Author response to both reviewers': Forage quality declines with rising temperatures, with implications for livestock production and methane emissions

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We would like to thank both reviewers for taking the time to read our manuscript and provide us with thought provoking and well-structured reviews. We agree with Reviewer 1, that we have raised some important questions regarding global methane contributions and feedbacks, and we agree with Reviewer 2, that the topic is interesting, relevant and timely. We are also pleased that the reviewers felt that we had done a good job gathering data to show the variability of forage quality for key quality parameters and plant species, across world climates. We collected a large dataset from 33 published articles from 32 sites in 17 countries (Table A1). This resulted in over 800 rows of data, each with detailed climatic, edaphic and management variables from 55 fodder plant species, and this dataset allowed us to identify general relationships (Table A2).

Reviewer 2 highlights the limitations of our modelling approach, in particular the parameters which we did not include. While we agree with the reviewer that some potentially important aspects are not represented in our model, it was not our intention to deliver a definitive estimate of the magnitude of the climate-forage-livestock emissions feedback, as this would require significant model development and is beyond the scope of this paper. Instead we hoped to highlight this logically robust and potentially important process, and support it with empirical evidence. We will clarify this point in the revised manuscript. We also hope that this paper will inspire discussion and a more complete work programme, in which all of the potentially important parameters can be modelled in more detail. We agree that we should highlight the parameters we did not model and then discuss their implications in more detail in the Discussion section.

One area where the reviewers' disagree is in the predictions of future methane production resulting from our analyses. Whilst Reviewer 1 would like to see our predictions presented in greater geographical detail, Reviewer 2 felt that the relationship between forage quality and elevated methane production is not sufficient to make robust predictions. We agree with the concerns of Reviewer 2. To rectify this, we would repeat our predictions using several other published models. We would use three models: one representing North American cattle (Ellis et al., 2007; Moraes et al., 2014); a second representing European cattle (Nielsen et al., 2013); and a third model representing North American, European and Australasian cattle (Storlein et al, 2014). As summarised in a recent review paper, all models which include neutral detergent fibre (NDF) and which are derived from *in-vivo* studies of forage dietary composition show that NDF has a positive effect on methane production in cattle (Appuhamy et al., 2016). Based on our observations of the model parameters we believe that these further models will provide broad support for our predictions and it is highly unlikely that the relationship is an artefact as Reviewer 2 postulates. However, it is likely that this portfolio of model outputs will differ in the magnitude of predicted methane production and so we propose to include a comparison of model outputs in our final paper.

We believe that the all of the remaining reviewers' comments can be satisfactorily addressed or clarified in the final manuscript, and that doing so will result in a significantly improved manuscript that allays their concerns. We also feel that our conclusions are robust and that this paper is of high interest to the readership of Biogeosciences.

Responses to specific comments are presented below, with reviewer comments in blue and author responses in black.

Responses to Reviewer 1

General comments: The authors present a meta-analysis of forage studies in order to ascertain any impacts of growing conditions on methane production by livestock. Overall I think this study is a valuable contribution by highlighting a positive feedback between temperature increases and methane production by livestock. The clear and succinct project raises many important questions for global methane contributions and feedbacks under future climate scenarios, but could be improved by some additional considerations and clarifications.

Response: We thank reviewer 1 for these positive comments and, in particular, that we have raised some important questions regarding global methane contributions and feedbacks.

Proposed changes: No changes requested by reviewer.

I did not find that the analysis of Nitrogen addition to add much value as part of the results section, and because of the limited data on this part of the study (and lack of a significant impact for the species with the most data) is too preliminary an analysis for inclusion.

Response: We feel that it is vital to include nitrogen in our analyses since it is a major global change driver and widely known to have an important effect on forage productivity (LeBauer and Treseder, 2008; Lee et al., 2010) and plant tissue chemistry (Aerts and Decaluwe, 1994; Aerts, 2009; Cherney and Cherney, 1997; Kering et al., 2011; King et al., 2012). Of the 803 rows of data we collected for this study, 535 rows included data on the quantity of nitrogen added, around 67% of the dataset.

Proposed changes: Although we feel that the nitrogen material does not distract from the main message of the paper and adds an important element to it, we would be happy to remove this section if the Editor sees fit. We will also clarify in the text the large amount of fertiliser application rate data that was collected if it remains part of the manuscript.

In addition, clarification and discussion if these results are indicative of changes within a species or between species (and relative contributions of each) is needed as this is considerable factor in the assumptions for their model.

Response: There will be substantial variation between species responses to climate and nitrogen, as supported by the data that we have presented. However, we were not focussed on these differences, preferring to focus on the effects of climate and fertilizer management. We therefore included species and site as random effects in our mixed effects statistical modelling. By doing this we accounted for differences between species and between sites without making them the focus of our analysis. Patterns generated by compositional and physiological changes could not be disentangled in our study, only the response of both aspects to mean annual temperatures (MAT). However, the positive responses to monthly temperatures and MAT imply that both compositional and physiological changes each play a role.

Proposed changes: We will add more detail in the manuscript so that these methodological aspects are clearer.

Furthermore, projections related to RCP 2.6 and 8.5 and increased methane should be developed further than currently presented to clarify the relationship to livestock assumptions in these models and present the spatial variability between regions of the world in more detail.

Response: We are pleased that the reviewer felt that our models are informative and we would like to present their outputs in greater detail. However, given the strongly conflicting opinions of Reviewers 1 and 2 we would like guidance from the Editor. We believe that our models are robust.

However, to demonstrate the potential uncertainty in our predictions and to reduce our reliance on a single published study, which quantified the relationship between forage NDF and methane production (Kasuya and Takahashi, 2010), we would now like to recalculate our predictions using a suite of other published models.

Proposed changes: We have summarised this proposed approach in more detail above. Based on our observations of the model parameters we believe that these additional models will broadly support our published model. There will likely be some quantitative differences between model outputs and we will include a comparison between models in our final paper. If these models provide consistent predictions we could then justifiably include more detailed maps.

Finally, additional clarification of other assumptions and limitations to this study are needed to generate discussion and thoughts about taking these coarse projections further. Grassland communities are complicated and although the authors show a response to general long term temperature to forage nutritive value, inter-annual and geographic variability (plus management) are additional important factors.

Response: We agree with this comment.

Proposed changes: We would address this by including more detail about the assumptions and limitations of our work. This would also enhance our manuscript in generating discussion and further research. Further detail of these changes is given below.

More detailed comments on specifics sections of the manuscript follow. Specific Comments Line 42, if 48% of the biomass is grass, would be good to know what composes the other 52%. This is a big deal for methane production and would help with conclusions and discussion points.

Response: Pasture contributes 48% (2.3 billion tons) of the biomass consumed by livestock, followed by grains (1.3 billion tons, 28%). The remainder is from leaves and stalks of field crops, such as corn (maize), sorghum or soybean (Herrero et al., 2013). This would certainly help with our discussion and we would like to include this.

Proposed changes: Manuscript text will be updated to include this information.

Line 51, consider talking about tundra regions here as well since you base your results on this climate type. Since these are harsh climate do they behave like arid regions (stressful) or temperate regions (cooler, so greater nutritive value)?

Response: We would be happy to discuss tundra regions and feel this would improve the paper.

Proposed changes: A short section on tundra would be added.

Line 84, you need to talk about the size of the database here and not just percentages. It is important to know the distribution and number of species across climate types, the amount of data that your fertilizer model is based off of, etc. It is hard to determine if the results you have are from within species variability or across species variability. The two lead to different conclusions and are an important to discussing changes in methane production from cattle in the same locations (are we assuming a change in forage species?).

Response: We agree that this is essential for the reader and apologise for the lack of information presented in the manuscript. We summarise the size of the dataset above.

Proposed changes: We will provide further detail of how this data breaks down across biogeographic regions, species, functional types etc. in the revised manuscript.

Line 91, a brief discussion of whether harvested time impacts DM and other variables and then later on, account for this in the analyses (i.e. on line 109 it is reported that a sample was taken at -5 degrees C).

Response: We would be happy to include this. This will also improve our introduction and discussion.

Proposed changes: Manuscript to be updated to include this information.

Line 143, is this for all temperature and rainfall values? Both the month of collection and mean annual values?

Response: Separate analyses were carried out for monthly and annual values.

Proposed changes: We will add detail to clarify this and also include a comparison of the results.

Line 167, RCP 2.6 and RCP 8.5 incorporate projections in the amount of livestock as a part of determining changes to radiative forcing. Be explicit here that you are restraining your analysis to just the projected temperature changes as determined by RCP 2.6 and then 8.5, not any changes related to projections in number of livestock in the scenarios or any assumptions about where, feed type, etc.

Response: This is correct and we are happy to clarify this.

Proposed changes: Manuscript will be updated to make this point clear.

Line 201, consider splitting the relationship between C3 and C4 plants here as you do in the model later on. Looks like a different response but hard to tell.

Response: We will split this in the figure. We do demonstrate in the results that C3 and C4 plants differ, which is an interesting finding from our study.

Line 203, please revise the table caption to better reflect the four models presented. The comparison to the results section and why the numbers of sites differ, plus the two models for NDF and CP are not clear.

Response: Thank you for this suggestion.

Proposed changes: The revised paper will include this additional information for the current models and those to be added.

Line 223, please clarify the figure explanation, it is hard to determine where the two scenarios come from in your temperature model for each size of livestock. Also consider some clarification in the methods section where you present equations for these (line 150).

Response: We agree that these changes will help clarify the paper

Proposed changes: We will clarify these points and update the methods section accordingly.

Line 223, I find the nitrogen addition discussion distracting and not needed for the main part of this paper. I think you could make a great point focusing on temperature and save discussion of nitrogen addition to the discussion. It complicates the methods section (data collection) and this is a small part of your database (8%), plus you find a temperature impact for the main species in your data, but not a nitrogen effect (making this a more complicated question).

Response: We feel that it is vital to include nitrogen in our analyses. Of the 803 rows of data we collected for this study, 535 rows included data on the quantity of nitrogen added. This is much greater (67%) than the 8% suggested above. We have discussed this comment in greater detail above.

Proposed changes: We await the recommendations of the Editor but suggest that the nitrogen component is retained.

Line 235, I like the analyses but the figures presented could be more informative. In this case these figures mainly represent areas with larger projected temperature change. Consider some alternative presentation, such as presenting the % change by continent, or other factor. A table or figure that presents changes by geographic location for different sizes of cattle would give much more information than currently presented in the text and figure. You could even consider ramifications of increased numbers of livestock in addition to the temperature impacts (as referred to in the discussion but not presented in the results).

Response: If the Editor approves this approach we would produce maps and tables which show predictions for specific regions. We limited our discussion to the ramifications of increases in the overall number of livestock, as the location of future increases in livestock production is highly uncertain and we therefore excluded this from our discussion.

Proposed changes: We would like to re-run our predictions using models based on studies of North American, European and Australasian cattle and we can therefore provide additional detail on geographical variation. We will discuss the issue of increased numbers of livestock. We will also clarify in the text that geographically explicit predictions of future changes in livestock number, species and breeds would be required to refine these predictions further.

Line 253, talk here a bit more about the assumptions in the model you have created (data sources, species variability vs community variability, forage type, etc.). Again, I think this is a valuable study and addition, just need to explain what additional information is needed to go beyond the "coarse projections."

Response: We agree with this comment and thank the reviewer for their positive comments.

Proposed changes: We would include more information on our assumptions in the methods and also provide a discussion of these issues.

Line 300, what is the magnitude difference of increased methane in housed cattle vs. the increase of methane from grass at warmer temperatures. Can you say your overall projection may increase?

Response: The methane emissions of housed cattle, which are fed a mixed ration of silage, grass and other components are generally much greater than those reared outdoors (O'Neill et al., 2011).

Proposed changes: We will present the magnitude of this difference in the revised manuscript.

Line 331, I liked the discussion overall, and think you cover a lot of good points about the conclusions of the study. Two additional factors to consider are the unknowns of the impact of increased C02 on NDF and CP for grass species (especially C3), how would this impact your conclusions. And secondly, consider a discussion about grazing pressure (which I know you excluded) changing community composition and species response, and those impacts to CD and NDF.

Response: Thank you for your positive and useful comments.

Proposed changes: We are aware of several studies of the effects of CO2 on plant nutritive quality and we would include these within our Discussion (e.g. Barbehenn et al., 2004; Roumet et al., 1999).

Responses to Reviewer 2

General comments: The study aims at investigating the relationship between forage quality, methane emissions from livestock, and projected future emissions. The topic is interesting, relevant and timely. The authors have done a good job gathering data to show the variability of forage quality for key quality parameters, plant species and across world climates. And that in itself would be useful material to be published (e.g. Fig. 1 and 2, Table 1) in a specialized forage science journal.

Response: We thank Reviewer 2 for these positive comments. We agree that we have collated a large dataset as summarised above.

Proposed changes: None required.

What I find less robust, is the use of statistical models derived with forage quality data, and the temperature under which the forage was sampled, to make (future) predictions of methane emissions by livestock. The analyses that would make this manuscript relevant for Biogeosciences are based on a few equations (derived from statistical analyses) which related methane emissions to the quality of the feed. Temperature is an explanatory variable that was used by other authors (Hirotaka Kasuya and Junichi Takahashi – see Asian-Aust. J. Anim. Sci. Vol. 23, No. 5: 563 - 566) to explain the intake of NDF, whereas methane emissions are driven by the intake of NDF. I find the extrapolation of these equations too week to make global predictions of methane emissions.

Response: The reviewers' comments do highlight some of the uncertainties in our analysis. However, while we agree with the reviewer that some potentially important aspects of the relationship are not represented in out model, it was not our intention to deliver a definitive estimate of the climate-forage-livestock emissions feedback. This would require significant model development and is beyond the scope of this paper. Instead we hoped to highlight this logically robust and potentially important process, and support it with empirical evidence. We agree that making predictions based on this one study is a major limitation of our study, but one which can be rectified.

Proposed changes: We have presented a detailed summary of our proposed solution above, i.e. to re-run our predictions using several other published models.

This sort of study, interesting and relevant, would be better substantiated using vegetation models that represent the physiological processes through which temperature would affect feed quality, and livestock models that would describe the effect of temperature on livestock (heat stress?) affecting the emission of methane. There are more weaknesses in the assumptions used for the study, which I describe below under specific comments. Unfortunately, I don't find this manuscript suitable for publication in Biogeosciences.

Response: We agree that vegetation models would offer a useful alternative methodology by way of comparison. However, we are not aware of any vegetation model which is suitable for this kind of analysis and on this scale, as the key vegetation properties that drive livestock methane emissions (e.g. NDF) are not represented in current models. We are also not aware of any complimentary livestock models that would be suitable. Incorporating these elements into existing models is a major task, albeit one which we hope to stimulate with this paper. Also, although we greatly appreciate the value of simulation models, we also know that they have their own limitations and uncertainties, making empirical studies a vital complement and source of information. Again, we would like to highlight that our aim with this paper is to present a potentially important process, which we believe is supported by empirical evidence. We hope that this manuscript inspires future work of the type the reviewer suggests.

Proposed changes: None suggested.

Specific comments L108: the authors used temperature at time of sampling, mean annual temperature (MAT) and monthly rainfall (MAR) over the past 10 years. The quality of the forage is associate to the current growing season, most like a seasonal and cumulative effect. So the use of an average long term (10 years) temperature of the temperature of the month of sampling seem inappropriate as predictors of feed quality.

Response: The use of MAT in this context allows us to link our statistical models to future climate models, which present predictions in terms of MAT. We agree that temperature during the month of sampling, which we have presented, and temperature during the growing season, which we have not presented, could also be valuable predictors. For the former, our output was very similar in terms of both gradient and intercept and this gave us additional confidence that our predictions are robust.

Proposed changes: We would be happy to compare the effects of MAT and growing temperatures in a new analysis, and then compare these values with those that we have used. We will present these either as a result or as a sensitivity analysis in an appendix.

L143-148: the use of equations developed for one experiment conducted in Japan, with a limited set of feedstuff (only 4 temperate climate species) to extrapolate global methane emissions seem largely inadequate for the purpose.

Response: This comment has been addressed previously. We will now calculate our predictions using a suite of models.

Proposed changes: As above.

L192: I would have expected a species effect in the analyses of NDF. Under the same climate and soil there will be plants with largely different values of NDF, and other quality parameters simply because of genetic differences.

Response: This point was also raised by reviewer 1 and our response to it can be found above. There will be substantial variation in species responses to climate and nitrogen, as supported by the data that we have presented. Please see our response above.

Proposed changes: We apologise that this point was not clear enough and will add detail to the manuscript, particularly in quantifying the random effect generated by species identity.

L232: the use of the selected statistical models derived from one single experiment, with future temperatures seem inappropriate to predict both future and actual methane emissions globally.

Response: As stated above, we intent to re-run our predictions using a suite of other models. However, we would like to point out that our statistical models were based on a dataset generated from many published articles.

Proposed changes: We will make the preliminary/discovery nature of our study more explicit throughout the manuscript, with caveats and cautions where necessary.

L247: I disagree with the authors. They don't describe here a climate feedback, but an artifact of the use of statistical models and projected temperatures. The relationship between temperatures and plant quality parameters is largely known in ecology. That explains the differences between ecotypes across the globe. However, the authors extend these relationships to the calculation of methane emissions, and that seems incorrect.

Response: We do not believe this is a statistical artefact. The logic is robust and the relationships presented are supported by previous published work. If the relationship between temperatures and plant quality is largely known and the relationship between plant quality and methane production well resolved then we believe that the overarching concept we have identified is robust. As summarised in a recent review paper, elevated forage NDF has been demonstrated to have a positive effect on methane production in cattle (Appuhamy et al., 2016).

Proposed changes: None suggested.

L264: the differences in NDF and CP across climate doesn't mean that ruminants are under nutritional stress. Livestock keepers manage different species and breed adapted to their climate across the globe.. And therefore it is not correct to use one equation derived for Bos taurus dry cows in Japan to predict global emissions of ruminants

Response: Whether different breeds of ruminants are under nutritional stress is outside the scope of our study. We identified that increasing temperatures are likely to reduce the nutritive value of forage grasses. It would be interesting for a further study to examine the effects of climate on nutritional stress in different species and breeds. However, we still believe that it is reasonable to propose that forage of reduced nutritive value may increase the prevalence of nutritional stress in livestock.

Proposed changes: We will discuss nutritional stress as part of a section highlighting the limitations of our study. We will also highlight that nutritional stress will vary between livestock breeds and species. Again, we have discussed our proposed solution to the issue of relying on one equation above.

References continue on the page below.

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