Review

Derivation of greenhouse gas emission factors for peatlands managed for extraction in the Republic of Ireland and the UK

by D. Wilson et al.

General comments

Peat extraction is a major land use category of peatlands in some countries, and the recent emission factor of the IPCC 2013 Wetland Supplement is indeed based on only a few studies mainly on boreal sites. Therefore, new emission data on peatlands managed for extraction is very welcome and fits well into the scope of Biogeosciences. However, there are some methodological issues which need to be addressed before the manuscript can be published.

1) The methods section is very brief and needs to be extended.

2.5.1 Field measurements:

- How often did you measure Reco and GPP at each measurement date?
- Did you ensure that the maximum PPFD was reached at each measurement date to avoid an extrapolation beyond measured values?
- Did you measure at different PPFD levels by shading or at different times of the day?
- For the NEE measurement: were the chambers also cooled (e.g. by icepacks) and the temperature measured inside the chamber to avoid more artificial conditions than necessary?
- What kind of chambers have been used for NEE measurements?
- How is the light transmissivity of these chambers (usually, it does not reach 100%) and was this accounted for when modelling GPP? It should be included in the GPP model as otherwise GPP might be underestimated.
- Why didn't you chose a site under ongoing (or recently ceased) industrial extraction?

2.5.2 Flux calculation

- “GPP was calculated as NEE minus Reco”: Which value of Reco was used; the nearest value in time or the one calculated by the model? If there was only one Reco measurement per measurement date, using the actual measured value could potentially induce some uncertainty as the time lag between the Reco measurement and the first GPP measurement is not clear and as there is probably a strong temperature-dependent diurnal variability of Reco.

2.5.3 Modelling

- Modelling GPP should be included in this sub-chapter
- Why was this specific GPP model chosen, and not a Michaelis-Menten type model, which is frequently used for GPP?
- How was “plot-specific LAI” modelled?

2) My major concern, however, are the NEE results of the GP sites. The vast majority of the measured fluxes (Figure 3) show an uptake of CO_2, especially at site DP1, but all annual balances (Table 2) show a net release. How do you explain these results?
In my opinion, there are several possibilities:

- The measurements themselves are biased: but as they were regular and rather frequent, I would suggest that this is not the reason for the surprising results.
- There are problems with either the Reco or the GPP model. Here, I would recommend checking the following issues:
    - Is there extrapolation beyond the range of measured temperature and PPFD data?
    - Regarding the GPP model: I doubt whether one "general model" and especially one (equation 4) not including the LAI could predict correctly the NEE for the whole year. Therefore, pooling data of several measurement campaigns might be an alternative if the LAI is not included.
    - Given the obvious discrepancy between measured NEE values and modelled sums I would strongly suggest that the authors check their model by e.g. a cross-validation approach (leaving one measurement date out at a time and trying to predict both Reco, GPP and NEE by the remaining data).

3) Measuring emissions from burning peat is a valuable addition to the manuscript. However, I'm not sure whether these numbers are to be used for L_{fire} (Wetlands Supplement) – is fire an issue for non-vegetated peat extraction sites in Ireland and the UK? Otherwise, wouldn't be burning of peat reported in the energy sector (and how is it done now if there are no numbers available)? These issues should be made clearer especially for those not familiar with reporting methodologies.

Specific comments

Abstract
I have recently experienced some discussions during which the relatively low EFs for peat extraction sites (at least compared to agriculture) tended to raise the rather questionable opinion that peat extraction is a climate-friendly activity in peatlands. Therefore, it would be helpful to clearly include a statement on the system boundaries of your study, especially as you included peat burning but no horticulture.

Results
Both WT and VMC enter the Reco-equations without any additional model parameter. That suggests that a) there is no optimum water level or moisture for respiration which I would expect to exist and b) respiration is highest at that highest VMC, i.e. at saturation which is rather surprising. Could you comment on this?

Discussion

Generally, I do not really understand why the emissions are lower than in other studies: Your study areas are relatively warm with mild winters (at leat compared to boreal sites), the physical environment is with maximum soil temperatures of 28°C not too extreme, and at least some of the sites are characterised by rather narrow CN ratios and sub-neutral pH-values. How is the WT compared to previous studies?

In my opinion, the choice of sites might be a reason why the emissions are lower than in other studies: Easily degradable organic matter would have been already gone, while during abstraction there would have been also less decomposed “fresh” peat at the surface during certain periods of time. Furthermore, your chose “unvegetated microsites” for the measurements, which suggests that parts of the peatlands are already re-vegetated. Probably, these microsites are unvegetated for a reason (peat quality, water repellency,...), and these conditions might also limit microbial activity. Are there any obvious differences between the vegetated and non-vegetated sites?

Effects of drainage level: To my understanding, the effect of the WT on single fluxes (i.e. the Reco dynamics) and the effect on the general emission level shouldn't be mixed up. While at the scale of single fluxes, effects of the WT might be obscured by a co-variance between WT and temperature, or the activity of the vegetation, the general height of the emission might indeed be influenced by the WT (which seems not need to be the case in the study). However, at the scale of single fluxed, I do not think that concluding that there is generally (nearly) no effect of the WT is not valid unless fluxes from all sites are combined into one model.

How do you differentiate between areas influenced by domestic peat cutting and otherwise disturbed peatlands with similar WT or vegetation which not used for agriculture or forestry? To do so, you would probably need to define a zone of influence. You briefly mention this problem in the discussion section, and I agree that there will be a problem with the activity data. Do you see any way forward to identify domestic peat cutting areas?

Tables and Figures
The tables and figures are generally of good quality.

Table 1: Please include the WT and the vegetation at the DP sites.

Figure 2: I don't think this figure is really necessary. If you should chose to keep it, please use percentages instead of absolute counts as due to the different lengths of the study periods the sites are hard to compare by absolute counts. In this case, please add the range of temperatures at which measurements took place.

Overall, the manuscript is clear and well-written, but, in some cases, uses IPCC-related jargon. Therefore, I would suggest to have the manuscript read by a scientist not familiar with National inventories or reporting issues. Similarly, the discussion should focus a bit stronger on those results interesting for scientists not involved with emission reporting.