nature portfolio

Peer Review File

Tailoring carbon farming can realise greater co-benefits

Corresponding Author: Professor Matthew Harrison

Parts of this Peer Review File have been redacted as indicated to remove third-party material.

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The article's main objective is to explore the scope of different interventions in Australian sheep farms targeting to reduce their net GHG impact, but analysing potential trade-offs and benefits on other sustainability aspects like profitability and biodiversity. This is done by scenario testing using a range of diverse and real farms across Australia.
I think the study is quite interesting and, in general, the study has been analysed quite appropriately.
I have only minor/moderate points that I would like to suggest that the authors could address:
The study is original, but this is not the first time that researchers attempt to do this type of approach trying to analyse the trade-offs in sustainability attributes after introducing measures to mitigate GHG emissions for example. Evaluating biodiversity changes, for example, in an integrated assessment comprising environment, economics, sustainability has been done before but it is very challenging. Has it been done properly? That is possibly the main question. In this sense, I feel that the authors could expand the past efforts of existing studies on the topic: Whereas it is important that the study refers and compares to other studies in the same context (i.e. Australia, sheep farming and current times). It would be interesting to cite other and older studies that have attempted to study GHG mitigation and associated trade-offs from ruminant production systems in areas other than Australia and ruminant species other than sheep farming (e.g. GHG mitigation +farm modelling +sustainability). (e.g. L131-L135). There are some using farm models for example.
Case study farms vs. generalised typologies: The study uses different case study farms as a sample to attempt to generalise findings on a broader scale. I think that the authors should provide some text on the pros/cons of using this type of approach vs. using a farm typology-based approach, where farm typologies may not necessarily be representing specific and real farms but rather generalise the main structure found on farms in the context.
Methods: the study uses a framework that combines different models/tools and equations. The key question in this type of studies is whether this framework is capable of capturing all of the potential synergies/trade-offs of studied interventions. For some, it is expected to do a good job but it is questionable that this is achieved in all cases. For example, for compost application and omega rich diets: changing to compost management implies several changes in the system including the quality of the C re-applied to the soil, emissions, etc. It is hard to see that an IPCC-based approach that is used for national inventories may be sufficiently robust to, for example, derive changes in GHG emissions. For omega-rich diets, literature suggest that this may lead to changes in enteric CH_4 and consequently, changes in manure CH_4. Has this been captured using IPCC based methodologies? I would suggest that the authors explain the shortcomings of each of the interventions modelling exercise.

Reviewer #2

(Remarks to the Author)
Overall Comment: The manuscript provides a comparative case study of sheep farms near the southern seaboard of Australia to ascertain the efficacy of numerous interventions to simultaneously achieve four objectives: increase productivity, improve profitability, elevate atmospheric carbon sequestration, and enhance biodiversity. To accomplish this goal, the authors examined "29 interventions" across seven farms to determine if any combination of interventions represent a panacea for simultaneously achieving all four objectives. It is unclear from Table 2 how this number of interventions was derived. It appears that six interventions were evaluated across all seven properties: these included planting 10% of the grazing area to "endemic" trees; feeding an enteric CH_4 inhibitor, grazing anti-methanogenic pastures; grazing

management; genetic improvement; and supplementary feedings. Additionally, another eight interventions were evaluated for individual farms or in one intervention (use of grazing cells) on two farms. Due to complex nature of the study, my comments consist mainly of overarching issues. More specific questions and comments are included in the attached copy of the manuscript.

Comment 1: The title seems misleading. It suggests that a panacea might be expected to achieve multiple objectives in complex and diverse farming systems. Simultaneously maximizing multiple objectives across diverse farming operations is never possible; at best one can optimize the outcomes of some interventions subject to the constraints imposed by other interventions. However, the study did not include an optimization analysis. In part, this may have been due to the treatments (interventions) included in the study being unbalance as they were not all applied across all seven case studies.

Comment 2: Farm-scale case study comparisons can provide useful information for other producers to consider with respect to the implementation of management innovations. However, this manuscript is limited because the seven-farm comparison was unbalanced preventing any statistical analysis and therefore not providing any overarching conclusions regarding the efficacy of individual interventions or combinations thereof with respect to the four stated objectives. The report provides only descriptive data and does not include any analysis to determine the statistical power of intervention effects. A statistical assessment could, at least, have been conducted for the six interventions that were included in all case seven studies. The lack of any statistical analysis prevents generalization of the results.

Comment 3: The methods section identifies numerous assumptions in the modelling aspects of the study; however, many (most) of these assumptions are not clearly supported, and no sensitivity analysis was conducted to determine the effect of these assumptions on model outputs.

Comment 4: The authors refer to fertilization as one of the interventions but never state what is being fertilized. It may be that most or all of the pasturage included in the study consisted of irrigated and fertilized planted pastures; however, this is never stated and presumably some of the pastures were native rangeland. As pointed out in the comment on line 759, "Rangelands do not require fertilization and in fact doing so can lead to major species composition shift."

Comment 5: The inclusion of forestation as an intervention is poorly substantiated and seems like a red-herring in the analysis of otherwise production-related interventions. I strongly recommend that forestation is removed from the assessment. Restricting the study to the production-related interventions will enhance the focus and relevance of the manuscript to sheep-farmers given that planting forests reduces their forage base and is widely unpopular. Throughout the manuscript, the authors variously refer to the planted trees as "native" (18 times) and "endemic" (3 times). The terms are not synonymous; it is hardly likely that endemic trees would have been used; by definition, endemic species are limited to specific areas. Additionally, there is a fundamental assumption that planting forests is ubiquitously a good thing and that doing so inevitably enhances biodiversity. As noted in the discussion (lines 650-651): "What about the deleterious aspects of tree planting, including increase fuel loads that feed devastating wildfires and, depending on the revegetation mix, that can decrease biodiversity in savanna ecosystems?" Most Australian ecosystems that contain a substantial number of trees are savannas or open woodlands and not closed canopy forests. Moreover, the suppression of fire in these ecosystems has led to increased woody plant density that has contributed to the increasingly destructive wildfires in recent times. The western European bias that forestation is ubiquitously beneficial has been directly challenged. See: (1) Veldman, J.W., et., al. 2015. Where tree planting and forest expansion are bad for biodiversity and ecosystem services. BioScioence, 65 (10), 1011-1018; https://doi.org/10.1093/biosci/biv118; and (2) Veldman, J.W., et al. 2015. Tyranny of trees in grassy biomes. Science, 347 (6221), 484-484. https://doi.org/10.1126/science.347.6221.484-c.

Comment 6: The long and rambling nature of the paper make it difficult to comprehend its inherent value. This is also due to the lack of a clear statement of the broader implications of the study. As such the study is of limited relevance for other parts of the world where sheep production is important, including but not limited to neighboring New Zealand, South Africa and Argentina.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Dear authors,
I think you have made a good job in managing appropiately my raised concerns/issues. Therefore, I would like to recommend that the article can be published. Best.

Reviewer #2

(Remarks to the Author)
The authors have systematically and comprehensively responded to the previous reviewer comments and have thereby substantially improved the manuscript. However, I still have some concerns that are provided below:

1. Revised title: The revised title is vague and is unlikely to attract much interest from prospective readers. What do the authors mean by “bespoke bottlenecks” and to what do the co-benefits refer. I suggest a title that tells the reader concisely what the manuscript presents. At a minimum the authors should state to what bottlenecks and co-benefits refer. Additionally, bespoke is an antiquated word and could be replaced with a more meangiful term such as identified or apparent.

2. The authors’ continued use “forest planting” as an intervention is concerning for the reason previously stated. They state that planting trees could represent either afforestation or reforestation. While the latter may have validity, a cursory examination of Australian vegetation maps indicates that, except for the Tasmanian case study (Farm S1), the other six case study farms are located in woodland or scrub/heathland biomes. Regardless of their definition of forests, which is not a globally accepted definition in which forests are generally considered to be closed canopy ecosystems consisting of tall trees, the authors are likely to receive less criticism if they simply use “tree planting” throughout the manuscript rather than “planting forests” that are not representative of most Australian woodlands. This would also obviate the need for the statement on Line 226-229: “While we apply the ‘forest’ nomenclature throughout the paper for consistency with scientific literature [This is NOT consistent with all scientific literature], the tree planting intervention may be more intuitively conceptualised as establishment of plantations or woodlands primarily comprising eucalypts and to a lesser extent, acacia species.”

3. Line 140: The authors conflate climatic and vegetation types by stating that “Farms were located in temperate regions rather than rangelands.” Rangelands occur in tropical, subtropical, temperate and even tundra regions. Therefore, the dominant ground cover on the study farms could consist of either native temperate rangelands or improved native and/or exotic pastures. The authors should stipulate the dominant herbaceous cover in each case study description.

4. Line 136 and elsewhere: What is meant by status quo farm systems?

5. In Lines 145-146; 166-167: The statement that “Using a participatory approach, farmers co-designed interventions and co-refined model inputs to validate modelled outputs” is unclear. First, did the seven famers work together or did each farmer work individually with the researchers in this regard? Also, how did the famers validate the modelled outputs? Was this validation based on their opinion or on measured biophysical outputs resulting from each intervention? An opinion-based approach would suggest consensus rather than validation, while the latter would represent actual validation of model outputs. This needs clarification.

6. Lines 172-174; and lines 800-801: The statement that “Several iterations between researchers and case study farmers were conducted to ensure that simulated outputs were accurate representations of baseline conditions” is also confusing. Were the model simulations not conducted to predict intervention effects, which were then compared with the baseline condition? This needs further clarification.

7. Lines 379-381: “Biodiversity was attributed low and high economic values to capture the range in income associated with variable biodiversity prices. Biodiversity was valued as a carbon sequestration co-benefit, assuming either AUD 29 or AUD 65/tonne CO_2eq. sequestered.” How were these two biodiversity values determined. It seems highly problematic to assign such blanket values to biodiversity relative to a single ecosystem service that trees provide, i.e., carbon sequestration. Also, what value was assigned to biodiversity in the remaining 90% of each case study? Biodiversity is not associate just with tress. This needs clarification.

8. Line 464: To what does “most prospective of those examined” mean? Might a better term be “impactful”.

9. Line 561-562: This should be changed to “impacted both carbon sequestration (through growth of trees) and enteric methane (through reduction in animal numbers)”.

10. Figure 9: The include image does not show the content of the figure.

11. Line 615: Include “conditions” after present.

12. Line 654: Change “emanate” to “result in”.

13. Line 665-666: I suggest deleting the statement starting with “comprising low closed/open canopy forests …” for the reasons stated above that these areas were likely woodlands and not forests.

14. Line 670: Why would trees be planted in riparian zones? This often leads to desiccation of riparian zones, especially where trees did not previously occur.

Reviewer #3

(Remarks to the Author)
Major comments
1. This is a paper with good material, but the arguments and written expression is overly complicated, which detracts from the overall impression. At the core is a technical analysis relying on case studies of sheep farms in Australia, and the most useful results are from the comparisons of those farms. As it stands, the manuscript is not really suitable for a journal such as Nature Communications, and would be better targeted to a journal where the results are more relevant, such as an

agricultural economics journal.
2. Title. The title is not very informative about its contents, which means it will be poorly cited. I suggest something more pragmatic, such as ‘Evaluating options to reduce greenhouse emissions and associated co-benefits: case studies of sheep farms in Australia’
3. Abstract, first sentence. This overstates the priorities at the farm level. Farmers are not necessarily worried about reducing greenhouse gas emissions / climate change / biodiversity, apart from dealing with government policies and regulations relating to those issues. Those issues are better viewed as externalities to farm production. This first sentence is okay at the community level, but not at the farm business level. This misunderstanding continues through the paper – why should farmers reduce greenhouse gas emissions / sequester carbon unless there is some direct benefit to them?
4. Page 2, first paragraph. The writing style is not clinical enough – many statements are overblown. Why start with global food security as the main driver when the analysis is about individual sheep farms (perhaps producing wool which is a fibre rather than food)? Most of this first paragraph could be dropped. There are too many normative statements here – ‘must be developed, ‘must be preserved’. Why conclude the paragraph by stating that only solutions that meet all goals (GHG reductions, production, profit, biodiversity) are in scope?
5. Page 2, second paragraph, first sentence. The sentence is a bit pointless. Why say that pathways have existed for decades, but then only give references from 2016? I think what you mean to say is that there is a great deal of knowledge about these issues. My other concern with the arguments in the second and third paragraphs is that there does not seem to be a systematic approach to selecting the factors that are relevant to reducing emissions in the livestock sector in Australia. I think the key reference in this topic is Henry and Eckard (2009).
6. Another reference that seems to be missing is Wiedemann S., Yan M., Henry B. and Murphy, C. 2016 – that is another similar case study in Australia.
7. Page 4, first paragraph, second sentence. This is presumably just for sheep- as there are also major production zones in northern Australia. This sentence suggests that the case studies are representative of the sheep industry in Australia, but I doubt that is true. The attached MLA figure provides a more accurate distribution of sheep in Australia in 2021, which shows that the case studies are not fully representative of the industry.

8. Page 25, Discussion, first paragraph. The term ‘bottleneck’ is problematic, because it is being used to indicate unique solutions at the farm level as distinct from industry or program level solutions. It would be better to begin the discussion with a review of the results in terms of industry or program level interventions, and then discuss more specific farm level interventions after that. It would be better to avoid the use of the term ‘bottleneck’.
9. There does not seem to be enough critical analysis of the outcomes. If the findings are that solutions have to be tailored at each individual farm level, this has large implications for costs and program effectiveness – and would make solutions very difficult to achieve.
10. The discussion section and the conclusions are written from the perspective of the sheep grazing industry in Australia, with an implicit assumption that the case studies are representative of the industry. Yet that is unlikely to be true. Many sheep producing regions in Australia are not represented, the largest farm in the case studies is from Tasmania, which is unusual, and four out of the seven farms are classified as regenerative farms, also very unusual. It would be more accurate to focus the discussion section on an analysis of similarities and differences between the case studies, and then the extent to which that may offer insights to the wider sector in Australia. It would be useful to pitch this paper as a cross-sectional analysis of the sheep industry in Australia, rather than how can the industry reduce GHG emissions on farm.
11. A real weakness of the paper is that there is no key message coming through from the results. It looks a bit like that the authors have assembled seven case studies and have then struggled to find a defining story. It would be more straightforward to have a single research question to provide more purpose to the paper – perhaps something like ‘Are strategies to reduce greenhouse gas emissions relevant across a cross section of sheep farms in Australia?

Henry, B. and Eckard, R. 2009. Greenhouse emissions in livestock systems. Tropical Grasslands, 43, 232-238.
Wiedemann S., Yan M., Henry B. and Murphy, C. 2016. Resource use and greenhouse gas emissions from three wool production regions in Australia. Journal of Cleaner Production DOI: 10.1016/j.jclepro.2016.02.025

Version 2:

Reviewer comments:

Reviewer #3

(Remarks to the Author)
I thank the reviewers for the careful responses to my previous review. I think the paper is much improved, but still have a few remaining comments.
1. In the reviewer response to my suggestions they emphasised that this was a co-design project with sheep producers, and that it is this co-design element that makes the paper so unique. Yet none of the sheep producers are co-authors on the paper, so I question whether co-design process has been as transparent as the authors suggest.
2. Abstract, first sentence. I disagree with the authors’ response about my previous comment here – their claims that industry

wants to make changes indicates to me that they are not talking to a broad enough cross section of farmers. Currently agriculture is exempt from meeting greenhouse targets in Australia, and there are no requirements for red meat producers to report their greenhouse gas emissions. There are certainly producers within agriculture who are interested in the issues, and there are activities to promote voluntary reductions, but the bulk of agricultural producers have little interest in voluntary reductions. As well the small number of sheep producers involved in the study does not suggest major industry interest. So the statements that producers 'need' to mitigate greenhouse gas emissions is too strong, as they are under no current obligations to do so. These and other statements would be better in terms of designing strategies with an eye to the future when requirements may come into place.
3. The paper is written in very academic terms, and the language that is used is not really reflective of a co-design study that has been done in partnership with sheep farmers. I recommend replacing terms like 'manifold priorities' and 'in silico' with simpler language.

Reviewer 1 comments to address

The study is original, but this is not the first time that researchers attempt to do this type of approach trying to analyse the trade-offs in sustainability attributes after introducing measures to mitigate GHG emissions for example. Evaluating biodiversity changes, for example, in an integrated assessment comprising environment, economics, sustainability has been done before but it is very challenging. Has it been done properly? That is possibly the main question.

We address this question in detail below.

In this sense, I feel that the authors could expand the past efforts of existing studies on the topic: Whereas it is important that the study refers and compares to other studies in the same context (i.e. Australia, sheep farming and current times). It would be interesting to cite other and older studies that have attempted to study GHG mitigation and associated trade-offs from ruminant production systems in areas other than Australia and ruminant species other than sheep farming (e.g. GHG mitigation +farm modelling +sustainability). (e.g. L131-L135). There are some using farm models for example.

We agree. In response, we added further background on previous work:

- Smith et al (2022) with respect to the fact that climate mitigation strategies differ widely in their biodiversity co-benefits, in fact some are counterproductive. Smith et al (2022) also suggest that social co-benefits and trade-offs can be best navigated by avoiding narrow goals, taking longer term views and minimising losses of intact ecosystems.
- Veldman et al (2015a; 2015b) in relation to the distinction between reforestation and afforestation (in the discussion) and the downsides of tree planting in areas that were historically grassy biomes (in the introduction).
- Balmford et al (2018) in relation to metrics used to compare farming systems, including their suggestion that multiple externalities alongside yields must be reported if environmental benefits from farming systems interventions are to be fully realised.
- Herrero et al (2016) with respect to the economic potential of managing interventions to livestock farms (less than 10% of technically possible due to adoption constraints, costs and numerous trade-offs), noting that more research and investment are needed to increase the affordability of mitigation practices while avoiding negative impacts on livelihoods, economic activities and the environment.
- Wang et al. (2024) in relation to metrics for appropriate assessment of livestock farming emissions intensity.

References added

Balmford, A., T. Amano, H. Bartlett, D. Chadwick, A. Collins, D. Edwards, R. Field, P. Garnsworthy, R. Green, P. Smith, H. Waters, A. Whitmore, D. M. Broom, J. Chara, T. Finch, E. Garnett, A. Gathorne-Hardy, J. Hernandez-Medrano, M. Herrero, F. Hua, A. Latawiec, T. Misselbrook, B. Phalan, B. I. Simmons, T. Takahashi, J. Vause, E. zu Ermgassen and R. Eisner (2018). "The environmental costs and benefits of high-yield farming." *Nature Sustainability* **1**(9): 477-485.

Herrero, M., B. Henderson, P. Havlík, P. K. Thornton, R. T. Conant, P. Smith, S. Wirsenius, A. N. Hristov, P. Gerber, M. Gill, K. Butterbach-Bahl, H. Valin, T. Garnett and E. Stehfest

(2016). "Greenhouse gas mitigation potentials in the livestock sector." *Nature Climate Change* **6**(5): 452-461.

Smith, P., A. Arneth, D. K. A. Barnes, K. Ichii, P. A. Marquet, A. Popp, H.-O. Pörtner, A. D. Rogers, R. J. Scholes, B. Strassburg, J. Wu and H. Ngo (2022). "How do we best synergize climate mitigation actions to co-benefit biodiversity?" *Global Change Biology* **28**(8): 2555-2577.

Veldman, J. W., G. E. Overbeck, D. Negreiros, G. Mahy, S. Le Stradic, G. W. Fernandes, G. Durigan, E. Buisson, F. E. Putz and W. J. Bond (2015a). "Tyranny of trees in grassy biomes." *Science* **347**(6221): 484-485.

Veldman, J. W., G. E. Overbeck, D. Negreiros, G. Mahy, S. Le Stradic, G. W. Fernandes, G. Durigan, E. Buisson, F. E. Putz and W. J. Bond (2015b). "Where Tree Planting and Forest Expansion are Bad for Biodiversity and Ecosystem Services." *BioScience* **65**(10): 1011-1018.

Wang, T., U. Kreuter, C. Davis and S. Cheye (2024). "Climate impacts of alternative beef production systems depend on the functional unit used: Weight or monetary value." *Proceedings of the National Academy of Sciences* 121(31): e2321245121.

Case study farms vs. generalised typologies: The study uses different case study farms as a sample to attempt to generalise findings on a broader scale. I think that the authors should provide some text on the pros/cons of using this type of approach vs. using a farm typology-based approach, where farm typologies may not necessarily be representing specific and real farms but rather generalise the main structure found on farms in the context.

Agree. We added the following to the discussion:

Several approaches for modelling farming systems are available, ranging from biophysical, bioeconomic, statistical and agent-based (*viz.* Meier et al. 2020; Shahpari et al. 2021; Phelan et al. 2018; Tao et al. 2022; Fischer et al. 2025). Among these, farm typologies – categorisation of heterogeneous farming systems using generalised or domain-specific approaches – have gained much popularity (Eshetae et al. 2024). The typologies conceptualisation aspires to group representative farming systems by categories, such as lifestyle, production system, agroecological environment, income and technology for example (Awoke et al. 2024). While typologies are often established with statistical approaches, they should be guided by contextualised biophysical and/or socio-economic factors (Alvarez et al. 2018). As the typology approach is not conducive to model calibration or validation (ie there are no real farm data for calibration of biomass, profit, vegetation carbon etc), we opted for a case study farm approach. As part of this, we iteratively refined model inputs with end-users until outputs closely represented the biophysical, economic and environmental status quo (*viz.* Bilotto et al. 2025). As our objective was to ascertain change relative to baseline conditions, ground-truthing baseline simulations with real data was essential. We note that the people-centric design we invoked are similar to participatory approaches espoused by those applying the typology paradigm (*viz*. Alvarez et al. 2018). Our farms were chosen for their distinctive agroecological context (e.g. Mediterranean, cool temperate etc) or production system (self-replacing, purchase/sell etc). Given that each farm was strategically selected for its position within the predominant sheep production zones in continent (MLA, 2022), results from our analysis are transferrable to farms with comparable production systems and agroecological regions, and the methods could be generically applied to any industry.

References added

Alvarez, S., Timler, C. J., Michalscheck, M., Paas, W., Descheemaeker, K., Tittonell, P., Andersson, J. A., & Groot, J. C. J. (2018). Capturing farm diversity with hypothesis-based typologies: An innovative methodological framework for farming system typology development. PLoS *ONE*, *13*(5), e0194757. https://doi.org/10.1371/journal.pone.0194757

Eshetae, M. A., Abera, W., Tamene, L., Mulatu, K., & Tesfaye, A. (2024). Understanding farm typology for targeting agricultural development in mixed crop-livestock farming systems of Ethiopia. *Farming System*, *2*(3). https://doi.org/10.1016/j.farsys.2024.100088

Bilotto, F., Christie-Whitehead, K. M., Malcolm, B., Barnes, N., Cullen, B. R., Ayre, M., & Harrison, M. T. (2025). Costs of transitioning to net-zero emissions under future climates. *Nature Communications*. https://doi.org/10.21203/rs.3.rs-2939816/v1

Fischer, K., Vico, G., Röcklinsberg, H., Liljenström, H., & Bommarco, R. (2024). Progress towards sustainable agriculture hampered by siloed scientific discourses. *Nature Sustainability*, *8*(1), 66-74. https://doi.org/10.1038/s41893-024-01474-9

MLA. (2022). *Sheep numbers- as at June 2021*. Meat and Livestock Australia. Retrieved 11/03/2024 from https://www.mla.com.au/globalassets/mla-corporate/prices--markets/documents/trends--analysis/fast-facts--maps/mla_sheep-distribution-map-june-2021_v02-final.pdf

Meier, E. A., Thorburn, P. J., Bell, L. W., Harrison, M. T., & Biggs, J. S. (2020). Greenhouse Gas Emissions From Cropping and Grazed Pastures Are Similar: A Simulation Analysis in Australia. *Frontiers in Sustainable Food Systems*, *3*. https://doi.org/12110.3389/fsufs.2019.00121

Phelan, D. C., Harrison, M. T., McLean, G., Cox, H., Pembletond, K. G., Dean, G. J., Parsons, D., Richter, M. E. D., Pengilley, G., Hinton, S. J., & Mohammed, C. L. (2018). Advancing a farmer decision support tool for agronomic decisions on rainfed and irrigated wheat cropping in Tasmania. *Agricultural Systems*, *167*, 113-124. https://doi.org/10.1016/j.agsy.2018.09.003

Shahpari, S., Allison, J., Harrison, M. T., & Stanley, R. (2021). An Integrated Economic, Environmental and Social Approach to Agricultural Land-Use Planning. *Land*, *10*(4). https://doi.org/10.3390/land10040364

Tao, R., Zhao, P., Wu, J., Martin, N. F., Harrison, M. T., Ferreira, C., Kalantari, Z., & Hovakimyan, N. (2022). Optimizing crop management with reinforcement learning and imitation learning. *arXiv preprint arXiv:2209.09991*.

Methods: the study uses a framework that combines different models/tools and equations. The key question in this type of studies is whether this framework is capable of capturing all of the potential synergies/trade-offs of studied interventions. For some, it is expected to do a good job but it is questionable that this is achieved in all cases. For example, for compost application and omega rich diets: changing to compost management implies several changes in the system including the quality of the C re-applied to the soil, emissions, etc. It is hard to see that an IPCC-based approach that is used for national inventories may be sufficiently robust to, for example, derive changes in GHG emissions.

This comment relates to transparency of, and rationale, for the methods we invoked. We used several assessment frameworks because this was a transdisciplinary study, including researchers, extension agencies, government and industry. This approach is more likely to yield robust outcomes than attempting to constrain the research question to a single framework or conceptualisation. Indeed, the transdisciplinary and participatory co-design aspect is a strength of our study, because we were able to disentangle economic, environmental and biophysical enablers and inhibitors to adoption of interventions to farm systems.

We note that this was a modelling study. Models are simplifications of reality: were they not a simplification, they would not be a model but would be reality. At no point do we profess to capture ALL synergies/trade-offs, nor was that the objective of the study. To improve transparency of our rationale, we revised the introduction to clarify our motivation:

"…the primary aim of this study was to elicit interventions that reduce GHG emissions and improve production, profit and biodiversity…"

And

"*The purpose of this study was not to conduct multi-objective optimisation, but rather to co-design systems interventions for sustainably reducing net farm GHG emissions while benefitting profit, livestock production and biodiversity*"

The compost intervention required substitution of the status quo synthetic mono-ammonium phosphate (MAP) fertiliser with compost. To model this, we deducted Scope 1 (direct emissions) and Scope 3 (indirect upstream and downstream emissions) associated with MAP fertiliser from net farm GHG emissions. For Scope 1 and 3 emissions associated with compost, we derive emission factors from the Australian National GHG Inventory and peer-reviewed field experimentation. We adopted results from Nicholson et al. (2017) for direct and indirect N_2O emissions attributed to nitrate leaching; these sources being the two largest Scope 1 emissions for MAP fertiliser and compost. Scope 3 emissions from compost application were determined using emissions factors from the Australian National GHG Inventory (which are developed by the Australian Government and more appropriate for use in Australia than IPCC emissions factors). Given that (1) the additional GHG associated with compost application was small relative to net farm GHG emissions (30.6 cf. 2,867 t CO_2eq; Table S11), (2) the GHG from compost caused little change in net farm GHG emissions (<5%; Fig 7) and (3) would require profoundly different application rates and emissions factors for the ranking among interventions to change (see panel S6 in Fig 7), we have high confidence that our conclusions are robust.

The omega-3 fatty acid dietary enrichment intervention was similarly grounded on empirical studies which demonstrated that dietary enrichment of ewes during pregnancy can increase the ratio of wether lambs to ewe lambs by 5% (Clayton 2014). Our sensitivity analysis (Figure S5, also shown below) indicates that net farm GHG emissions were insensitive to the change in ratio of wether to ewe lambs, again demonstrating that if our input data were to change significantly, the relative ranking of our interventions would be similar (viz. Fig 6, 7). This demonstrates that our conclusions are robust.

Department of Climate Change, Energy, the Environment and Water (2023a) The Australian Government Submission to the United Nations Framework Convention on Climate Change - Australian National Greenhouse Accounts National Inventory Report. Available at https://www.dcceew.gov.au/climate-change/publications/national-inventory-reports (Accessed 12 Aug 2024).

Department of Climate Change, Energy, the Environment and Water (2023b) Australian National Greenhouse Accounts Factors- for individuals and organisations estimating greenhouse gas emissions. Table 18; Available at https://www.dcceew.gov.au/sites/default/files/documents/national-greenhouse-accounts-factors-2022.pdf (Accessed 12 Aug 2024).

Leech, F.J., Richardson A.E., Kertesz, M.A., Orchard, B.A., Benerjee, S. & Graham, P. (2019) Comparative effect of alternative fertilisers on pasture production, soil properties and soil microbial community structure. *Crop & Pasture Science 70*, 1110-1127. https://doi.org/10.1071/CP19018

Nicholson, F., Bhogal, A., Cardenas, L., Chadwick, D., Misselbrook, T., Rollett, A., Taylor, M., Thorman, R., Williams, J. (2017) Nitrogen losses to the environment following food-based digestate and compost applications to agricultural land. *Environmental Pollution* **228**, 504-518.

For omega-rich diets, literature suggest that this may lead to changes in enteric CH4 and consequently, changes in manure CH4. Has this been captured using IPCC based methodologies? I would suggest that the authors explain the shortcomings of each of the interventions modelling exercise.

We did not use IPCC methods. We used Australian National GHG Inventories (see above) and clarify this aspect in the methods. We assumed that omega-3 dietary enrichment reduced enteric CH_4 emissions by 10-25% following empirical evidence in the literature (e.g. https://www.ocl-journal.org/articles/ocl/pdf/2015/06/ocl150022.pdf). For the 59 day period ewes were supplemented while pregnant, this equated to a reduction of 14-29 t CO_2e compared with net farm GHG of 6,885 t CO_2eq. Manure methane GHG emissions are a generally small proportion of total farm GHG emissions; in this case, manure CH_4 comprised 2.7% of farm emissions (186 t CO_2eq). Reducing manure or enteric methane in account of the effect of omega-3 enrichment during ewe pregnancy has *de minimus* effect on net farm GHG emissions. This result is further supported by the sensitivity analysis below.

We added the above to the manuscript.

Reviewer 2 comments to address

Overall Comment: The manuscript provides a comparative case study of sheep farms near the southern seaboard of Australia to ascertain the efficacy of numerous interventions to simultaneously achieve four objectives: increase productivity, improve profitability, elevate atmospheric carbon sequestration, and enhance biodiversity. To accomplish this goal, the authors examined "29 interventions" across seven farms to determine if any combination of interventions represent a panacea for simultaneously achieving all four objectives. It is unclear from Table 2 how this number of interventions was derived. It appears that six interventions were evaluated across all seven properties: these included planting 10% of the grazing area to "endemic" trees; feeding an enteric CH4 inhibitor, grazing anti-methanogenic pastures; grazing management; genetic improvement; and supplementary feedings. Additionally, another eight interventions were evaluated for individual farms or in one intervention (use of grazing cells) on two farms.

This is a good observation. We have clarified the terminology: we examined 29 intervention *scenarios* and 13 individual interventions. See Table 1 in the manuscript and revisions thereto (rows depict interventions; numbers in parenthesis were added to clarify each intervention scenario).

Due to complex nature of the study, my comments consist mainly of overarching issues. More specific questions and comments are included in the attached copy of the manuscript.

Comment 1: The title seems misleading. It suggests that a panacea might be expected to achieve multiple objectives in complex and diverse farming systems. Simultaneously maximizing multiple objectives across diverse farming operations is never possible; at best one can optimize the outcomes of some interventions subject to the constraints imposed by other interventions. However, the study did not include an optimization analysis. In part, this may have been due to the treatments (interventions) included in the study being unbalance as they were not all applied across all seven case studies.

We revised the title to "*Carbon dioxide removal practices that address bespoke bottlenecks realise the greatest co-benefit*", which better reflects one of the key discoveries of the paper.

The aim of the paper was not optimisation of multiple objectives. It was to co-develop and contrast interventions for improving profit, food production and biodiversity while reducing GHG emissions. We have clarified this in the introduction with the statement

"*…the primary aim of this study was to elicit interventions that reduce GHG emissions while also having co-benefits for production, profit and biodiversity*".

Comment 2: Farm-scale case study comparisons can provide useful information for other producers to consider with respect to the implementation of management innovations. However, this manuscript is limited because the seven-farm comparison was unbalanced preventing any statistical analysis and therefore not providing any overarching conclusions regarding the efficacy of individual interventions or combinations thereof with respect to the four stated objectives. The report provides only descriptive data and does not include any analysis to determine the statistical power of intervention effects. A statistical assessment could, at least, have been conducted for the six interventions that were included in all case seven studies. The lack of any statistical analysis prevents generalization of the results.

Outputs from biophysical models cannot be compared through statistical imputation because they are computed using deterministic equations. We further note that neither comparison of the farms per se, nor the significance of differences in GHG mitigation in the same interventions across farms was the purpose of the paper. Here our primary aim was to compare interventions in terms of changes evoked *relative to the baseline system*. Our results (Figs 6, 7 and 8) show changes *relative to the baseline* of each farm.

Statistical comparison of the effects of each intervention across farms would be fundamentally flawed because all farms have different status quo. They are not starting from a the same initial condition (e.g. a sterile petri dish in a laboratory): they are real systems with existing states that encapsulate all of the historical management, economic, agronomic and environmental history leading up to the study. This is why our primary metric for assessing results are *changes relative to baseline conditions* (Figs 6, 7, 8 and 9).

To address uncertainty associated with each intervention, we conduct multiple scenarios for each intervention (e.g. low and high enteric CH_4 mitigation, multiple lambing times, alternative ewe culling dates, low and high lamb liveweight gains etc as shown in Table 1). We also conducted a sensitivity analysis for those interventions to further demonstrate variability associated with input assumptions (see below).

Comment 3: The methods section identifies numerous assumptions in the modelling aspects of the study; however, many (most) of these assumptions are not clearly supported, and no sensitivity analysis was conducted to determine the effect of these assumptions on model outputs.

We disagree that our assumptions were not clearly supported. In fact, all assumptions are either hinged upon empirical data from peer-reviewed literature or, where peer-reviewed evidence was not available, assumptions are supported with personal communications with disciplinary experts. Our assumptions are transparently shown in Table 1 and discussed further in the methods.

The statement that sensitivity analysis were not conducted indicates that the reviewer did not understand the methods, perhaps because they were too brief. In response, we elaborate in the methods text, tables and in the supplementary information. Many of the interventions were modelled to reveal how variation in net GHG emissions varied with plausible ranges that could be used for each intervention. For example, variability associated with feeding a genetic enteric methane inhibitor was conducted by assessing low and high plausible mitigation in enteric methane: we adopted 18% and 37% enteric methane inhibition based on empirical evidence (Honan et al., 2022; Hodge et al., 2024). This range captures the realistic low and high mitigation of enteric methane and was part of the original submission. Interventions relating to lamb liveweight gain, enteric CH_4 reduction with pastures, grazing area with exclusion fencing and lambing times were similarly assessed using multiple scenarios (second column in Table 1). However, in an effort to further elaborate, we added text to Table 1 and the paper, including the distinction between scenarios.

We acknowledge that some interventions did not contain scenario analysis. As such, we conducted further analyses. To standardise perturbation across interventions (the purpose of sensitivity analysis being comparison of relative variation across metrics with a consistent deviation), we examined the change in net farm GHG emissions associated with 50% reduction in each intervention. Fig. S5 (below) shows mean percentage change in net farm GHG emissions. The analysis indicates that planting trees was the most sensitive intervention, followed by the 37% and 18% feed additive interventions. These results can be explained by the fact that the tree planting intervention impacted on both carbon sequestration (through tree growth) and enteric CH_4 (through reduced animal numbers). The sensitivity associated with the 37% and 18% enteric CH_4 inhibitor interventions is also sensible considering enteric CH_4 often dominates farm GHG emissions profiles.

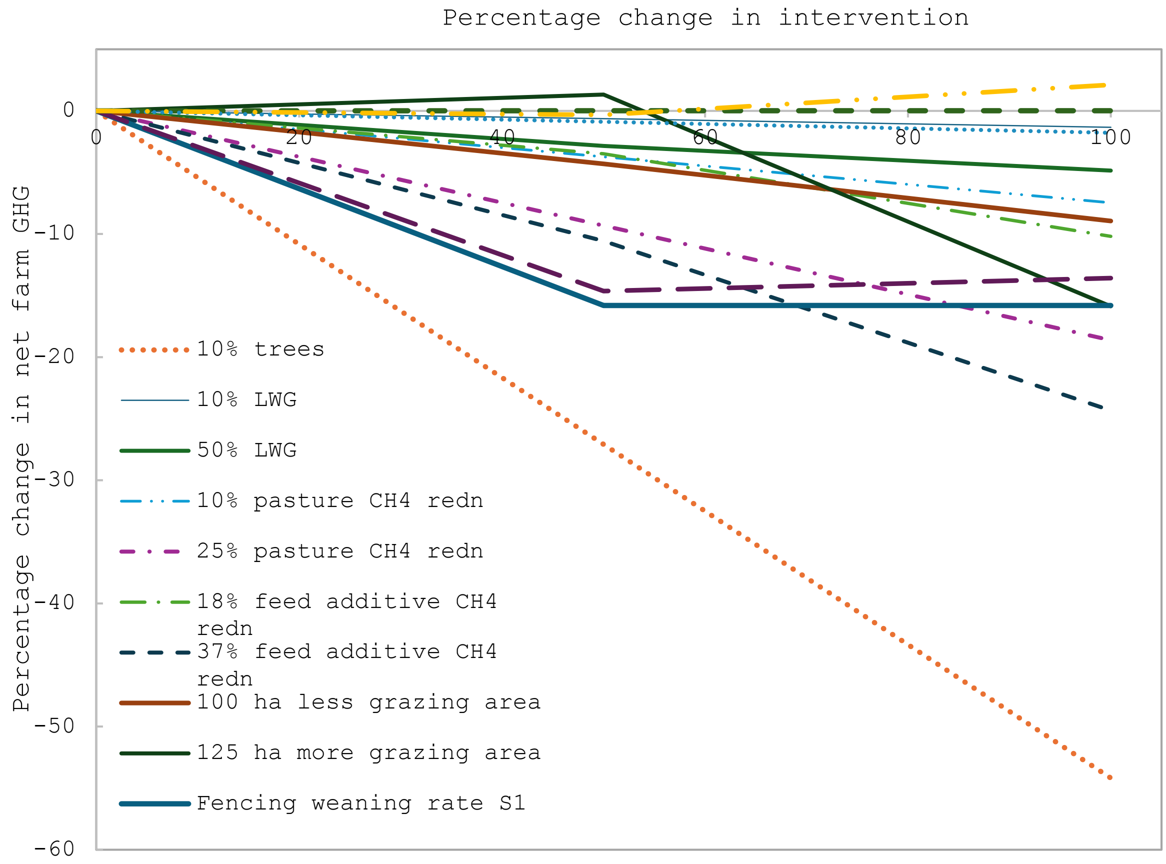

Figure S5: Percentage change in intervention versus percentage change in net farm GHG emissions. One hundred percent change in intervention corresponds to intervention values in Table 1. For all interventions except those ending in the farm abbreviation (ie the five interventions at the bottom of the legend), *n* = 7; for other interventions, *n* = 1.

Comment 4: The authors refer to fertilization as one of the interventions but never state what is being fertilized. It may be that most or all of the pasturage included in the study consisted of irrigated and fertilized planted pastures; however, this is never stated and presumably some of the pastures were native rangeland. As pointed out in the comment on line 759, "Rangelands do not require fertilization and in fact doing so can lead to major species composition shift."

This intervention included replacement of synthesis inorganic nitrogen fertiliser with organic compost and was specific to one farm. This was shown in Table 1, but we have included more clarity around this in several places of the paper.

Consistent with the majority of the Australian sheep population (https://www.mla.com.au/globalassets/mla-corporate/prices--markets/documents/trends--analysis/fast-facts--maps/mla_sheep-distribution-map-june-2021_v02-final.pdf), none of the farms were located in the rangelands. We clarify this in section 2.1.

Comment 5: The inclusion of forestation as an intervention is poorly substantiated and seems like a red-herring in the analysis of otherwise production-related interventions. I strongly recommend that forestation is removed from the assessment. Restricting the study to the production-related interventions will enhance the focus and relevance of the manuscript to sheep-farmers given that planting forests reduces their forage base and is widely unpopular.

The request to remove trees and focus on “production-related” interventions implies misunderstanding of the objective of this work. We intentionally include **both** carbon dioxide removals (sequestration in woody vegetation and in soils) and mitigation interventions (avoidance of enteric methane and reduction of nitrous oxide emissions) in the study. We note that carbon sequestration interventions were reflected in not just planting trees, but also in the improvement of soil organic carbon (SOC) arising from gains in pasture production from several interventions.

We disagree that planting trees is “widely unpopular”. All case study farmers had planted trees prior to the analysis; many expressed the biodiversity benefits arising from historical tree plantings on their farms. Our extension demonstrated significant interest from the scientific community in learning about strategic planting of trees, evidenced by over 31,000 views of our webinar in less than six months: Planting trees on farm: implications for biodiversity and natural capital. We agree that planting trees reduces productive pasture area and we include this trade-off in the study design, because co-benefits and trade-offs arising from mitigation interventions was the key motivation of this work.

We note that the term “forest” could evoke perceptions of rainforests with dense canopies that all but shade out the understory. This is not the case here. As noted in the introduction, “planting trees for farm-level carbon removals can occur via several avenues, including silvopasture, agroforestry, shelter belts, plantations and/or remnant vegetation conservation and/or restoration”. While we adopt the term ‘forest’ for consistency with the scientific literature, it should be noted that ‘forests’ as defined here comprise all woody canopies with more than 20% canopy cover over 2.0 metres in height appearing at least three times over the simulation. This definition aligns with the National Forest and Sparse Woody Vegetation Data (Version 6.0, 2021) and was articulated in section 2.4 of the methods. To clarify, we added the following to the methods:

“…*while we use ‘forest’ nomenclature throughout the paper for consistency with scientific literature, tree planting in this study may be more intuitively conceptualised as the establishment of farm plantations, woodlands or shrublands consistent with the aforementioned definition of forests…*”

Throughout the manuscript, the authors variously refer to the planted trees as "native" (18 times) and "endemic" (3 times). The terms are not synonymous; it is hardly likely that endemic trees would have been used; by definition, endemic species are limited to specific areas.

We replaced endemic with native species throughout the manuscript.

Additionally, there is a fundamental assumption that planting forests is ubiquitously a good thing and that doing so inevitably enhances biodiversity. As noted in the discussion (lines 650-651): "What about the deleterious aspects of tree planting, including increase fuel loads that feed devastating wildfires and, depending on the revegetation mix, that can decrease biodiversity in savanna ecosystems?" Most Australian ecosystems that contain a substantial number of trees are savannas or open woodlands and not closed canopy forests. Moreover, the suppression of fire in these ecosystems has led to increased woody plant density that has contributed to the increasingly destructive wildfires in recent times. The western European bias that forestation is ubiquitously beneficial has been directly challenged. See:

(1) Veldman, J.W., et., al. 2015. Where tree planting and forest expansion are bad for biodiversity and ecosystem services. BioScience, 65 (10), 1011-1018; https://doi.org/10.1093/biosci/biv118; and (2) Veldman, J.W., et al. 2015. Tyranny of trees in grassy biomes. Science, 347 (6221), 484-484. https://doi.org/10.1126/science.347.6221.484-c.

This is a fair argument. We added the following to the discussion:

Planting trees on farm may not emanate beneficial outcomes. Planting trees can increase fuel loads that feed devasting bushfires, which are becoming increasingly prevalent in southern Australia with the changing climate (Canadell et al. 2021) and severely impact the abundance/occurrence of native taxa (Driscoll et al. 2024). Veldman et al (2015a) contend that afforestation and reforestation should be clearly distinguished; the former being tree planting on lands that were not historically forest, the latter being the planting of trees on deforested lands. Veldman et al (2015b) opine that afforestation on grasslands, savannas and open-canopy woodlands can severely compromise ecosystems services such as hydrology, sunlight availability and soil nutrient cycles, devastating biodiversity and ecosystems services. We note that prior to European settlement in Australia, our case study regions likely supported native woody vegetation (Geoscience Australia, 2018; Municipal Association of Victoria, 2018) comprising low closed/open canopy forests and tall closed/open canopy shrubs (2-10 metres tall). While this suggests that our tree planting intervention would be classified as reforestation, we do not advocate *en masse* planting of trees on farmlands, regardless of whether the objective is reforestation or afforestation. Rather, we suggest that tree planting occur only with careful planning, on regions with low pasture productivity, along fence lines, or within remnant vegetation or riparian zones. Connecting remnant vegetation across farms can create wildlife corridors, which facilitate biodiversity movement across landscapes (Brudvig et al. 2009; Travers et al 2021). Regardless of whether or not trees are planted, judicious management of detrimental species – such as noxious weeds and feral animals - remains necessary to prevent land degradation and competition with native species for resources.

Reference added

Brudvig, L. A., Damschen, E. I., Tewksbury, J. J., Haddad, N. M., & Levey, D. J. (2009). Landscape connectivity promotes plant biodiversity spillover into non-target habitats. *Proceedings of the National Academy of Sciences*, *106*(23), 9328-9332. https://doi.org/10.1073/pnas.0809658106

Canadell, J. G., Meyer, C. P., Cook, G. D., Dowdy, A., Briggs, P. R., Knauer, J., Pepler, A., & Haverd, V. (2021). Multi-decadal increase of forest burned area in Australia is linked to climate change. *Nature Communications*, *12*(1). https://doi.org/10.1038/s41467-021-27225-4

Driscoll, D. A., Macdonald, K. J., Gibson, R. K., Doherty, T. S., Nimmo, D. G., Nolan, R. H., Ritchie, E. G., Williamson, G. J., Heard, G. W., Tasker, E. M., Bilney, R., Porch, N., Collett, R. A., Crates, R. A., Hewitt, A. C., Pendall, E., Boer, M. M., Gates, J., Boulton, R. L., . . . Phillips, R. D. (2024). Biodiversity impacts of the 2019-2020 Australian megafires. *Nature*, *635*(8040), 898-+. https://doi.org/10.1038/s41586-024-08174-6

Geoscience Australia. (2018). *Natural vegetation – Pre-European settlement (1788) (Edition 2)*. https://d28rz98at9flks.cloudfront.net/42280/42280_00_0.jpg

Municipal Association of Victoria. (2018). *Simplified native vegetation map of Victoria - pre 1750*. MAVH. https://www.aph.gov.au/DocumentStore.ashx?id=abd52915-81ae-4fc4-8ee3-2ece6e58cb43

Travers, E., Härdtle, W., & Matthies, D. (2021). Corridors as a tool for linking habitats ? Shortcomings and perspectives for plant conservation. *Journal for Nature Conservation*, *60*. https://doi.org/10.1016/j.jnc.2021.125974

Veldman, J. W., Overbeck, G. E., Negreiros, D., Mahy, G., Le Stradic, S., Fernandes, G. W., Durigan, G., Buisson, E., Putz, F. E., & Bond, W. J. (2015a). Tyranny of trees in grassy biomes. *Science*, *347*(6221), 484-485. DOI: 10.1126/science.347.6221.484-c

Veldman, J. W., Overbeck, G. E., Negreiros, D., Mahy, G., Le Stradic, S., Fernandes, G. W., Durigan, G., Buisson, E., Putz, F. E., & Bond, W. J. (2015b). Where Tree Planting and Forest Expansion are Bad for Biodiversity and Ecosystem Services. *Bioscience*, *65*(10), 1011-1018. https://doi.org/10.1093/biosci/biv118

Comment 6: The long and rambling nature of the paper make it difficult to comprehend its inherent value. This is also due to the lack of a clear statement of the broader implications of the study. As such the study is of limited relevance for other parts of the world where sheep production is important, including but not limited to neighboring New Zealand, South Africa and Argentina.

The manuscript documents a multi-region, multi-intervention transdisciplinary study. Ensuring that the methods are transparent, the claims defensible and interpretations are logical cannot be done in a superficial manner. Indeed, many of the questions from this reviewer highlighted a need for more text in the methods. Despite this, we have revised much of the manuscript with a mind to (1) transparency, (2) logic and (3) brevity.

We disagree with the comment that there is a lack of clarity of the broader implications of the study. The first section of the discussion clearly underlines the economic, environmental and biophysical benefits that arise when interventions target a bespoke deficit, which is the main conclusion of the study. This key message is repeated in the abstract and conclusions. We are concerned that the reviewer has overlooked this message on multiple occasions. This message has implications for any innovation purported for farmland, regardless of production system, sector or agroecological region.

Replies to edits by Reviewer 2 on manuscript PDF file

Line 1: We revised the title to *Carbon dioxide removal practices that address bespoke bottlenecks realise the greatest co-benefit*

Line 23-48: This text has been removed from the abstract and is clarified in the methods: we examined 29 intervention *scenarios* and 13 individual interventions. See Table 1 in the manuscript and revisions thereto (rows depict interventions; numbers in parenthesis were added to clarify each intervention scenario).

Line 49: Removed keywords also in title

Lines 53-65: amended as suggested, except for “and yet land area is finite”, because the purpose of the first paragraph is to highlight tensions land managers face in prioritising sustainability indicators.

Lines 66-74: Removed this paragraph in response to the reviewer request above that the text was too long.

L76: changed to “agriculture, forestry and land-use sectors”

Line 79: Added reviewer suggestion for citation to Wang, T., U. Kreuter, C. Davis and S. Cheye (2024). "Climate impacts of alternative beef production systems depend on the functional unit used: Weight or monetary value." Proceedings of the National Academy of Sciences **121**(31): e2321245121.

Line 89: reworded to 'which impacts long-term adoption, or lack thereof'

Line 91: added appropriate citations

Line 102: amended as suggested

Line 109: amended as suggested

Line 112: amended as suggested

Line 114: amended as suggested

Line 116-119: we thank the reviewer for their suggestion but have chosen to retain the text as it was originally presented as the focus of those sentences was carbon removals (which the reviewer deleted).

Line 120: amended as suggested

Line 125-126: have amended to "Despite such potential, perceptions towards the protection, conservation or planting of trees on farm polarise audiences, with antagonists contending that trees reduce pasture production"

Line 131: altered to While 'much research has focussed'

Line 137: Amended sentence to start with We

Line 139: Amended start of the sentence to "Using people-centred design with industry stakeholders…"

Line 153: amended as suggested

Line 163: The reviewer commented 'there is no prior discussion on the modelling platform and the accuracy of it in predicting outputs'. The paragraph following this comment in the methods contained the said discussion, but to further clarify, we added the following:

GrassGro simulations of monthly pasture growth rates, stocking rates and liveweight production were validated with each case study farmer using the participatory approach described in section 2.1. Maximum root depth was adjusted to ensure that seasonal pasture growth rates reflected those required to support liveweight gain and annual production at baseline stocking rates (Table S1). Other than maximum pasture root depth, no calibration was conducted, as many studies have demonstrated the accuracy of seasonal pasture growth and liveweight dynamics simulated by GrassGro (viz. Clark et al. 2000; Harrison et al., 2014a; McPhee et al., 2024). This approach reduced uncertainty associated with parameter calibration. Several iterations between researchers and case study farmers were conducted to ensure that simulated outputs were accurate representations of baseline conditions.

Line 177: 'Joining' refers to the time that rams are joined with ewes. We changed this to 'mating and weaning periods'.

Line 178: amended with "(i.e. stocking rates, triggers for rest including available pasture biomass or liveweight gain)"

Line 179: removed etc and replaced with date(s) and shearing time

Line 181: sentence has been removed in line with additional text added in response to comment on line 163

Line 185: replaced with were

Line 186: 'analytical' refers to 'analysis period' as distinct from the initialisation period. This is less subjective that 'prediction'.

Line 187: citation added

Line 230: removed "… from results shown hereto"

Line 232-234: The reviewer highlighted text, but did not comment. We presume that this comment was related to the definition of "forest". To clarify, we added the following to section 2.4:

We define 'forest' as those regions containing woody vegetation (i.e. trees, shrubs and understorey species) in the National Forest and Sparse Woody Vegetation Data (Version 6.0 2021) with more than 20% canopy cover and at least 2.0 metres in height at least three times over the simulation period. This definition is consistent with that of the Australian Government (Department of Agriculture, Fisheries and Forestry [DAFF], 2016). While we apply the 'forest' nomenclature throughout the paper for consistency with scientific literature, the farm tree planting intervention may be more intuitively conceptualised as establishment of plantations or woodlands primarily comprising eucalypts and to a lesser extent, acacia including Racosperma (Geoscience Australia, 2018).

Line 242: Added:

This quantum was deemed appropriate after scenario analyses which showed that forests initialised with more than 80% biomass resulted in excessively conservative simulated forest biomass (simulated biomass at tree maturation was much lower than that in the National Forest and Spare Woody Vegetation Data).

Line 247: Amended

Line 255: Reworded to:

Habitat condition shown in Fig. 2 represents the capacity of that pixel to support native species occurring in 1750, prior to European settlement (Department of Climate Change Energy the Environment and Water, 2024).

Line 269: Amended with

…where habitat condition is 50% or greater…

Line 280: amended as suggested

Line 291: The 10% value was agreed by case study farmers as a realistic percentage of grazable area that could be planted with trees, assuming the objective was to reduce farm GHG emissions by carbon removals from trees. We assumed trees were planted on grazing land for conservatism, with commensurate reduction in associated inputs. It was also important that the percentage of grazing area planted with trees was standardised across farms to avoid confounding comparisons of carbon sequestration with tree area planted. We have amended the text in section 2.6 in this vein.

Line 291-296: This section has been altered as follows:

In many cases, we examined multiple scenarios for each intervention to elucidate sensitivity of that intervention on net farm GHG emissions. Details of and rationale underpinning each intervention are provided in Table 1. In brief, thematic interventions applied to all farms included (1) planting native tree species on 10% of grazing land (the grazing area percentage that case study farmers agreed was a realistic target for farm carbon removals); (2) using a feed supplement enteric CH_4 inhibitor provided to weaned sheep to achieve either low (18%) or high (37%) CH_4 reduction based on empirical measurements using low-emissions feed supplementation in grazing contexts (Hodge et al., 2024; Honan et al., 2022); (3) renovation of pastures with anti-methanogenic ecotypes to achieve either low (10%) or high (25%) CH_4 inhibition based on values reported in peer-reviewed literature (Badgery et al., 2023); (4) improved grazing management and/or livestock genetic improvement to realise either low 10% (Alcock et al., 2015; Harrison et al., 2014b) or high 50% improvement in liveweight gain from birth to sale for all non-replacement animals. The 50% liveweight gain intervention was deliberately implemented as a stretch target to elicit the upper bound of enteric CH_4 avoidance from selling juvenile animals earlier, should this be achieved in practice. Farm-specific interventions co-designed with case study farmers included adoption of confinement feeding to preserve ground cover by feeding animals in feedlots during dry periods, replacing synthetic inorganic nitrogen fertiliser with organic compost spread on grazed pastures, adopting cell grazing (high intensity, short duration grazing) and diet manipulation to increase omega-3 delivering to increase ratio of wether to ewe lambs (Table 1).

Line 301: this was only for one farm. The revised text reads

"*replacing inorganic nitrogen fertiliser with compost spread on grazed pastures*".

Line 303: Amended as suggested

Line 306: Amended as suggested

Line 307: Amended as suggested

Line 308: See response to L232-234

Line 318: See response to L232-234

Line 335: Amended as suggested

Line 338: representing a plausible future price of AUD 100/tonne CO_2eq. (Stern & Stiglitz, 2017)

Line 340: removed and clarified with additional text in Table 1 re compost intervention for farm S6.

Line 370: amended as suggested

Line 373: Added:

Biodiversity was attributed low and high economic values to capture the range in income associated with variable biodiversity prices. Biodiversity was valued as a carbon sequestration co-benefit, assuming either AUD 29 or AUD 65/tonne CO2eq. sequestered (Carbon Neutral, 2022).

Line 418: Amended to *meat plus wool protein*

Line 438: Amended to the text below. We note that this text again relates to one of the key messages of the manuscript.

Across thirteen interventions (see rows in Table 1), no single intervention simultaneously reduced GHG while concurrently improving profit, production and biodiversity

Line 441: Amended (the journal submission process did this during conversion to PDF).

Line 442 (Table 2): Amended

Line 442 (Table 2): Amended. Includes meat plus wool. We also added another row wither protein sold per hectare per year (also expressed in meat plus wool).

Line 452 (Fig. 5): The right hand axis is expressed in AUD. It is possible that the reviewer reviewed this figure under low resolution.

Line 459: amended to enterprise profit over livestock production or GHG reductions

Line 481: amended to "*the most prospective among those examined*"

Line 498: amended to "…*by higher incomes derived from the reduction of enteric CH_4*"

Line 510: amended to "*as input costs were more compensated by higher income from reduction of enteric CH_4 compared with low prices*"

Line 514 (Fig. 7): Border sizes of bubbles in Fig 7 increased.

Line 548-549: amended to AUD

Line 565: Amended to:

"Substituting pasture fertilisation from inorganic synthetic fertiliser (mono-ammonium phosphate) to organic compost for farm S6 (VIC) was detrimental for emissions abatement, production and GM"

Line 573: replaced EI with emissions intensity throughout manuscript

Line 591: Reviewer asked how effective habitat was measured. This is stated in the methods (effective habit represents total habitat area available for biodiversity, accounting for habitat condition in each location (grid cell) and farm area).

Line 618-620: Reviewer 2 highlighted this text but did not comment. We assume this related to their main comments. See above.

Line 629: removed 'browsing' and reworded as suggested (*grazing wildlife*)

Line 632-633: text has been removed.

Line 637: Amended to "…*No two farms are identical, as their current status reflects the cumulation of ecological, climatic, economic and management factors leading up to the present …*".

Line 643-644: Added:

"…*By the same token, potential for improvement could be enumerated by comparing the lowest and highest performers within a given agroecological region and enterprise type, perhaps using a typologies approach (viz. Alvarez et al. 2018; Fischer et al. 2024). Elite performers are also more likely to have information on those practices or technologies that underpin their historical trajectory of improvement*…"

Line 648: Amended

Line 650-651: See response to Reviewer 2 main comment that trees are ubiquitously a good thing (page 10 above).

Line 658: amended to ‘native’ throughout paper.

Line 661: sentence removed

Line 674-675: Amended to:

“As well, integrating perennial legumes (Badgery et al. 2023) and provision of shelter and shade (Bilotto et al. (2025) may yield livestock productivity co-benefits through improved liveweight gain and/or reduced exposure to climatic elements, respectively.”

Line 680: Added “but also may result in additional trade-offs”

Line 685: Added “*…and aligning stocking rates with long-term environmental conditions…*”

Line 688: sentence has been removed to reduce length of discussion

Line 694: amended to “*for alignment with legislation or from societal pressure*”

Line 711: joining refers to mating. We have clarified this throughout paper.

Line 716: Amended to

“*…simpler to implement and more reversible compared with other interventions examined here (e.g. planting trees or renovating pasture specie*s)…”

Line 719: amended

Lines 733-737: See main response to Reviewer 2 commentary on planting trees above.

Line 759: This section has been removed to reduce length of discussion

Line 772: Revised to “*Farmers often have intimate knowledge of their production system and can be a key source of innovation*”

Line 784-789: Our experience with case study farmers clearly demonstrates a focus on profit and sustainability, because their farm is the asset sustaining their livelihoods. We added:

“*Our experience opposes widespread generalisations that all farmers prioritise short-term profits that degrade ecosystems over time (viz. Matson et al. 1997). This may be because such perceptions often emanate from studies that implicitly centre on cropping systems (including regular use of chemical pesticides, irrigation, mechanisation) or deforestation. Indeed, some authors have suggested that ecosystems losses associated with agricultural land-use expansion tends to be constrained to a few global hotspots, such as the Amazon and Afrotropics (Kehoe et al. 2017).*”

Line 801-803: Title, and native species terminology have been revised.

Line 815: Revised to “*We uncovered many trade-offs and co-benefits associated with carbon removals and reductions. Planting native species significantly reduced net farm GHG emissions, but carbon removals diminished over time and were reversible (e.g. due to loss of trees), which has implications for farms aspiring to achieve net-zero status*”

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

Dear authors,

I think you have made a good job in managing appropriately my raised concerns/issues. Therefore, I would like to recommend that the article can be published. Best.

We thank Reviewer 1 for their positive feedback on our manuscript.

Reviewer #2 (Remarks to the Author):

The authors have systematically and comprehensively responded to the previous reviewer comments and have thereby substantially improved the manuscript.

We appreciate the insightful comments from Reviewer 2.

1. Revised title: The revised title is vague and is unlikely to attract much interest from prospective readers. What do the authors mean by “bespoke bottlenecks” and to what do the co-benefits refer. I suggest a title that tells the reader concisely what the manuscript presents. At a minimum the authors should state to what bottlenecks and co-benefits refer. Additionally, bespoke is an antiquated word and could be replaced with a more meaningful term such as identified or apparent.

One of the key conclusions of our study is that targeting areas of deficit—such as degraded land or excessive feral and domestic grazing pressure—tends to realise the greatest improvements in production, biodiversity and/or profitability. Put another way, those indicators most in need of attention elicit the greatest benefits when they are addressed. We use “co-benefits” in reference to non-climate benefits associated with practice change aimed at GHG emissions mitigation, consistent with previous literature (Bustamante et al. 2014; Cohen et al. 2021; Luderer et al. 2019). We feel that reflecting this conclusion in the title will be central to the impact of the paper. As such, we revised the title to:

Tailoring sheep farming greenhouse gas emissions interventions can realise greater co-benefits

Bustamante, M., Robledo-Abad, C., Harper, R., Mbow, C., Ravindranat, N.H., Sperling, F., Haberl, H., de Siqueira Pinto, A. and Smith, P. (2014), Co-benefits, trade-offs, barriers and policies for greenhouse gas mitigation in the agriculture, forestry and other land use (AFOLU) sector. Glob Change Biol, 20: 3270-3290. https://doi.org/10.1111/gcb.12591

Cohen, B., A. Cowie, M. Babiker, A. Leip and P. Smith (2021). "Co-benefits and trade-offs of climate change mitigation actions and the Sustainable Development Goals." *Sustainable Production and Consumption* **26**: 805-813.

Luderer, G., Pehl, M., Arvesen, A. *et al.* Environmental co-benefits and adverse side-effects of alternative power sector decarbonization strategies. *Nat Commun* **10**, 5229 (2019). https://doi.org/10.1038/s41467-019-13067-8

2. The authors’ continued use “forest planting” as an intervention is concerning for the reason previously stated. They state that planting trees could represent either afforestation or reforestation. While the latter may have validity, a cursory examination of Australian

vegetation maps indicates that, except for the Tasmanian case study (Farm S1), the other six case study farms are located in woodland or scrub/heathland biomes. Regardless of their definition of forests, which is not a globally accepted definition in which forests are generally considered to be closed canopy ecosystems consisting of tall trees, the authors are likely to receive less criticism if they simply use "tree planting" throughout the manuscript rather than "planting forests" that are not representative of most Australian woodlands. This would also obviate the need for the statement on Line 226-229: "While we apply the 'forest' nomenclature throughout the paper for consistency with scientific literature [This is NOT consistent with all scientific literature], the tree planting intervention may be more intuitively conceptualised as establishment of plantations or woodlands primarily comprising eucalypts and to a lesser extent, acacia species."

We agree that "forest planting" could be confusing and have revised this to "tree planting" in reference to the <u>intervention</u> in which trees were planted. We replaced "forest" with "woodlands" to describe <u>existing</u> stands exceeding two metres in height with overstorey strata equal to or greater than 20 percent following the Australian Government National Forestry Inventory (https://www.agriculture.gov.au/abares/forestsaustralia/profiles/australias-forests-2016).

We revised the methods as follows:

Spatially-explicit carbon in woodlands was modelled for each 25 m^2 pixel using FLINTpro (https://flintpro.com/), a spatiotemporal model that uses algorithms consistent with the Australian National Greenhouse Gas Inventory. The baseline assessment represents woodland carbon stock in aboveground biomass, belowground biomass, and dead organic matter across farms through time. We define 'woodlands' as those regions containing trees, shrubs and understorey species in the National Forest and Sparse Woody Vegetation Data (Version 6.0, 2021) with more than 20% canopy cover and reaching at least 2.0 metres in height at least three times over the simulation. This definition is consistent with that of the Australian Government (Department of Agriculture, Fisheries and Forestry [DAFF], 2016). The National Forest and Sparse Woody Vegetation Data (Version 6.0, 2021) classify the landscape into 'non-woodland' (<5% canopy cover), 'sparse woodlands' (5-19% canopy cover) and 'woodlands' (≥20% canopy cover). Land areas that did not achieve this definition for at least three points during the simulation period were deemed non-woodlands. Where a pixel transitioned from woodland to non-woodland, we assume a vegetation loss event, moving carbon from living biomass pools to dead organic matter pools. Where a pixel changes from non-woodland to woodland, carbon accumulates based on vegetation type and growing conditions.

3. Line 140: The authors conflate climatic and vegetation types by stating that "Farms were located in temperate regions rather than rangelands." Rangelands occur in tropical, subtropical, temperate and even tundra regions. Therefore, the dominant ground cover on the study farms could consist of either native temperate rangelands or improved native and/or exotic pastures. The authors should stipulate the dominant herbaceous cover in each case study description.

The geographical region defined by the Australian Federal Government as rangelands does not coincide with temperate regions:
https://www.dcceew.gov.au/environment/land/rangelands

[Figure Redacted]

To avoid confusion, we removed mention of rangelands. Extant native and introduced pasture species are provided in Section 1 of the supplementary information. The revised text in the manuscript reads:

As the majority of the Australian sheep flock are located in southern Australia (Meat and Livestock Australia [MLA], 2022), case study farms were chosen from southern Western Australia (WA), South Australia (SA), Victoria (VIC), New South Wales (NSW) and Tasmania (TAS).

4. Line 136 and elsewhere: What is meant by status quo farm systems?

Here we were referring to existing (baseline) farm systems. This was stated in the methods, although we have added clarity throughout the paper.

5. In Lines 145-146; 166-167: The statement that “Using a participatory approach, farmers co-designed interventions and co-refined model inputs to validate modelled outputs” is unclear. First, did the seven famers work together or did each farmer work individually with the researchers in this regard? Also, how did the famers validate the modelled outputs? Was this validation based on their opinion or on measured biophysical outputs resulting from each intervention? An opinion-based approach would suggest consensus rather than validation, while the latter would represent actual validation of model outputs. This needs clarification.

We added further details to the methods to ensure transparency of the approach:

Using a participatory co-design approach, each farmer worked with the research team to co-refine model inputs to validate the simulated baseline for their farm. This process required several iterations between researchers and each case study farmer until consensus was reached on the final model outputs. Modelled baseline variables included livestock numbers and classes (ewes, lambs, weaners), liveweights, wool and meat production, seasonal pasture growth, supplementary feed use, farm area under vegetation, timing of historical tree planting, costs and financial income. Baseline data for model initialisation were derived from farm management records, livestock inventories, sale records, fuel, fertiliser, irrigation and electricity usage using farmer records collated between 2021 and 2024. Approaches for calibration of biodiversity habitat and woodland carbon stocks are detailed in the ‘Baseline woodland carbon sequestration’ and ‘Baseline woodland biodiversity’ sections below. Because biophysical simulations were conducted over a longer period than biophysical farm data were available (‘Biophysical livestock production systems simulation’ section), simulations based on initial variables were iteratively refined with each case study farmer *until simulated baseline variables were deemed representative for their farm following the participatory process articulated in Bilotto et al. (2025). For example, breeder flock size was iteratively refined until the number of surplus maiden ewes and wethers sold each year met with the experience of each farmer for their farm. This approach ensured rigour in each simulated variable but also afforded farmer trust, credibility and legitimacy in the modelling process with each iteration. Consensus baseline information are shown in Table 1; model inputs are shown in Tables S1-S5. Once consensus was reached for each baseline variable, two types of intervention were co-designed using the consensus baseline files for each farm: demand-driven, where farmers stipulated the intervention, and generic, where the research team designed common interventions that were applied to and compared across farms. In each case, only those variables pertaining to the intervention were altered; other variables were not changed from the consensus baseline conditions. Model inputs for demand-driven interventions were stipulated by each case study farmer (e.g. where trees would be planted) and by the research team for generic interventions using peer-reviewed literature where relevant (e.g. reduction in enteric methane associated with supplementation with Asparagopsis measured in scientific experiments). Model input assumptions for each intervention are provided in Table 2, the 'farming systems interventions' section and the supplementary information. While transdisciplinary assessments such as those enacted in the present study are more difficult to operationalise cf. reductionist studies due concerted coordination and co-learning required between parties, participatory approaches are arguably more amendable to impact, because they elucidate enablers and inhibitors of behavioural change (Bilotto et al. 2025; Harrison et al. 2021). Use of people-centric design in this way meant engaging end-users to develop fit-for-purpose interventions aimed at GHG emissions reduction or removal but also (ideally) improvements in livestock production, profit and biodiversity habitat.*

6. Lines 172-174; and lines 800-801: The statement that "Several iterations between researchers and case study farmers were conducted to ensure that simulated outputs were accurate representations of baseline conditions" is also confusing. Were the model simulations not conducted to predict intervention effects, which were then compared with the baseline condition? This needs further clarification.

Here we were referring to calibration of each baseline variable using the iterative participatory approach detailed above. Once consensus was reached (and baselines were considered appropriate), only those variables pertaining to the intervention were altered; other variables were not changed from the consensus baseline. Simulated results from interventions were then compared with baselines for the same farm (e.g. Fig. 4). We added further details to the manuscript along these lines.

7. Lines 379-381: "Biodiversity was attributed low and high economic values to capture the range in income associated with variable biodiversity prices. Biodiversity was valued as a carbon sequestration co-benefit, assuming either AUD 29 or AUD 65/tonne CO2eq. sequestered." How were these two biodiversity values determined. It seems highly problematic to assign such blanket values to biodiversity relative to a single ecosystem service that trees provide, i.e., carbon sequestration. Also, what value was assigned to biodiversity in the remaining 90% of each case study? Biodiversity is not associate just with tress. This needs clarification.

The purpose of the study was to develop GHG emissions mitigation *interventions* that also had co-benefits in terms of biodiversity, profitability and productivity. As such, biodiversity was economically quantified as a co-benefit associated with carbon sequestered by planted trees. We quantified habitat condition, effective habitat area and threatened species habitat (details and rationale are provided in the 'baseline woodland biodiversity' section of the methods) associated with planting trees. Financial income associated with biodiversity in extant woodlands were not included because none of our case studies derived income from biodiversity associated with existing woodlands. Including income from biodiversity associated with existing trees would neither form a representative economic baseline nor facilitate standardised comparison with other interventions. Low and high economic values associated with biodiversity as a co-benefit of tree planting were assigned based on planting native tree species in Australia. Low and high values reflect a range of valuation methods from an extensive review of the literature, including comparable projects and methods in which co-benefits were quantified (Carbon Neutral 2022). We believe this is one of the most rigorous and relevant valuations available, considering that biodiversity markets associated with planting native tree species in Australia are nascent: the **first project** under the recently legislated Nature Repair Market was registered on 12 August 2025. Quantification of the economic value associated with planting native trees in this way is not only analytically defensible, such analyses are highly topical for the land-use industry.

We have clarified the methods of the paper in this vein.

8. Line 464: To what does "most prospective of those examined" mean? Might a better term be "impactful".

We feel that "impactful" could be construed as having either positive or negative connotations. We revised the sentence to '…*most favourable of those examined'*.

9. Line 561-562: This should be changed to "impacted both carbon sequestration (through growth of trees) and enteric methane (through reduction in animal numbers)".

Revised as suggested.

10. Figure 9: The include image does not show the content of the figure.

We are unsure what the reviewer is suggesting by "the include image". Assuming they were referring to the variables shown in the charts and description provided in the caption of the figure, we clarified the caption as follows:

Change in absolute (A) and percentage (B) effective habitat area, high quality habitat condition, threatened species habitat and benefit for plant species persistence between 2020 to 2050 associated with planting 10% of pasture area with native tree species (see Methods for details of each biodiversity indicator).

Note that Figure 9 has been revised to Figure 7.

11. Line 615: Include "conditions" after present.

Revised as suggested.

12. Line 654: Change "emanate" to "result in".

Revised as suggested.

13. Line 665-666: I suggest deleting the statement starting with “comprising low closed/open canopy forests …” for the reasons stated above that these areas were likely woodlands and not forests.

We replaced “forests” with “woodlands” and revised the text (following this reviewer’s suggestions in the previous review) to:

Veldman et al. (2015b) highlight the distinction between afforestation and reforestation; the former being tree planting on land that was not historically forest, the latter being the planting of trees on deforested land. Veldman et al. (2015a) opine that afforestation on grasslands, savannas and open-canopy woodlands can severely compromise hydrology, sunlight availability and soil nutrient cycles, devastating extant biodiversity and ecosystems services. While we note that prior to European settlement in Australia, our case study regions supported woodlands (Geoscience Australia, 2018; Municipal Association of Victoria, 2018), we are not advocating planting of trees on farmland en masse. Rather, we suggest that tree planting occur only with careful planning, on regions such as those with low pasture productivity, along fence lines, where appropriate.

14. Line 670: Why would trees be planted in riparian zones? This often leads to desiccation of riparian zones, especially where trees did not previously occur.

“Riparian” has been removed from this sentence (see above).

Reviewer #3 (Remarks to the Author):

Major comments
1. This is a paper with good material, but the arguments and written expression is overly complicated, which detracts from the overall impression. At the core is a technical analysis relying on case studies of sheep farms in Australia, and the most useful results are from the comparisons of those farms. As it stands, the manuscript is not really suitable for a journal such as Nature Communications, and would be better targeted to a journal where the results are more relevant, such as an agricultural economics journal.

This comment, along with other comments from this reviewer below, appear to reflect confusion over the purpose of the study. In fact, this reviewer appears to be suggesting that we focus on another question entirely, evidenced by their final comment ‘*It would be more straightforward to have a single research question to provide more purpose to the paper – perhaps something like ‘Are strategies to reduce greenhouse gas emissions relevant across a cross section of sheep farms in Australia?*’. As mentioned in the Introduction, the purpose and indeed novelty of this work is the co-benefits and trade-offs caused by GHG emissions mitigation interventions on profitability, productivity and biodiversity. We are unaware of any study that holistically addresses interdisciplinary implications of systems interventions on GHG mitigation, profitability, biodiversity and productivity. All of these aspects are important, because an intervention that has positive effects in one dimension (e.g. reduced GHG emissions) may come at the expense of other dimensions (e.g. reduced biodiversity or increased costs). It is thus critically important that all of these dimensions are addressed simultaneously when assessing proposed mitigation practices, not using siloed assessments of each metric in isolation. We suggest that this approach can be generically adapted to any agroecological region, production system or interdisciplinary mitigation problem. Concurrent assessment of changes in multiple indicators following practice change would arguably lead to more sustainable outcomes that perpetuate over the long-term, because enablers and barriers to adoption are teased out and addressed as part of the co-design process.

To clarify the purpose of the study, we added the following to the introduction. Note that the paragraph is written in present tense, following Nature Communications guidelines for the final paragraph of the introduction:

Here, our purpose was to co-design GHG mitigation practices that also improve production, profit and biodiversity. *We select case study farms spanning the southern seaboard of Australia, then use transdisciplinary participatory action research to iteratively refine modelling frameworks to ensure reliable simulation of baseline production, profit, spatially-explicit carbon and biodiversity. Through people-centred design, we co-develop interventions aimed at carbon sequestration, enteric CH_4 mitigation and avoidance, improvements in production efficiency and/or environmental stewardship, then contrast changes borne by these interventions with historical baselines. Our purpose was participatory co-design of GHG mitigation interventions that benefit enterprise profit, liveweight production and biodiversity. We find that planting trees and adoption of antimethanogenic feed supplements resulted in similar GHG mitigation, but carbon sequestration from planting trees were generally lower cost than other mitigation options. However, carbon sequestration generally diminishes over time, whereas emissions reductions from avoidance of methane is permanent and irreversible. While many interventions are capable of unidimensional benefit (e.g. improving profit or GHG abatement), few interventions reduce net farm GHG emissions while also improving profit and biodiversity. Our results suggest that antimethanogenic pastures are among the most favourable, evoking significant mitigation and modest economic benefit. We find that benefit conferred by any farming system intervention should be assessed using an interdisciplinary range of indicators, because benefits afforded in one dimension can be more than offset by changes in other dimensions. Our analysis also suggests that benefits associated with such interventions may be more appropriate if assessed relative to the historical status of the same farm, rather than across farms, because all farms have unique environmental, economic and management status.*

Given that Reviewer 3 expressed confusion over the aims of the study, we refined and reiterated the motivation of the study in the abstract and conditioned our justification of the rationale in the introduction and discussion. We further clarified the methods and reduced use of jargon throughout the paper. Examples include: (i) replacing *forest* with *woodlands*, (ii) replacing *status quo* with *baseline farm system,* and (iii) changing from *bottleneck* to *underlying deficiency* or similar. We added further details of the analytics underpinning the methods, including the transdisciplinary participatory approach with the case study farmers used to ensure that model simulation of baseline conditions were robust.

2. Title. The title is not very informative about its contents, which means it will be poorly cited. I suggest something more pragmatic, such as ‘Evaluating options to reduce greenhouse emissions and associated co-benefits: case studies of sheep farms in Australia’

We revised the title to:

Tailoring sheep farming greenhouse gas emissions interventions can realise greater co-benefits

The revised title reflects one of the key conclusions of the study and is more specific. It is also less than the maximum title length required by Nature Communications (15 words).

3. Abstract, first sentence. This overstates the priorities at the farm level. Farmers are not necessarily worried about reducing greenhouse gas emissions / climate change / biodiversity, apart from dealing with government policies and regulations relating to those issues. Those issues are better viewed as externalities to farm production. This first sentence is okay at the community level, but not at the farm business level. This misunderstanding continues through the paper – why should farmers reduce greenhouse gas emissions / sequester carbon unless there is some direct benefit to them?

We very much disagree with the reviewer's opinion that the first sentence overstates priorities at the farm level (*Land managers are challenged with the need to balance manifold priorities, including adapting to climate variability, greenhouse gas (GHG) emissions mitigation, biodiversity conservation and sustaining profit*). Farmers have and are implementing strategies to reduce GHG emissions for market access, carbon insetting (reducing GHG emissions within farm businesses without selling carbon credits), alignment with industry regulation (e.g. https://www.mla.com.au/research-and-development/Environment-sustainability/) and current or forthcoming legislation (https://www.dcceew.gov.au/climate-change/emissions-reduction/net-zero). Farm management requires balancing priorities! The notion that farmers are 'not necessarily worried about climate change/biodiversity' is unsupported and contradicts a significant body of peer-reviewed evidence (Dang et al. 2019; Pearce et al. 2018; Siebert et al. 2026; Wheeler et al. 2021). It is however important to note that a range of values, perceptions and beliefs influence farmer behaviour. For example, stewardship as well as intergenerational sustainability values can be one of the most important determinants of farmer behaviour (Siebert et al. 2006) and well as indicators that visually respond to management practices (Dang et al. 2019).

The first sentence of the abstract is designed to set the scene. Use of arable land is under competing pressures to (1) produce enough food to sustain a burgeoning global population, (2) store carbon to offset global warming and prevent climate change and (3) preserve finite biodiversity habitat. At the same time, land managers are being forced to adapt to the changing climate and remain profitable despite costs that are increasing at greater rates than prices. As such, land managers will need to not just consider their management priorities, but also consider trade-offs that may occur as a result of practices they impose. It is this issue that is central to the current study.

Our involvement with farmers across a range of industries (red meat, dairy, horticulture, broadacre cropping, viticulture) has shown the converse of that postulated by the reviewer. Our experience demonstrates that producers are indeed concerned with reducing GHG emissions, climate change and biodiversity. We sought participation in this project through expression of interest and received many more applicants than we had the resources to engage with (further details added to first section of the methods). The applicants themselves evidence farmer desire to implement practices for reducing GHG, improving biodiversity and improving profitability.

The comment "*why should farmers reduce greenhouse gas emissions / sequester carbon unless there is some direct benefit to them?*" partly comes back to the motivation of the study: farmers will not change practices to reduce GHG emissions or improve biodiversity unless there is an economic benefit.

We need to be clear that the purpose of the study was to co-design GHG emissions mitigation interventions that also had beneficial effects on productivity, profitability and biodiversity. We feel that on many occasions, this objective has been overlooked by this reviewer. As mentioned above, we are unaware of any study that addresses holistic interdisciplinary implications of systems interventions on GHG mitigation, profitability, biodiversity and productivity. All of these aspects are important, because an intervention that has positive effects in one dimension (e.g. reduced GHG emissions) may come at the expense of other dimensions (e.g. reduced biodiversity or increased costs). It is thus critically important that all of these dimensions are addressed simultaneously when assessing proposed mitigation practices, not using siloed assessments of each metric in isolation. We suggest that this approach can be generically adapted to any agroecological region, production system or interdisciplinary mitigation problem. Concurrent assessment of changes in multiple indicators following practice change would arguably lead to more sustainable outcomes that perpetuate over the long-term, because enablers and barriers to adoption are teased out and addressed as part of the co-design process.

Dang, H. L., Li, E., Nuberg, I., & Bruwer, J. (2019). Factors influencing the adaptation of farmers in response to climate change: a review. *Climate and Development*, *11*(9), 765–774. https://doi.org/10.1080/17565529.2018.1562866

Klebl, F., A. Parisi, K. Häfner, A. Adler, S. Barreiro, F. V. Bodea, V. Brönnimann, J. P. R. de Vries, A. Dos Santos, A. S. C. Hood, I. Melts, R. Popa, F. Vajna, E. Velado-Alonso and M. L. Kernecker (2024). "How values and perceptions shape farmers' biodiversity management: Insights from ten European countries." Biological Conservation **291**: 110496.

Pearce, T.D.; Rodríguez, E.H.; Fawcett, D.; Ford, J.D. How Is Australia Adapting to Climate Change Based on a Systematic Review? *Sustainability* **2018**, *10*, 3280. https://doi.org/10.3390/su10093280

Siebert, R., Toogood, M. and Knierim, A. (2006), Factors Affecting European Farmers' Participation in Biodiversity Policies. *Sociologia Ruralis*, 46: 318-340. https://doi.org/10.1111/j.1467-9523.2006.00420.x

Wheeler, S. A., C. Nauges and A. Zuo (2021). "How stable are Australian farmers' climate change risk perceptions? New evidence of the feedback loop between risk perceptions and behaviour." *Global Environmental Change* **68**: 102274.

4. Page 2, first paragraph. The writing style is not clinical enough – many statements are overblown. Why start with global food security as the main driver when the analysis is about individual sheep farms (perhaps producing wool which is a fibre rather than food)? Most of this first paragraph could be dropped. There are too many normative statements here – 'must be developed, 'must be preserved'. Why conclude the paragraph by stating that only solutions that meet all goals (GHG reductions, production, profit, biodiversity) are in scope?

The first paragraph was designed to show that land use must be prioritised between motivations for food/fibre security in light of the growing global population, GHG emissions mitigation (due to global warming), preservation of biodiversity habitat (as land area is finite and agriculture occupies around 37% of the earth's terrestrial area). As such, land use prioritisation will become increasingly important in future. The uses to which land is put, and how priorities are balanced between objectives, was a key focus of our study. We consolidated the first two paragraphs, as the second paragraph originally dealt with GHG emissions mitigation and was verbose. In response to the reviewer's comment about food vs fibre, sheep produce both wool and meat, as do all of the case study farms in our study (Fig. 1). We have removed the normative statements the reviewer refers to, noting that Nature

Communications requires the first part of the introduction to set out the grand challenges and significance of the study in a general nature. The first and second paragraphs have been integrated as follows:

In response to climate change, population growth and biodiversity habitat destruction, land use activities and balancing of land-use priorities will become increasingly important in future (Harrison, 2021; Harrison et al., 2011; Liu et al., 2021). Contemporary farmers are under mounting pressure from market signals, legislation, regulation and consumer preferences to adopt innovations for sustainably reducing greenhouse gas (GHG) emissions (Meier et al., 2020b; Taylor et al., 2016). At the same time, farm operation must be profitable, lest farmers will be unable to generate sufficient cash flow to endure (Ho et al., 2014). As land use is the primary driver of biodiversity loss, there is also a need to develop farming practices that preserve or enhance existing biodiversity habitat (Ceballos et al. 2015; Dias et al. 2019; Cabernard et al. 2024). While pathways for reducing agricultural GHG emissions have long existed (Burke et al. 1990; Paustian et al. 1997; Smith et al. 2007; Henry and Eckard 2009; Tollesfson et al. 2010), little attention has been given to coincident implications for profit, productivity and/or biodiversity (Harrison et al., 2021). Such implications are important, because economic, environmental, social and/or psychological co-benefits and trade-offs associated with a proposed innovation ultimately catalyse or inhibit behavioural change (Rizzo et al., 2024). Indeed, recent work has suggested that progress towards sustainable agriculture is hampered by siloed scientific discourses that lack interdisciplinary dialogue (Fischer et al. 2025).

5. Page 2, second paragraph, first sentence. The sentence is a bit pointless. Why say that pathways have existed for decades, but then only give references from 2016? I think what you mean to say is that there is a great deal of knowledge about these issues. My other concern with the arguments in the second and third paragraphs is that there does not seem to be a systematic approach to selecting the factors that are relevant to reducing emissions in the livestock sector in Australia. I think the key reference in this topic is Henry and Eckard (2009).

Much of the reply to this comment has been provided above in response to the previous comment. The first two paragraphs were merged and more historical references to demonstrate early research on GHG emissions mitigation have been cited. The second paragraph now speaks to enteric methane emissions, which in the absence of land use change is the largest farm level GHG. We then continue the introduction with text relating to antimethanogenic pastures and planting trees, as these interventions were put to us for analysis as part of the inter-disciplinary co-design assessment conducted in the present manuscript. It is important that these interventions be discussed in the introduction by way of background to the study.

6. Another reference that seems to be missing is Wiedemann S., Yan M., Henry B. and Murphy, C. 2016 – that is another similar case study in Australia.

We now compare emissions intensities from the Wiedemann et al (2016) study with our emissions intensities, including discussion of reasons for similarities and differences.

7. Page 4, first paragraph, second sentence. This is presumably just for sheep- as there are also major production zones in northern Australia. This sentence suggests that the case studies are representative of the sheep industry in Australia, but I doubt that is true. The

attached MLA figure provides a more accurate distribution of sheep in Australia in 2021, which shows that the case studies are not fully representative of the industry.

We focus on sheep production zones as opposed to all livestock production in Australia (which include northern Australia, where cattle farms are numerous). Farms were strategically chosen based on their (1) position within key sheep production zones of Australia (MLA 2022), (2) agroecological region, ranging from cool temperate in southern Victoria to semi-arid in Western Australia, (3) enterprise mix and number of animals, and (4) willingness to engage and likelihood of persisting with the study over four years. We have elaborated on the methods used to select farms as follows:

As climate and soil type influence plant growth, livestock carrying capacity, enterprise and often farm size, real farms were strategically chosen across Australia following advertisement of the study in national industry newsletters in January 2022 (MLA Weekly, SALRC and WALRC). Following 115 expressions of interest in participating in the study, seven farms were chosen based on (1) their position within key sheep production zones of Australia (MLA 2022), (2) agroecological region, ranging from cool temperate in southern Victoria to semi-arid in Western Australia, (3) enterprise mix and number of animals, and (4) willingness to engage and likelihood of persisting with the study over four years. Farms chosen practise enterprises typical of Australian sheep production systems, focussing on meat and/or wool production (Supplementary Information section 1 and Table S1). Climate, soil, pasture and production system parameters of these farms were used to initialise models before which several interventions, and several scenarios within interventions, were examined in silico. As the majority of the Australian sheep flock are located in southern Australia (Meat and Livestock Australia [MLA], 2022), case study farms were chosen from southern Western Australia (WA), South Australia (SA), Victoria (Vic), New South Wales (NSW) and Tasmania (Tas), noting we are not suggesting that our case studies reflect the full spectrum of diversity comprising the Australian sheep industry. Farm sizes ranged from 250 ha to 7,777 ha, with 41-97% of farm area subject to livestock grazing, supporting 950-56,000 dry sheep equivalents (DSE; Fig. 8). Following convention we assume that one DSE represents a two-year old 45 kg Merino wether consuming 7.6 MJ per day (McLaren, 1997). Baseline data were collated from each farm to calibrate a range of biophysical, economic and ecological models (see below).

We did not and have not suggested the seven case studies are representative of the entire industry. Part of the novelty of our study was the co-design of interdisciplinary interventions, and the co-benefits and trade-offs between economic, environmental and agronomic dimensions. The purpose of our study was not to compare the existing states of sheep farms nor represent the entire industry. If this were the purpose, we would have sampled many hundreds of farms, yet such design would have precluded the depth of interdisciplinary analysis conducted here (across pasture and livestock production, herd/flock dynamics, spatially explicit carbon and biodiversity, profitability and greenhouse gas emissions).

8. Page 25, Discussion, first paragraph. The term 'bottleneck' is problematic, because it is being used to indicate unique solutions at the farm level as distinct from industry or program level solutions. It would be better to begin the discussion with a review of the results in terms of industry or program level interventions, and then discuss more specific farm level interventions after that. It would be better to avoid the use of the term 'bottleneck'.

One of the conclusions of our study is that the greatest benefit in terms of GHG mitigation, improvement in profit, biodiversity and productivity is likely to occur when underlying deficits are addressed. Those deficits can span from environmental (e.g. excessive feral grazing), economic (high expenditure on purchased supplementary feed) or management (e.g. producing fewer saleable livestock relative to the number of breeders retained increases emissions intensity). While underlying deficits are of site-specific nature and differ from farm to farm, this conclusion applies to the whole livestock industry, and one could argue that it would similarly apply to the agriculture industry more broadly. Our study indicates that the notion in which a single intervention could be applied generically and result in widespread benefit is implausible. Interdisciplinary benefit is more likely to occur when close attention to historical climate, management and context of the farm system is accounted for. Only then can areas of underperformance be diagnosed and overcome.

Notwithstanding the above, we restructured the discussion and removed the term ‘bottleneck’ as suggested by the reviewer.

9. There does not seem to be enough critical analysis of the outcomes. If the findings are that solutions have to be tailored at each individual farm level, this has large implications for costs and program effectiveness – and would make solutions very difficult to achieve.

We agree with the argument that critical analysis was lacking and added further discussion to that effect, including assumptions pertaining to planting trees on pasture land, costs associated with feed supplementation, intake of antimethanogenic feed additives, and the need to tailor solutions to the individual farm level.

We disagree with the argument that site-specific solutions are difficult to achieve. In fact, we argue that previous attempts to reduce agricultural GHG emissions through universal roll out of ‘one-size-fits-all’ solutions have likely inhibited more substantial reduction of historical GHG emissions in the past, noting that national livestock GHG emissions (in the absence of land-use change) have been relatively constant over the last decade (Bowen-Butchart et al. 2025). To demonstrate how our approach could be operationalised, we have added further text on how individual solutions could be recognised and tailored at the farm level, along with the limitations of doing so.

Bowen Butchart, D., K. M. Christie-Whitehead, G. Roberts, R. Eisner, H. Reinke, S. Munidasa, A. Macdonald, V. Higgins, N. Doran-Browne and M. T. Harrison (2025). "Advancing quantification of Australia's beef cattle and sheep emissions accounts - Carbon sinks and emissions hot spots battle it out en route to net zero." Agricultural Systems 222: 104168.

10. The discussion section and the conclusions are written from the perspective of the sheep grazing industry in Australia, with an implicit assumption that the case studies are representative of the industry. Yet that is unlikely to be true. Many sheep producing regions in Australia are not represented, the largest farm in the case studies is from Tasmania, which is unusual. It would be more accurate to focus the discussion section on an analysis of similarities and differences between the case studies, and then the extent to which that may offer insights to the wider sector in Australia. It would be useful to pitch this paper as a cross-sectional analysis of the sheep industry in Australia, rather than how can the industry reduce GHG emissions on farm.

We make no assumption that the case studies are representative of the entire sheep industry, nor of a particular region. That was not the purpose of the study. As previously mentioned, the purpose was to co-design GHG emissions mitigation *interventions* that also had benefits for profit, production and biodiversity. We selected farms from diverse regions, sizes and production systems, yet we made no assertion these farms would represent the entire population of farms in the Australian sheep industry. The diversity in farm sizes was necessary because productivity, profitability and GHG emissions depends on farm area, animals carried and enterprises supported; this diversity is reflected in Fig. 1. We find comparison of farms less relevant than comparison of the influence of each intervention.

Notwithstanding the above, we restructured the discussion in line with the former suggestion by this reviewer and previous reviewers. We now begin with a discussion of the common interventions across farms followed by demand-driven (farm-specific) interventions, starting with the most important conclusion that site-specific intervention results in the greatest interdisciplinary benefit given this addresses the main aim of the study. We follow with discussion of planting trees, antimethanogenic legumes and antimethanogenic feed additives, as these interventions were assessed for all farms, then demand-driven interventions and alternative assessment approaches (as suggested by this reviewer in their previous comment).

We end the discussion with implications and broader significance of the study, including factors that influence adoption, and the longevity with which impacts of interventions may occur. This includes stacking of interventions, demand-driven innovations and the extent to which change is dictated by baseline conditions. Considering the motivations of the paper, we believe that a comparison of *interventions* across farms is more appropriate than a comparison of *farms* per se. Indeed, one of the insights arising from this study is that is it more appropriate to compare relative effects of an intervention via relative change for a given farm (e.g. percentage change) rather than comparing absolute states across farms (e.g. profit of farm 1 with profit of farm 2), because all farms have different numbers of animals, costs, land area, costs, management etc.

11. A real weakness of the paper is that there is no key message coming through from the results. It looks a bit like that the authors have assembled seven case studies and have then struggled to find a defining story. It would be more straightforward to have a single research question to provide more purpose to the paper – perhaps something like ‘Are strategies to reduce greenhouse gas emissions relevant across a cross section of sheep farms in Australia?

It is possible that this reviewer was unclear on the aim of our study, which was to co-design GHG emissions mitigation interventions that also had beneficial effects on productivity, profitability and biodiversity. We have clarified this objective throughout the manuscript and have re-written much of the discussion in this vein. Some of the key messages from the paper include that (1) the greatest benefit tends to occur from addressing site-specific problems, (2) stacking interventions can further improve co-benefits (but can also further increase trade-offs), (3) comparison of interventions is preferable on a relative basis (comparing a farm against itself, rather than against other farms), because relative improvement in GHG mitigation, profit or biodiversity depends on existing state (and no two farms have the same existing state), (4) the most appropriate intervention depends on whether the farmer’s motivation is land stewardship, profit, biodiversity preservation or other, and (5) GHG mitigation via carbon sequestration tends to be lower cost and often comes with productivity co-benefits, while mitigation via reduced/avoided methane is permanent but costs more when delivered via feed additives.

Citations to the references below have been added to the discussion.

Henry, B. and Eckard, R. 2009. Greenhouse emissions in livestock systems. Tropical Grasslands, 43, 232-238.

Wiedemann S., Yan M., Henry B. and Murphy, C. 2016. Resource use and greenhouse gas emissions from three wool production regions in Australia. Journal of Cleaner Production DOI: 10.1016/j.jclepro.2016.02.025

Reviewer #3 (Remarks to the Author)

I thank the reviewers for the careful responses to my previous review. I think the paper is much improved, but still have a few remaining comments.

We thank the reviewer for their feedback.

1. In the reviewer response to my suggestions they emphasised that this was a co-design project with sheep producers, and that it is this co-design element that makes the paper so unique. Yet none of the sheep producers are co-authors on the paper, so I question whether co-design process has been as transparent as the authors suggest.

Our study participants were briefed on the participatory action research co-design aspirations of the study from the outset and throughout the project via regular communications. Co-design of interventions appropriate to agroecological region and production system was conducted with the intent of the farmers and research team sharing and gaining knowledge, and the research team documenting results. While research outcomes were shared and refined iteratively throughout the process, the intention was not for case study farmers to co-author the manuscript, but rather co-design interventions, and potentially adopt beneficial practices arising from the participatory process.

2. Abstract, first sentence. I disagree with the authors' response about my previous comment here – their claims that industry wants to make changes indicates to me that they are not talking to a broad enough cross section of farmers. Currently agriculture is exempt from meeting greenhouse targets in Australia, and there are no requirements for red meat producers to report their greenhouse gas emissions. There are certainly producers within agriculture who are interested in the issues, and there are activities to promote voluntary reductions, but the bulk of agricultural producers have little interest in voluntary reductions. As well the small number of sheep producers involved in the study does not suggest major industry interest. So the statements that producers 'need' to mitigate greenhouse gas emissions is too strong, as they are under no current obligations to do so. These and other statements would be better in terms of designing strategies with an eye to the future when requirements may come into place.

The first sentence of the abstract "*Land managers are challenged with the need to balance manifold priorities, including adapting to climate variability, greenhouse gas (GHG) emissions mitigation, biodiversity conservation and sustaining profit*" was designed to demonstrate that land managers are increasingly pressured from multiple directions to balance management practices affording production (for food security), profit (for economic viability), biodiversity conservation (particularly threatened species) and reduce GHG emissions (to avoid global warming). The need to reduce GHG emissions is becoming increasingly important nationally and internationally, and in future, climate impact disclosures will be mandated by the Australian Government. We revised the sentence to capture these pressures on land users:

While land managers face growing societal and policy expectations to sustain food production, conserve biodiversity, enhance carbon sequestration, maintain economic viability and reduce greenhouse gas (GHG) emissions, practices affording these outcomes may not always be congruent.

3. The paper is written in very academic terms, and the language that is used is not really

reflective of a co-design study that has been done in partnership with sheep farmers. I recommend replacing terms like ‘manifold priorities’ and ‘in silico’ with simpler language.

We replaced ‘manifold’ with the revised sentence documented in the previous response. ‘In silico’ is a more succinct, accurate and appropriate way of saying “experimental simulations done on a computer” and so we have elected to retain the former phrase.

[Figure Redacted]