The authors would like to thank the referee for his/her time and valuable comments. We have addressed all the referee's comments and suggestions below and revised the manuscript accordingly as follows:

Referee comment
This paper described measurements of soil GHG fluxes from drained and afforested peatland plots, and compares with neighbouring undrained and unplanted plots, as well as a nearby near-pristine site. The work is unusual, being a long-term experiment and well replicated. The paper is well-written and fairly concise, and addresses a pertinent topic.

Author response
Minor point: we also measured from undrained and planted plots.

Referee comment
1- *The major weakness of the study is that the comparison does not include any direct measurement of uptake of CO_2 by the plants, either tree or understory, yet this is a major term in the budget.*
2- *There is some discussion as to what this term might be, based on measurements at other peatland sites and forest yield tables, but this is fairly speculative, and not clearly explained how this was combined with the measurements that were made. I don't follow how the direct measurements say that n-pris has a higher net GHG flux than DP, yet conclude it might have the half the GHG flux when Ps uptake is accounted for. Do the authors assume a steady state where efflux = influx? This would make little sense. Large CO_2 effluxes probably correlate with influxes, but quantifying the imbalance brought about by changing water table etc is necessary to answer the questions posed.*
To be harsh, I don't think we can actually draw any conclusions about the net effect of drainage or restoration on the GHG balance from this work - we still don't know if it is a good thing or a bad thing. This is reflected in section 5.4, which doesn't actually say what the implications are. So, the paper could focus on just CH_4 + N_2O, as this is a simple comparison, with the CO_2 effluxes discussed but made very clear that their interpretation is by no means straightforward.
3- *Or, the details of the calculation by which the net GHG balance, including photosynthetic uptake, needs to be much more clearly explained, probably tabulated.*

Author response
1- We agree that it is a limitation of this study that we were not able to measure C uptake by vegetation concurrently with soil effluxes, but there are obvious major technical and practical obstacles in doing this on this time scale, for small plot sizes, with microscale heterogeneity and across multiple treatments that include mature trees.
2- As the referee points out the CO_2 uptake by vegetation is a major term in the budget so we do believe it is useful and necessary to discuss the soil GHG fluxes in comparison with likely magnitudes of vegetation CO_2 fluxes. The referee is wrong as our calculation of NEE for the tree-planted treatments did not use yield table information, but used actual field mensuration of tree dbh, mean height and density, taken from detailed harvest planning information for Flanders Moss at forest sub-compartments scale and species-specific empirical biomass-dimension relationships to estimate C stock. This was then averaged over the length of time since planting to estimate a mean annual growth rate and NEE, (although as stated this does not include accumulation of leaf, branch and root litter).

We do not agree that they are 'speculative' as all the values we cited were from well defined peatlands in Scotland (Lindsay, 2010; Levy, 2009; and Billett et al., 2010) with similar climatic conditions, and were thus relevant to our study. Our estimate of the effect of drainage on total net GHG emissions is straight forward as it equals the difference between total GHG effluxes from similar planted sites either with or without drainage.

Some difference in the below canopy vegetation CO_2 uptake might be expected but this will be minimal compared to that of the trees. For the discussion of the effect of restoration on total net GHG emission, we have revised the manuscript to focus on what can be derived from our results, and only indicated the NEE from the literature for comparison. Therefore we have discussed the implication for restoration which is that whichever plausible NEE values are used for restored peatbog the results indicate that the total net GHG effluxes are likely to increase if previously afforested peatland is restored. We note a similar implication of changing water table depth after restoration was discussed in a paper on measured UK peatbog CH_4 emissions published by Levy et al. (2012) while our manuscript was in review. Both the abstract and conclusions are revised accordingly.

3- Following on from the above, we have clarified the calculation of NEE in the revised manuscript in section 5.4 and in Table 4. We have revised the manuscript to focus our discussion on soil effluxes, as suggested by referee 1, and only at the end of the discussion (section 5.4) we have included NEE values from the literature for comparison.

Referee comment

Other work has derived estimates of the effect of drainage/restoration expressed as kg CH_4 m^{-2} y^{-1} per cm change in water table. Could this be calculated for comparison?

Author response

While we know this approach has been taken elsewhere (e.g. Levy et al. 2012) , Fig 6 shows that the relationship is a threshold response as in some other studies (i.e. the change with water table depth was not linear within each treatment) and therefore a summary number like that suggested by the referee is not possible for our study. We have mentioned this point in the discussion section 5.1.

Referee comment

How do the $Fch4$ rates compare with other UK studies on peat? Include some quantitative comparison with existing UK syntheses: Baird AJ, Holden J, Chapman P (2009) A Literature Review of Evidence on Emissions of Methane in Peatlands. Defra Project SP0574. pp Page, University of Leeds. Bussell J, Jones DL, Healey JR, Pullin AS (2010) How do draining and re-wetting affect carbon stores and greenhouse gas fluxes in peatland soils? CEE review 08-012 (SR49). Collaboration for Environmental Evidence: http:\\www.environmentalevidence.org/SR49.html. Collaboration for Environmental Evidence Systematic Review. Levy PE, Burden A, Cooper MDC et al. (2012) Methane emissions from soils: synthesis and analysis of a large UK data set. Global Change Biology, 18, 1657-1669. doi: 10.1111/j.1365-2486.2011.02616.x.

Author response

The authors are well aware of these references and participated in e.g. the original workshop where the Baird et al. (2009) report originated and have also compiled a literature review on GHG (See chapter 4 in Morison et al. 2012). In our original manuscript we only compared the results with those from similar comparable sites i.e. deep raised peatbog or afforested peatbogs rather than e.g. blanket bog, fens or grazed or otherwise managed sites (Billet et al, 2004; Worrall et al. 2009 and Laine et al. 2007) or studies at different scales, or from those detailing microsites such as lawn, hollow or hummock (e.g. Laine et al 2007 and Bussell et al., 2010), and with data that is considered to be reliable. However, we have now included a comparison with the relevant data from the Bussell et al. 2010 review (section 5.4), and have included comparison with the range of measurements reported in the recent synthesis by Levy et al. 2012 (section 5.1), which has been published while this manuscript was in review.

Referee comment

The statistical analysis is applied to the median of the replicates in a block because of some high values. However, this merits some discussion - are these high values real (explicable) or not? Was a threshold value used to exclude unbelievable values? This could be presented as

a sensitivity analysis - how do results differ when the analysis is applied to the raw data, block medians or block means etc?

Author response

Comparing the mean flux of 3 replicates to the median flux for all the gases and across all treatments and blocks resulted in a mean flux estimate up to 8% higher compared to the median value. This relatively small difference (in comparison to flux differences between applied treatments) is the result of a positively skewed distribution of flux values. There appear to be a number of "hot-spots" that result in significantly larger values than much of the remainder of the plot. Although hot spots are expected with chamber measurements and already known (e.g. Christensen S, Simkins S, Tiedje JM, Spatial Variation in Denitrification - Dependency of Activity Centers On the Soil Environment. Soil Sci Soc Am J 54:1608-1613, 1990; and Dinsmore KJ, Skiba UM, Billett MF, Rees RM, Drewer J. Spatial and temporal variability in CH4 and N2O fluxes from a Scottish ombrotrophic peatland: Implications for modelling and up-scaling. Soil Biology and Biochemistry 41:1315-1323, 2009) this difference between median and mean flux is of less importance than whether the hypothesis being tested (i.e. H0: no difference between treatment fluxes) leads to different results depending on whether the median or mean is used. However, ANOVA analyses give very similar results for both mean and median estimates. As expected the model error fitting diagnostics (distribution and normality of errors etc.) are a slightly better for the fit of the median flux as this statistic doesn't include the occasional "spikes" in flux level that are a feature of the mean flux estimate.

Referee comment

The static chamber method is rather error prone, especially when using only three time points. A linear increase was assumed, but particularly in the case of CO_2 the response is often nonlinear. Can some more evidence of quality control be provided? eg. what were the r2 on the regressions, do nonlinear fits change the results? There are many papers on the topic and the appropriate analyses should be done. See for example: Kroon PS, Hensen A, Bulk WCM, Jongejan PaC, Vermeulen AT (2008) The importance of reducing the systematic error due to non-linearity in N2O flux measurements by static chambers. Nutrient Cycling in Agroecosystems, 82, 175-186. doi: 10.1007/s10705-008-9179-x. Pedersen AR, Petersen SO, Schelde K (2010) A comprehensive approach to soil-atmosphere trace-gas flux estimation with static chambers. European Journal of Soil Science, 61, 888-902.

Author response

This is an important comment and we agree that flux calculations based on static chamber results are error prone due to non-linearity in the concentration increase with time after chamber closure and more time points would be better. However, this was not possible on every sampling day due to the scale of the measurements and replications, but we have tested our closed chambers for linearity at the start of the experiment by measuring the gas concentrations at 0, 5, 10, 20, 40, and 60 minutes intervals after chamber closure. We have added this information to the manuscript (Method Section 3.1) for clarification. Also analysis of the regressions for the flux calculations for the whole data set showed approximately 70%, 90% and 50% of the CH_4, CO_2 and N_2O flux respectively had an R^2 better than 0.8. We have also tested our results using the HMR flux calculation software (Pederson et al, 2010) indicated by Referee 1 but this did not change the overall results.

According to Conen and Smith (2000, An explanation of linear increase in gas concentration under closed chambers used to measure gas exchange between soil and the atmosphere, European Journal of Soil Science, 51, 111-117). the differences between fluxes calculated from increasing concentration within the chamber's headspace and that expected under undisturbed conditions are due to a proportion of the gas produced being stored within the soil profile while the chamber was in place, due to changes in soil diffusion gradient. However, they indicated that the discrepancy caused by this effect increased with increasing air-filled porosity and decreasing height of the chamber. This might explain the linearity in the gas concentration increase with time for our chamber design (0.25 m height by 0.16 m^2 area) and the expected low air-filled porosity throughout the period for our peatbog sites.

GWP applies to the mass of an emitted gas (the radiative forcing relative to CO_2 over some time span), not to a site. This should be renamed net GHG flux (kg CO_2-eq m^{-2} yr^1).

Author response
We agree and have revised all relevant text in the manuscript and Table 4 to replace GWP by total net GHG emission and indicated that this was calculated using the global warming potential (GWP) of the three GHGs considered here.

Referee comment
The recent Fluxnet CH_4 workshop agreed that units of nmol CH_4 m^{-2} s^{-1} should be the standard unit, as it conforms to SI and is unambiguous. Neither tonnes nor hectares are SI units. Fluxes are expressed here as per day and per year, yet the integration from the measurements up to this level is not described.

Author response
All the units in the manuscript (text, tables and figures) have now been expressed as g or mg per m^{-2} day or year to conform to SI units as suggested. The integration of the measurements to annual fluxes was described in the section 3.3 Statistical analysis (pg. 7321, line23), but we further revised the text to clarify this.

Referee comment
Section 3.3: "temperature/treatment interaction" may be confusing to non-statisticians - probably better called "treatment-specific temperature coefficients".

Author response
Manuscript revised as suggested.

Referee comment
Does Fig 5 show raw data or block medians?

Author response
Fig 5 now revised to show the data are block means.

Referee comment
Fig 6 would be better as x-y scatter of Fch4 vs WT, with different symbols for treatments, normalised to T=10.

Author response
As indicated in the manuscript (pg 7329, line 12) there were no significant differences in the soil temperature between the treatments, and we are uncertain of the need or merit in normalising to a particular temperature, when the T variation is a seasonal one, with other possible confounding variable changes. Also the main relationship between CH_4 fluxes and WT were between the treatments (rather than within each treatment) and showed a threshold for uDuP and n-pris treatments which has now been further clarified in the manuscript (section 5.1). Therefore, we think the current graph gives a clearer indication of the overall impact of WT on methane variations and wish to keep it as it is, but all units have now been changed to conform to SI as suggested.

Referee comment
Fig 7 y-axis units are missing from the axis label.

Author response
Fig 7 revised accordingly