

Synthesis of peer review comments and author's responses on Defra BD5104 report '*Restoration of heather-dominated blanket bog vegetation on grouse moors for biodiversity, carbon storage, greenhouse gas emissions and water regulation: comparing burning to alternative mowing and uncut management*'.

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Peer reviewers: Professor Chris Evans (Centre of Ecology and Hydrology (CEH)), Richard Lindsay (University of East London (UEL)), Professor Guy Kirk (Cranfield University), Professor Peter Smith (University of Aberdeen), David Glaves (Natural England (NE))

The Department for Environment, Food & Rural Affairs has a rigorous process for assuring the quality of the science that it funds before publications, which includes using peer review to assess reports. If this process identifies issues, these are raised with authors and agreement sought before publication. In this report, it is our view that there are still issues outstanding between the reviewers' comments and the responses of the author of the report. Therefore, for transparency, we are publishing this summary of the peer review to be helpful to readers in recognising that this is still an area of scientific discourse.

The main concerns of the reviews centre in three key areas:

- the methods used for and interpretation of the peat carbon accumulation and past burn frequency data in Section 4.3;
- and on the landscape scale model used to determine historical carbon loss (4.6).

Further comments related to methods used in calculating peat shrinkage and expansion in Section 4.4.1 and covered more generic topics, such as the level of detail and the limitations of the study.

Some of the work contained in the final report has been published (Heinemeyer, A., et al. 2018; Heinemeyer, A. and Swindles, G. 2018; Morton P.A. and Heinemeyer A. 2019). A fuller examination of some of the areas of continuing discourse has been published in Evans et al. (2019) as a comment paper on the Heinemeyer et al. (2018) paper. The comment paper outlines the researchers' main concerns about the study design, methods, and interpretation and calls into question the validity of the papers conclusions on peatland carbon stocks and burn history (and in turn the related conclusions outlined in this report). Heinemeyer et al. (2019) provided a peer-reviewed response to the comment paper in which additional clarification and justification of methods and findings were provided.

In the following sections each reviewer's comments are summarised, then followed by a summary of the author's response. For the complete peer review comments and author responses see Appendix 16.

1. Professor Chris Evans (CEH)

The reviewer praised the scope of the project and the substantial body of work but expressed concern about the length and level of detail of the report, outlining that a non-expert reader would likely find the level of detail impenetrable and thereby diminishing the impact and benefit of the report to policy makers. The reviewer recognised the importance of comparing mowing- versus burn-management of blanket bog in the context of the management of the Pennines, but asked for greater recognition within the study of the fact that not all bogs are managed this way. Prof Evans also expressed concern about the lack of a truly unmanaged control site in the study, leading to a

comparison of two forms of detrimental management on three degraded sites and asked for this to be discussed further.

Prof Evans outlined his concerns about some of the methodology used in the project. This includes the measurement of respiration rates, methane (CH₄) and nitrous oxide (N₂O) on bare soils plots from which all vegetation was removed. It was recommended to not draw inferences about management impacts on anything based on these plots as it is unclear how the bare plots could inform understanding of management impacts. In addition, the validity of the extremely high CH₄ fluxes measured in the last few years of the study was called into question, pointing out that if these numbers were correct, there would be a strong argument for draining the UK's blanket bogs as a climate mitigation measure. The review also identified questionable results from the core-based analysis of carbon (C) accumulation rates; this issue was also raised by some of the other peer reviewers. Prof Evans recommended that these C accumulation rates should not be included in the report in the absence of more reliable methods to identify dateable horizons. Finally, the reviewer also noted the apparent mismatch between the core-derived C accumulation rates and the chamber-derived flux measurement which show the opposite treatment effect.

For a more detailed outline and reasoning behind the scientific concerns relating to the peat C accumulation, CH₄ fluxes, and the past burn history, please see the Evans et al. (2019) comment paper published in response to the Heinemeyer et al. (2018) paper which utilised data and methods discussed in this report.

Author's response

The author welcomed the overall praise, but disagreed with the concern raised about the length and level of detail of the report, outlining that this detail is required to comprehend the complex nature of the topic and noted that the holistic approach is a step-change in how to assess policy relevant questions about land management. The author recognised the need for an unmanaged control site at a catchment scale, but highlighted that this would have required considerable amount of additional resources and the existing plot-level control (unmanaged plots) therefore offered the most sensible approach. It was recommended that a future study would ideally include such a control site.

The author disagreed with the concerns outlined by the reviewer about the flux rates (CH₄ and N₂O emissions on bare soil plots) and the inferences drawn about the management impacts. The author outlined how the concerns are addressed and noted that these are due to misunderstanding or overlooking information provided in the report, while also dismissing the flux collar methods used by Evans et al. (2016) in the Defra Project SP1210.

In addition, the author disagreed with the reviewer's comment about the validity of the extremely high CH₄ fluxes recorded in 2015/16 while acknowledging the implications of the values. In defence of this position, the author provided details of other studies that found similarly high CH₄ flux values, particularly from a similar blanket bog study conducted around the same time at Moor House.

The author also disagreed with the conclusion that the peat C accumulation rates might be incorrect and defended the methods, outlining that this is what was achievable with the current techniques, while also highlighting that the peat C accumulation section has been peer-reviewed externally as part of the Heinemeyer et al. (2018) paper. Evans et al. (2019) provided a critique of this paper with Heinemeyer et al (2019) responding in a rebuttal of the concerns raised by Evans et al (2019). The author noted that there is a known contradiction between peat core and flux derived C

accumulation rate estimates (which was openly discussed in the report) and provided detailed explanation for this in the context of the report.

2. Richard Lindsay (UEL)

The reviewer described the project as an extremely well-thought out programme in terms of the experimental approach, praising its consideration of a very wide range of possible relevant factors and attempts to ensure that all these are considered and woven into the experimental design. The reviewer included a series of observations about the work and the report, outlining his concerns about the limitations and constraints imposed on the study being accurately and fully reported.

The reviewer highlighted that the report is not directly relevant to the 70% of blanket bog habitat which is not managed as grouse moor by burning. Instead, the study addresses forms of management which are only directly related to blanket bog managed as grouse moor where *Calluna* dominance is viewed as an issue and burning is currently the preferred management tool. In light of this, the reviewer asked that the focus of the report to be more clearly stated within the title and the introduction to the study. The reviewer noted that the wider implications of the study lie more with its investigation of burning as a management tool for blanket bog.

Two significant weaknesses of the study were outlined by the reviewer. The first relates to the lack of detail provided about the physical and ecological nature of the study sites, study plots, and quadrats rendering it impossible to relate the results obtained to actual conditions on the ground. The reviewer recommended the inclusion of aerial images for each plot, which would give a better idea of the physical nature of the ground within each plot. The second weakness relates to the lack of critical assessment of the vegetation data in ecological terms. The characteristic diversity of peat bog systems, or of poor-fen systems was not addressed at all. Similarly, analyses on the responses to management interventions appear to treat all species the same even though they are not. To address this point, the reviewer, with input from the main author and PhD student, Phoebe Morton, undertook an ecological re-working of the vegetation data presented in Appendix 3a of the report.

In addition, the limitations of a four- or five-year study was highlighted by the reviewer, and stressed that it is important that funders manage expectations of what a short-term study can demonstrate and to consider ways of extending the work. The reviewer stressed that it is also important to recognise that the results obtained here are of a very preliminary nature in terms of the ecosystem responses.

The reviewer also expressed concerns about the methods used and the interpretations made in Section 4.3 on past burn/fire frequency and peat carbon accumulation. The interpretations of the spheroidal carbonaceous particles (SCP) peaks are questioned, along with the level of physical assessment of the peat (including macrofossil content) from each site, with implications for the interpretation of the results.

Author's response

The author welcomed the positive overall comments on the report. The title of the report was amended to include 'heather-dominated...grouse moors' to reflect the reviewer's comments on the general focus of the project. However, there was disagreement around the wider implications of study; in the author's view the implications of mowing as a management option needs to be assessed in light of apparent take up of this option on grouse moors in recent years.

The author agreed with the limitations of the short-term study noted by the reviewer and supported the need for long-term monitoring is included in the report. The author noted that the limitations deriving from the preliminary nature of the results, especially in terms of the ecosystem responses, were acknowledged in the report. These were attempted to be countered by the landscape scale modelling and the C stock analysis.

The significant weaknesses mentioned by the reviewer were addressed by outlining the financial limitations of the study which prevented the provision of detailed aerial images for each plot, and to carry out an ecological assessment. The author made available on request the pictures taken for the project, and expressed his gratitude for the additional ecological assessment R. Lindsay completed which can now be found in Appendix 3a.

The concerns outlined about Section 4.3 were countered mostly by the defence that this section has undergone peer-review for the Heinemeyer et al. (2018) paper. In addition, the author noted that physical assessment of the peat is not necessary for the calculation of peat accumulation while noting resource limitations that prevented macro-fossil analysis (which was ultimately not necessary to test the hypotheses on carbon accumulation).

3. Professor Guy Kirk (Cranfield University)

The reviewer was asked to comment on the methods used in Section 4.4.1. 'Peat shrinkage and expansion', with a particular focus on the way methods and results from Bellamy et al. (2005) study were utilised in this project and the implications of this. The reviewer was concerned that circular reasoning was used to calculate an apparent change in peat depth, and identified the use of incorrect numbers for the rate of change in organic C per unit soil mass for peat soils. It is highlighted that the data for peat soils in the Bellamy et al. (2005) study are less reliable than the rest of their data, as only 86 out of 2,578 resampled National Soil Inventory sites were classified as peat soils. The reviewer concluded that the basis for the calculations in the report involving the Bellamy et al. (2005) data is weak.

Author's response

The author rejected the claim about a circular reasoning, noting that the same approach was used in the quoted Bellamy et al. (2005) paper. The value chosen for the rate of change in organic C per unit soil mass for peat soils was also defended by claiming that it represents 'true' peat as opposed to 'peaty' soils (as in this study). It was noted that the reliability of the data for peat soils in the Bellamy et al. (2005) study was irrelevant in relation to this study. The methodology was clearly outlined and has since been published (Morton and Heinemeyer, 2019). In conclusion, the author noted that the reviewer assessed this section of the report and the corresponding appendix possibly in haste and was surprised by the critique, claiming that it does not stand up to the outlined methods and calculations, and that the main issue seems to be a lack of understanding around the 'apparent' C stock change.

4. Professor Pete Smith (University of Aberdeen)

The reviewer's comments focused on section 4.6. 'Landscape scale model scenarios'. The evaluation of the model for soil C change undertaken by the reviewer suggests that the model outputs need to be treated with extreme caution until more data can be found against which to test the model and confirm its adequate performance. Without this, there can be little confidence in the explanatory/predictive capability of a model. It was noted that the arguments presented by the author form only a weak test of model performance and there does not seem to be a systematic

assessment of separating data that can be trusted and those that cannot. Based on these points, the reviewer notes that conclusions based upon lost C due to management are therefore uncertain and unsafe. The reviewer notes that this would not pass the peer review process of a scientific journal even if previous model and field measurement comparison done by Heinemeyer et al. (2010) did.

The reviewer also questions the credibility of the amount of soil C that has been lost in the UK due to management as determined by the model simulation, especially in comparison with global estimates of historical soil C loss rates even accounting for known differences between soil types. The reviewer notes that the author's claim of peat loss 10 times the estimated global average requires sufficient and robust evidence which this study lacks.

The reviewer concluded that while some evaluation of the model for water table depth provides some comfort that the model is performing adequately for predicting water table depth, the evaluation for the model's performance for soil C change is not adequate, and the small amount of evaluation for soil C actually shows inadequate model performance with respect to predicting soil C levels (particularly under unmanaged conditions). Until better model performance can be demonstrated by thorough evaluation of the model against measured data for soil C change, the findings related to historical soil C loss and soil C loss due to management should be considered unsafe according to the reviewer.

Author's response

The author claimed that the reviewer undertook only a brief and unscientific assessment of the modelling section. The author acknowledged the overall spread of the carbon stock data and argued in favour of the chosen methods regardless of the uncertainties and defended the comparison of the modelled data and the up-scaled estimates. The author noted that the national soil C levels are estimates (i.e. NSRI data) and are not measurements, therefore these cannot and were not intended to be directly compared to modelled data as part of an evaluation. In addition, the author noted that the model concept questions existing modelling studies and pointed towards a successfully peer-reviewed publication by the author that compared model estimates and field C stock measurements (Heinemeyer et al. 2010).

The author dismissed the global studies referred to by the reviewer as irrelevant because these are skewed towards mineral soil and low soil organic C (SOC), excluding peatlands. The large extent of peatlands in the UK was highlighted along with calculations and other studies to support the level of peat losses identified in the report. In addition, the lack of considerations of the impacts of past land management on SOC loss in peatlands in the global studies was also noted, along with the current lack of incorporation of the process of cutting and burning peat for fuel and agricultural cultivation. The author stressed that this project should be seen as a necessary first step to progress the peatland studies towards the inclusion of these processes into modelling.

5. David Glaves (NE)

The reviewer agreed with Richard Lindsay's comments about the limited interpretations of the findings for the whole vegetation community, and from an ecological perspective in terms of blanket bog restoration. The reviewer identified a couple of areas not addressed in the study. These are (i) an ecological interpretation of the balance between blanket bog and other components of the vegetation community more characteristic of fen and other habitats/vegetation types and (ii) how this balance has changed over time across the treatments. The reviewer recommended the inclusion of an additional ecological assessment by R. Lindsay to the report or in an Appendix given the value it adds to the study.

Concerns were raised about the differences in peat C accumulation rates between the stock and flux methods, bulk densities and hence the difficulty of determining the age of sections of the peat profiles. Further review was recommended to address the issues, and ultimately it was suggested that consideration might be given to the omission of the part dealing with the peat C stock study.

Similarly, concerns were raised about the performance of the landscapes scale model in relation to soil C change and historical and management-related loss, with an indication of bias in over-predicting soil C and an apparent lack of validation of predictions. The reviewer shared the concerns about this section of the study expressed by other reviewers and recommended that consideration should perhaps be given to excluding this section from the report, especially as it seems to be beyond the project aims and objectives.

Author's response

The author noted that an ecological interpretation as outlined by the reviewer was not part of the project design due to budget constraints but welcomed and supported the inclusion of R. Lindsay's additional work as Appendix 3a.

The author rejected the concerns raised about up-scaling of the C budgets and differences between C accumulation results between the C flux and C stock methods, in line with his comments on C. Evans' and R. Lindsay's concerns on this. The author restated that this section underwent peer review as part of a publication in a journal noting that an omissions of this section would not be justified.

Similarly, the author also rejected the concerns raised about the landscape scale model's assessments of historical soil C loss outlining the points made in response to Prof Smiths' comments.

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