**Reviewer Report**

The role of animal agriculture in driving climate change is an important and often complex topic, where science can help guide societal understanding and policy makers. This study contributes to this scientific basis, and is novel in combining the impact of livestock emissions reductions with potential biomass carbon recovery, to provide a ‘climate opportunity cost’ of animal agriculture. The manuscript is generally very well-written and clear regarding how the study was carried out, the assumptions and data used, and the conclusions are hard-hitting.

My comments are long: I have tried to be as thorough as possible to help the authors strengthen this manuscript before publication, and mostly the comments should be straight-forward to address, relating to clarifications, consistencies, and suggestions for improved referencing.

I do have two more significant comments relating to the assumptions used in the modelling which need addressing (see below, major comments 1) and 2)). The authors are generally clear and up-front about their assumptions, and do emphasize that this is a simple study, however, I think further clarity is needed, and a discussion in particular of two of the assumptions and how they have shaped the results (soy as a replacement of protein and potential biomass carbon recovery). I think at the least it needs to be acknowledged that these two assumptions likely overstate the magnitude of the results, and why. The authors could also provide results of a simple sensitivity analysis (perhaps in the SI) of these assumptions: this would strengthen the main conclusions of their study, that a phased transition from animal agriculture to plant-based diets would lead to significant emission reduction and biomass carbon recovery, delivering a major contribution towards the Paris targets.

**Major Comments**

1) The assumption that soy protein is used to replace all animal protein. If the authors don’t provide results of sensitivity analysis on this assumption, they need to at least acknowledge that the impact would be different under other plant proteins. This assumption could be discussed in the Discussion, when the biomass recovery assumption is discussed. In reality, a range of plant proteins are being used now and would be used under further shifts to plant-based diets. There are of course many regions where other plant protein crops would be favored, biophysically, culturally and socio-economically, over soy protein. Additionally, a proportion of plant-based protein consumed is processed, and would be under a future plant-based scenario, and this will also influence the emissions of a plant-based diet, e.g Santo et al. (2020, Frontiers in Sustainable Food Systems) compares environmental impact of processed plant-based foods to wholefood plant foods. In reality, the assumption that soy protein would replace all animal protein likely overstates the emission reduction of this phased out animal agriculture scenario somewhat.

2) Assumption on biomass recovery. This assumption and uncertainty around it are already discussed (page 13) but this needs expansion, particularly because this is the major driver of the study’s results. The authors should acknowledge other rising pressures on land besides animal agriculture: energy (solar, wind, biomass), urbanization, and climate change (wildfires, drought), all of which are expected to increase this century. My understanding of the methods of Hayek et al. (2021) – and the authors can correct me if they think this is wrong – is that these other pressures on land were not accounted for (though a rising population was). As the purpose here is a self-described simple analysis with clear assumptions, I don’t think the authors necessarily need to model these changes from other land-use pressures - which of course would be very challenging and full of uncertainty - but they should acknowledge that the likely biomass recovery would in reality fall below the potential level they use because of these other pressures, and potentially by a quite a sizeable amount. They could consider a simple sensitivity analysis to show they did consider this in their study, and that the main conclusions remain unchanged, even if, say, a third of that biomass carbon recovery wasn’t realized.

**Other comments**

**Abstract:**

1) This is a study on global phaseout of animal agriculture but ‘global’ isn’t used in the abstract – I think it would add clarity to put it in e.g “a 15 year phaseout of animal agriculture globally”.

2) ‘Via’ on first line, and elsewhere in manuscript – personally, I find this too informal/colloquial – ‘through’ instead?

3) Comma after ‘However’ on 3rd line.

4) Freeze in radiative forcing for 30 years ***from***2030 (for clarity).

5) Offset 70% of ***current*** anthropogenic emissions (for clarity).

**Significance statement:**

1) Use of ‘global-warming’ here but ‘global warming’ elsewhere – consistency needed.

2) “70% reduction in the use of fossil fuels in energy and transportation” – I didn’t come across the methods or references for this calculation in the manuscript?

**Main Text:**

1) 2nd paragraph (references Hristov et al. 2013a ….): other good reference to add: the recent meta-analysis on LCAs of different beef management practices, Cusack et al. (2021, Global Change Biology); Springmann et al. (2018, Nature), who compare multiple means of reducing environmental pressures of the food system; Poore and Nemecek (2018).

‘Sizeable’ is a more appropriate word than ‘some’ at the start of this sentence: other means of achieving food-system emission reductions are not as large as plant-based diets, but they are sizeable and important. I also think the sentence should include “plant-based diets” as an option because it makes more sense when in the next sentence you write “of these options”, and you can then cite the important Poore and Nemecek meta-analysis on plant and animal food emissions in that first sentence.

2) 3rd paragraph: MacLeod et al. (2020) is a study on aquaculture emissions, but this is a point about the GHG reduction of plant-based diets – reference used in error in this sentence? Please also check use of MacLeod et al. (2018) here. I also don’t think the Long Shadow report (Steinfeld et al., 2006) is best used here: it covers the environmental impact of animal agriculture well, but doesn’t quantify the GHG benefits of shifts towards plant-based diets (e.g as done in Poore and Nemecek, 2018, Nature).

3) Livestock emissions are stated as 15% of anthropogenic emissions: the authors cite the Long Shadow report here (Steinfeld et al. 2006) but this report actually cited an 18% figure. However, the methodology used to reach that 18% figure was criticized. Until recently the more commonly accepted figure has been the FAO’s 14.5%, however, Twine (2021, Sustainability) provides a more up-to-date estimate of a minimum of 16.5%. The authors should use the Twine estimate, perhaps also acknowledging the FAO’s 14.5% prior estimate.

The authors calculate that livestock emissions equate to 4 % CO2, 35 % CH4, 66 % N20: I’m curious to know what their own calculations here translate to in terms of a % total CO2eq?

4) End of fifth paragraph: use of ‘rapidly’: this is certainly true of the short-lived atmospheric emissions. I did consider if it was true of biomass recovery: Strassburg et al. (2020) do state that carbon stocks of tropical forests (which make up most of the area of restored ecosystems in their modelling) return to >50% of reference ecosystem carbon stocks in the first 20 years – so I think the authors are justified in the use of ‘rapidly’ here.

5) Page 10: linking of 1.9 and 2.6 Wm-2 to 1.5 DC and 2 DC: this is an important linkage and I think it would be beneficial to the reader if the specific chapter of the IPCC report was cited here.

6) Sentence on page 13 “As slower biomass recovery…..” – this sentence sounds incomplete/doesn’t make sense. Is there supposed to be a comma between the previous sentence and this one perhaps?

7) It is good to see on page 14 consideration to the uneven social and economic impact of this transition.

8) Use of CRFD: this is an important way to look at warming/cooling potential. It isn’t my area of expertise, but I believe this approach is similar/synonymous with the recently introduced GWP\* metric (e.g Allen et al., 2018, Climate and Atmospheric Science)? Given the familiarity with the GWP\* metric, particularly in recent discussions of the warming impact of livestock, I think it would add clarity to the reader if the authors referred to GWP\* when discussing their CRFD approach, and noted the similarities/differences.

9) Use of both ’15 year’ and ’15-year’ in the text (consistency).

10) In the text the authors switch between using ‘%’ and ‘percent’ I’d advise using ‘%’ only.

11) Bottom of page 11, regarding “19% of protein in the human diet” – this needs a reference.

12) Emissions impact comparison to driving: the authors need to be careful making this comparison. In a previous similar comparison, the Long Shadow report was criticized for comparing ‘tailpipe’ transport emissions to emissions from a comprehensive LCA of livestock. A similar comparison is done here, whereby the authors are not considering LCA emissions of driving (car construction, road infrastructure, etc.). I agree it is helpful to make comparisons people are familiar with, and no comparison will be perfect, but the one given here is at risk of being challenged in the same way the Long Shadow report was.

13) Linking back to the major comment 2) in this section, the results shown on page 12 (kg beef -> 470kg CO2eq) are very large, and driven by the biomass recovery estimates: the food meta-analysis of Poore and Nemecek (2018, Nature) found median beef emissions of 50kg CO2eq, without considering biomass recovery. Have the authors looked for any other similar results to compare to their own? And given the very large influence and uncertainty of the biomass recovery (as discussed already in major comment 2)) it may be worth highlighting the contribution of each of these two components on the 470kg CO2eq and related numbers more clearly.

14) Regarding the Sala et al., (2021) estimate: my understanding is that Sala et al. estimate annual emissions of 0.58 Gt CO2, lower than the 1 Gt CO2 used in your analysis – please check this. Is this estimated 120 Gt CO2 (which may now need to be revised on the 0.58 Gt CO2 figure), accounting for the additional soy-protein used to replace that food in the food system, as done on the previous animal product estimates?

15) Top of page 14: the authors could cite Poore and Nemecek (2018, Nature) as supporting this finding from their own study.

16) The authors compared their model outputs to those of Riahi et al. (2017) – which outputs were compared?

17) Bottom of page 14: Regarding the economic and social impacts of a transition away from animal agriculture: a reference here would be good. Food security risk for some regions of the world would also be an issue.

18) The latest IPBES (IPBES, 2019) report would be a good reference to add to the risk to global biodiversity (in addition to existing Newbold et al. and WWF references).

19) “Global plant-only diets are feasible without…..” – at the macro-level, perhaps, but there would be major changes in some regions, which shouldn’t be forgotten.

20) Top of page 16: whilst there are significant risk associated with negative emission technologies (NETs), some IPCC scenarios of their deployment are infeasible, and they remain only in early development/operation, this statement needs tempering somewhat: CCS projects are in existence, and are capturing and storing carbon in the order of millions of tonnes (<https://www.rff.org/publications/explainers/carbon-capture-and-storage-101/>), though this is very far from the scale envisaged in many IPCC scenarios. There are far fewer BECCS projects in operation, and also very few DAC projects: <https://www.iea.org/reports/direct-air-capture>. These technologies are unproven at large-scale, and we still do not know exactly what their potential delivery of negative emissions/capture carbon is. If the authors want to highlight these risks, and suggest that whilst their scenario is infeasible so are many IPCC scenarios for NETs, they could cite Anderson and Peters (2016, Science) here.

**Methods:**

1) Page 19 (‘Estimating species-species land use): no space between ‘in’ and ‘*m*’ (“except for milk which is reported **in*m***^2).

2) Page 24: ‘scitik-learn’ – should make aware that this is a Python software.

3) Page 25: do authors mean ‘2100’ here, not ‘2200’?

4) Page 26: references needed for fuel efficiency and emission intensity, although as I have stated above the authors should consider this comparison.

**Tables:**

Table 1: seems to be a repetition of ‘chickens’ in the table (rows 6 and 7).

**Figures:**

1) Figure 2: there are several double spacings in the figure legend (and it looks like in other figure legends too).

2) Figure 6: the 10.6 km per liter of gas should be referenced.

**Supplementary Figures:**

1) Reference to Figure 2-S1 through 2-S21: For my full understanding of these figures, please could the authors explain why under BAU, atmospheric concentration of CH4 increases decades beyond 2020, when they assume that BAU emissions remain constant at 2019 levels? I would assume the atmospheric concentration to level off sooner, because of the approx. 9-year half-life, but I assume I am misunderstanding something here.

2) Figure 5-S1 through 5-S4: again, for my understanding, the top-left pane shows the CO2 emissions under ‘immediate elimination of animal agriculture’ return to BAU emissions after 30 years. Please can the authors explain this, given this scenario is based on a plant-based food system which is lower annual emissions than the BAU system? I see that the biomass recovery is modelled over 30 years, but after that period you still have a food system operating on lower emissions than BAU?

3) Typos in Figure 5-S5: “the decline **int he** first 30 years” and “**decreasess**”.

**References**

1. Cusack, D. F. *et al.* Reducing climate impacts of beef production: A synthesis of life cycle assessments across management systems and global regions. *Glob. Chang. Biol.* **27**, 1721–1736 (2021).

2. Twine, R. Emissions from animal agriculture—16.5% is the new minimum figure. *Sustain.* **13**, (2021).

3. Santo, R. E. *et al.* Considering Plant-Based Meat Substitutes and Cell-Based Meats: A Public Health and Food Systems Perspective. *Front. Sustain. Food Syst.* **4**, 1–23 (2020).

4. Springmann, M. *et al.* Options for keeping the food system within environmental limits. *Nature* **562**, 519–525 (2018).

5. Allen, M. R. *et al.* A solution to the misrepresentations of CO2-equivalent emissions of short-lived climate pollutants under ambitious mitigation. *npj Clim. Atmos. Sci.* **1**, 1–8 (2018).

6. IPBES. *Summary for policymakers of the global assessment report on biodiversity and ecosystem services of the Intergovernmental Science-Policy Platform on Biodiversity and Ecosystem Services. S. Díaz, J. Settele, E. S. Brondízio E.S., H. T. Ngo, M.* https://ipbes.net/system/tdf/ipbes\_global\_assessment\_report\_summary\_for\_policymakers.pdf?file=1&type=node&id=35329 (2019).

7. Anderson, K. & Peters, G. The trouble with negative emissions. *Science (80-. ).* **354**, 182–183 (2016).