to CO_2 mitigation. To avoid this impression, we will make the following chnages:***

a) ***We replace the last line of the abstract with: "Although the primary problem remains mitigation of fossil fuel emissions, our results highlight the unrealised potential for the mitigation of CH_4 emissions to make the Paris climate targets more achievable".***

b) ***By rewriting the last three sentences of the paper as: "Stabilising the climate primarily requires urgent action to mitigate CO_2 emissions. However, CH_4 mitigation has the potential to make the Paris targets more achievable by offsetting up to 188-212 GtC of anthropogenic CO_2 emissions. We conclude that CH_4 mitigation would be effective globally and especially so for the major CH_4-emitting regions of India, USA and China".***

2) The description and figures and tables associated with the experimental design and its corresponding conditions are inconsistent and confusing.

Response: ***We respond below to the specific reviewer comments on these points.***

Specific comments and suggestions

Abstract

Lines 29-31: You should include that BECCS assumptions in general contribute to most of this range.

Response: ***We will amend the abstract to include this point about the BECCS assumptions.***

Introduction

You should include a description and examples of the expected emissions for 1.5 and 2 degree targets, which generally indicate that total (and fossil fuel) emissions need to drop to zero or negative to reach these goals. This provides a better context for why you are looking at how methane and land mitigation can alleviate the pressure to eliminate fossil fuel emissions completely.

Response: ***The idealised temperature pathways used in this work imply the need to drop to zero emissions (lines 209-210). In our previous work (cited paper by Comyn-Platt et al., 2018), we derive the parameters for the temperature pathways from comparison with CMIP5 simulations for the RCP2.6 scenario (Supplementary Information, Figure 2 of Comyn-Platt et al., 2018).***

We add remaining carbon budget for the 1.5°C and 2°C warming targets, equivalent to the discussion we gave in our earlier paper (cited paper by Comyn-Platt et al., 2018) and also from the published literature and IPCC reports. As indicated above, we amend the text to make explicit the need for complete removal of fossil carbon emissions and the likely need for negative emission technologies.

Approach and Methodology

line 149: What is the first variable k?

Response: ***k is a dimensionless scaling constant such that the global annual wetland methane emissions are 180 Tg CH_4 in 2000. We will add this sentence.***

line 172: It isn't clear how the global annual CH4 concentration is used to linearly interpolate monthly ozone values.

> ***<u>Response:</u> We do not model tropospheric ozone production from methane explicitly in IMOGEN. Instead, we use two sets of monthly near-surface O_3 concentration fields (January-December) from HADGEM3-A GA4.0 model runs, the sets corresponding to low (1285 ppbv) and high (2062 ppbv) global mean atmospheric CH_4 concentrations. We assume that the atmospheric O_3 concentration responds linearly to the atmospheric CH_4 concentration in each grid cell. We derive separate linear relationships for each month and grid cell, and use these to calculate the surface O_3 concentration from the corresponding atmospheric CH_4 concentration as it evolves during the IMOGEN run.***
>
> ***We will amend the text using the above.***

lines 203-204? Why only the non-CO_2 components? If the models use different radiation schemes the CO2 component could also contribute to this uncertainty. Unless the CO_2 radiative forcing is calculated the same way across the GCMs and in IMOGEN?

> ***<u>Response:</u> From the cited paper by Huntingford et al. (2010), IMOGEN uses four parameters for the energy balance model: these are climate feedback parameters over land and ocean, λ_l and λ_o (W m^{-2} K^{-1}) respectively, oceanic "effective thermal diffusivity", κ (W m^{-1} K^{-1}) representing the ocean thermal inertia and a land-sea temperature contrast parameter, ν, linearly relating warming over land, ΔT_l (K) to warming over ocean, ΔT_o (K), as $\Delta T_l = \nu \Delta T_o$. The climate feedback parameters (λ_l and λ_o) are calibrated using GCM data for top of the atmosphere radiative fluxes, mean land and ocean surface temperatures, along with an estimate of the radiative forcing modelled by the GCM for the CO_2 changes. Thus, IMOGEN emulates the radiative forcing of CO_2 within the individual GCMs.***
>
> ***For a given prescribed trajectory in temperature, and pathway in atmospheric non-CO_2 greenhouse gas emissions, we calculate compatible CO_2 emissions. These emissions trajectories are different, dependent on which climate or Earth system (ES) model is emulated (via IMOGEN).***

line 208: They "use" or "define" a framework?

> ***<u>Response:</u> Accepted, missing word in "Huntingford et al. (2017) a framework"***

line 207: "emissions"

> ***<u>Response:</u> We will add "of greenhouse gases and short-lived climate forcers" after emissions in "model the efforts of humanity to limit emissions and, if necessary, capture atmospheric carbon"***

lines 219-265: (section 2.3) This is very confusing and doesn't align with figure 3 or table 2. Table 2 is the most clear expression of the experimental design and should be used to organize this section and should be referenced up front. I suggest starting this section with a clear explanation of how many scenarios there are and each of the distinct components used to build them. Also, the nomenclature across the text, table 2, and figure 3 is inconsistent, adding to the confusion. It is also unclear how you reach different temperatures for control simulation, which appears to have a prescribed radiative forcing. I presume that the total radiative forcing is not prescribed, and that CO2 conc. and associated CH4 conc. feedbacks adjust to meet the prescribed temperature.

Response: ***With this and the comments from Reviewer 2, we restructure Section 2.3-2.5 to make the scenarios used clearer, to remove any inconsistencies and to avoid any repetition. See also the response below to the comment on lines 328-353.***

The reviewer is correct that "the total radiative forcing is not prescribed, … adjust to meet the prescribed temperature" .We will expand the text on the inverse version of IMOGEN to make this clear (lines 197-206).

line 231: specify "… reduction in CH4 emissions …"

Response: ***We will amend the text as suggested.***

line 240: figures 4 and 3 should be switched

Response: ***This may follow from the restructuring of Sections 2.3-2.5.***

lines 267-281: Are you assuming that carbon stored in the atmosphere is just CO2?

Response: ***The reviewer is correct that the calculation of the atmospheric carbon store in the post-processing does not take account of CH_4. This is a reasonable approximation, however, given the relative magnitude of the atmospheric concentrations of methane (~2 ppmv at the surface) and carbon dioxide (400 ppmv).***

For the contemporary period, IMOGEN retains ~50% of carbon dioxide emissions in the atmosphere, after land and ocean draw down is accounted. In that sense, there is closure of direct carbon units. For methane, a slightly different approach is used. We calculate the atmospheric CH_4 concentrations from the CH_4 emissions (from both anthopogenic and natural sources) and an atmospheric loss term, parameterised via a methane turnover lietime. We take account of the radiative forcing of atmospheric methane and its effect on the terrestrial carbon cycle (through tropospheric O_3 production and vegetation O_3 damage).

line 296: Is there a better word than "productivity" here? Maybe "efficacy" or "mitigation potential"?

Response: ***We will amend the text along the lines suggested.***

lines 328-353: this section 2.5 should be moved up and merged with section 2.3 (see previous comment) in order to clarify the experimental design.

Response: ***As per the responses to the comment on lines 219-265 and from Reviewer 2, we will restructure sections 2.3-2.5. Some of the existing Section 2.5 will be moved, but the material on optimisation and mitigation potential needs to come after Section 2.4.***

<u>Results and Discussion</u>

line 367: and saturation effects?

Response: ***We reconfirm this sentence is correct, but there would also be eventual saturation. The oceanic draw down of CO_2 is based on the work of Joos et al (1996), with the equations specific to IMOGEN given in the Appendix of Huntingford et al (2004). The CO_2 draw down flux is based on the difference between atmospheric CO_2 and CO_2 concentrations near the ocean surface. The oceanic CO_2 concentration is calculated as a weighted integration in time of this flux, and where such weighting accounts for oceanic diffusive mixing. If atmospheric CO_2 rises quickly, there is a co-benefit as the oceanic draw down will***

rise due to the gradient between the two CO_2 concentrations. However, the reviewer is correct, that under climate stabilisation, saturation will occur and this flux will decrease to zero.

lines 370-378: I would also like to see this put into the context of overall scenario uncertainty, as this is highly dependent upon human action. For example, the methane-related-mitigation AFFEB (over 85 years) is on the order of only about 6 years of late century SSP5-8.5 emissions, which is a reference scenario. In the greater context, the potential methane mitigation effects represent more of a cushion or insurance approach to meeting idealized targets.

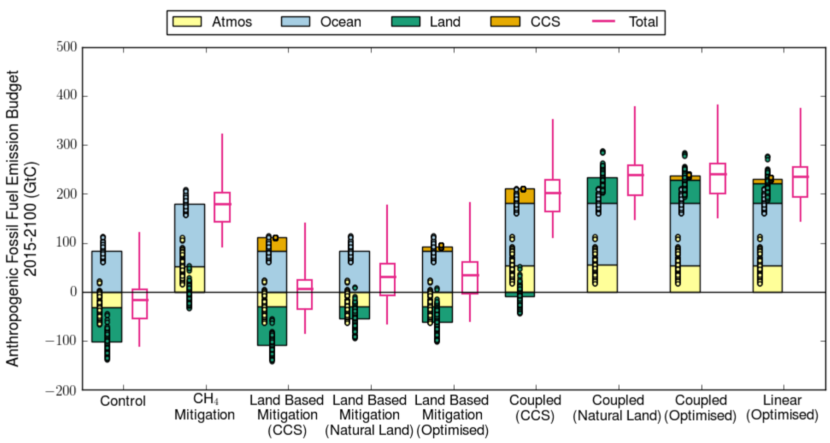
Response:* *We use the SSP2 reference scenario as the control scenario. The baseline forcing in this scenario is slightly above 6 W m^{-2}. The SSP5 8.5 scenario is a long-way from the aspirations of the Paris agreement. This paper focusses on the more policy-relevant question of how CH_4 mitigation can contribute to meeting the Paris targets. In response to this and related comments, we make it clearer that CH_4 mitigation is in no way an alternative to CO_2 mitigation.

lines 379-383: This appears to be true, but this may be a coincidence as the dynamics appear to be quite different between separate and coupled mitigation. The figures do not show the correct breakdowns for the linear sum.

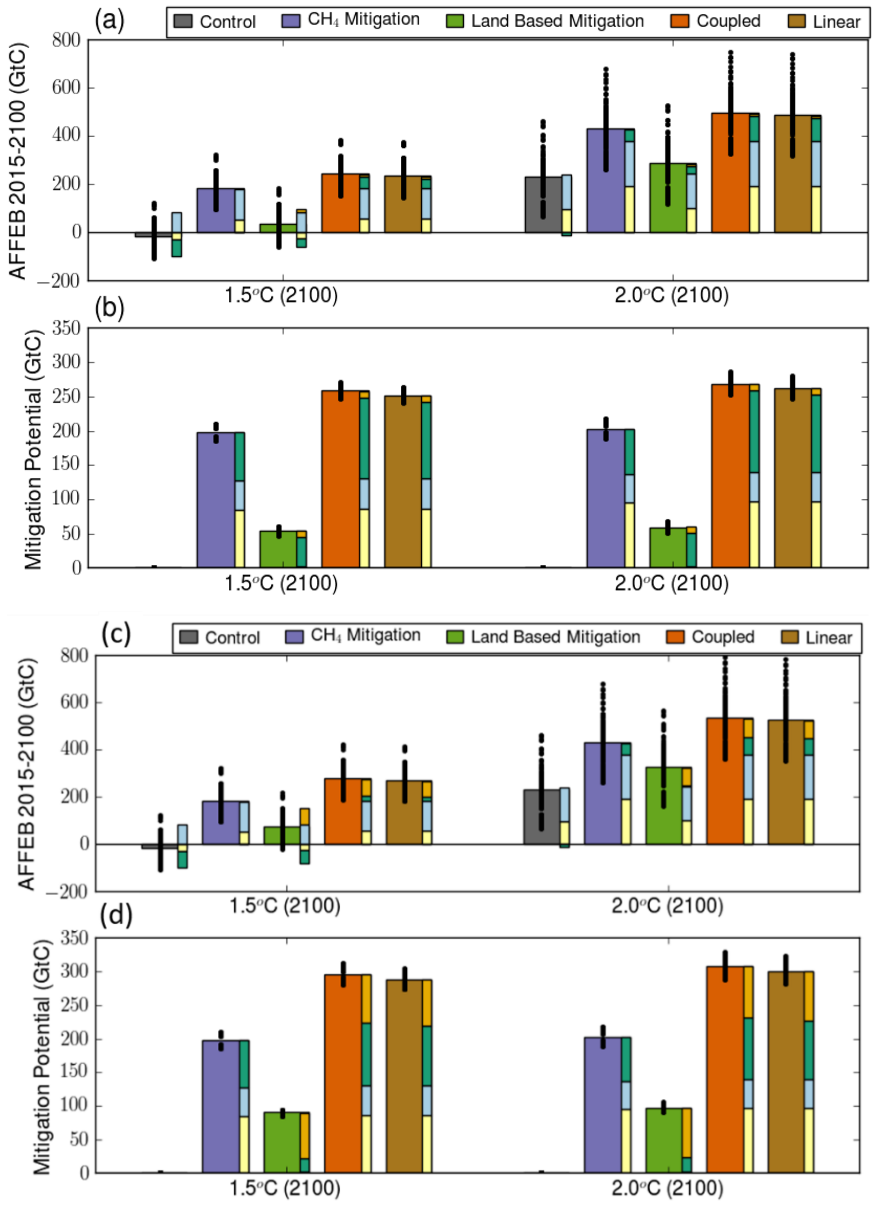
Response:* *Although Figures 6 and 7 are correct and as the reviewer notes for Figure 6 (see below), adding the CH_4 mitigation (second bar) to any of the land-based mitigation options (bars 3-5) does not appear to give the corresponding coupled option (bars 6-8). This is because the gain in the land carbon store for the methane mitigation option is shown as a reduction from -70.8 GtC in the control run to -1.4 GtC in the methane mitigation option (median of ensemble). This then explains the positive changes shown for the land carbon stores in the coupled runs. Comparing the final two bars, there is very good agreement in the breakdown of the carbon stores for the coupled and linear (i.e., the sum of the individual) mitigation options. Thus the dynamics and in the coupled and linear cases are almost identical.

Although we state that "there is increased uptake of carbon by the land, directly because of the increased atmospheric CO_2 concentration and indirectly through the reduction in O_3 damage, which is greater than the land carbon lost through land-use changes" (lines 363-365), this did not make clear of the size of the change. We will add a sentence to the text and figure caption to make the point about the change in the land carbon store for the methane mitigation option.

Figure 6



<u>Figure 7</u>



lines 389-415: section 3.2 Is this for the 1.5 degree scenario only?

> ***<u>Response:</u> We will amend the text and add a reference to Supplementary information, Figure 2, which shows the equivalent plot for 2°C of warming to Figure 10.***

lines 413-415: Clarify that this is the BECCS only amount (for only 1.5 degrees?), which is double the original amount. Also add the numbers for land mitigation potential shown in figure 7c-d, as these are apparently in the abstract (100 GtC) and are comparable to the numbers in the previous section. This also makes a better case for "strong sensitivity" to BECCS assumptions, although tripling productivity and reducing transport losses by 2/3 to get a doubling in reduction is hardly a "strong sensitivity."

> ***<u>Response:</u> We apologise as we inadvertently gave the incorrect numbers for the carbon budgets. The correct text should read "We now find the global land-based mitigation potential to be ~~56-62~~ <u>88-100 G</u>tC, as shown in Fig. 7(c) and (d). We use κ = 3 in the subsequent analysis of regional mitigation options and of BECCS water requirements." This is now consistent with the land-based bars in Figure 7d (second bar) and the upper range of 100***

GtC given in the abstract. The range is based on the results from both the 1.5°C and 2°C runs for the optimised land-based mitigation.

line 430: larger than what? the land mitigation? You should also note the exceptions here, which are abundant: canada, mexico, south america, brazil, west africa, south africa, korea, japan

Response: ***We were referring to the global impact of methane mitigation. As presented in Figures 9 and 10, the reviewer is correct that the land-based mitigation is larger than methane mitigation for a number of the IMAGE regions, especially when using a BECCS scaling factor $\kappa = 3$. As we are discussing the mitigation options at a regional level, we will amend lines 430-431 as shown "CH_4 mitigation ~~generally has a larger impact on emission budget reductions,~~ is an effective mitigation strategy for all regions, and especially the major methane emitting region."***

line 473: "… regions that produce …"

Response: ***Accept text change***

lines 481-482: This needs more explanation. It isn't clear from the figure that these three regions have water issues under this case, especially china. While two of them would use all water availability, one does not, and none appear to exceed availability.

Response: ***In the next comment, the reviewer has suggested adding the BECCS water demand and percent of available used to Table 4. We will use this information to clarify whether these IMAGE regions have issues with water availability.***

line 469: Table 4 should include BECCS demand and percent of available used in the example cases. Then you would have a basis for the statement in lines 48-482.

Response: ***We will add these to Table4 or add as a new Table.***

Conclusion

line 499: This "strong sensitivity" is not clear from the paper. The results can more clearly explain how BECCS mitigation can double, although based on tripling of productivity and a 2/3 reduction in transport losses, which nearly doubles the land mitigation potential. This is tremendous increase in BECCS efficacy to get this result, so I am not sure that it is a "strong sensitivity." And you don't show what figure 11 looks like with the original beccs values, to see how much difference the beccs efficacy makes on land cover. Also note that this has a much smaller relative effect on the total AFFEB.

Response: ***We accept the reviewer's viewpoint that our perturbations are relatively large so the changes do not necessarily imply a "strong sensitivity". We have therefore removed "strong" from line 499.***

Tables and Figures

Figures 3 and 4 are out of order.

Response: ***This will be resolved with the restructuring of Sections 2.3-2.5.***

Figure 3 The legends and the caption and table 2 and the references in the text don't match, which makes the experimental design unclear. The CH4 plots are c, e, g

> ***<u>Response:</u> The figure is intended to show key data inputs for or differences between the model runs to help inform reader's understanding of the paper. The titles of the panels are correct but we accept the figure and panels need careful reading. The other reviewer also commented that the figure is unclear. We will amend the figure to make it clearer (and potentially split the figure into two as part of the restructuring of Section 2.3-2.5).***

Figure 6 Is the linear optimized the sum of ch4 and land based mitigation? Is so, then the bar breakdown is incorrect, as the coupling changes the land response.

> ***<u>Response:</u> Please see response to comment on lines 379-383 above, as this is a repeat of that comment.***

Figure 7 again the linear sum does not look correct

> ***<u>Response:</u> Please see response to comment on lines 379-383 above, as this is a repeat of that comment.***

Figures 12 and 13 I am confused about water withdrawal vs. irrigation demand. Isn't water withdrawn for irrigation? is this irrigation demand assumed to be additional water withdrawal?

> ***<u>Response:</u> We will amend this. The reviewer is correct that irrigation is water withdrawal. The separation arose as we had separate datasets for (a) agricultural irrigation and (b) water withdrawals for energy production, use in industry and in cities. We will adjust the figure to remove any confusion.***

Table 4 You should include BECCS demand and percent of available that would be used.

> ***<u>Response:</u> Please see the response to the comment on line 469 above, as this is a repeat of that comment.***