Reply to referee comment (#1 by Joshua Ratcliffe) on HESS-2021-585

We thank Joshua Ratcliffe for his detailed review and constructive comments. In the following, our response is given in italic, and the referee comments in normal text.

In this manuscript the authors investigate how the management of a beaver dam has affected the lateral flow of water, and the vertical CO2 fluxes, from a well known and long running GHG monitoring site at Mer Bleue peatland. They use a modelling approach to provide several 'what-if' scenarios to model what the CO2 flux may have been without any alteration of the beaver pond level. This is a very innovative paper which addresses an important and overlooked aspect of the peatland GHG flux, i.e. what is going on at the margins. It will additionally be good to see this published as it concerns Mer Bleue, the longest running EC record on a boreal peatland and a site from which much of our understanding of boreal peatland ecology and gas flux is derived.

I really like the approach the authors have used to tease out the effect of the beaver pond from the background variability. I think it is clear from the measured data in Figure 2 and Figure 3 and Figure 6 that there was a major change in the ecosystem corresponding with the rise in the water level of the beaver pond, the coup model nicely confirms this as a mechanism.

Importantly, this confirms the wide-ranging ecological effects water table management can have hundreds of meters from the flux measurement site. This has important implications for other sites and boreal ecosystems, including other long-running flux measurement sites. I know some researchers who work on relatively degraded peatlands, with low horizontal hydraulic conductivity, who are sceptical that water level management hundreds of meters away can have any affect at all and I think it would be good to discuss how Mer Bleue might contrast to other peatlands. See my comment to this further below.

The coup model itself preforms adequately, and the authors discuss the limitations of the model in appropriate detail.

While I generally think this is a very good paper, I thought it was lacking some detail which would ensure it's comparability to other sites and situations. While hydrological feedbacks are mentioned, it's not really clear just how strong these feedbacks can be. In the discussion I would like to see a short paragraph about the horizontal hydrologic conductivity and how Mer Bleaur compares to other peatlands and how we might expect this to change if the water table was to undergo sustained lowering for a period of decades or more.

Response: *We agree that it would be interesting to discuss the broad implications of our results, for example "how far away does a change in the marginal stressor impact net CO_2 uptake of the bog". For that, we will do a back-of-the-envelope calculation to show the degree of influence as a function of distance from the disturbance. Currently, the sensitivity of hydraulic gradient to the beaver pond water level in Fig. 7 was discussed only by changing the water level, but in the revision we will discuss the influence of distance from the CO_2 measurement site (drainage distance), which also, affects the lateral hydraulic gradient on the GPP, ER and NEE fluxes.*

Moreover, in the revision, we will also conduct an uncertainty analysis to quantify how the uncertainties of the parameters in saturation conductivity (and other key model parameters, see the response to reviewer #2) influence the hydraulic feedback and C fluxes.

One thing that bothers me rather a lot is that the increase in ER with the beaver pond level listed in Figure 7 seems to contradict what was established in Lafleur et al., 2005 (Ecosystem Respiration in a Cool Temperate Bog Depends on Peat Temperature But Not Water Table) where water table fluctuations were found to have little or no effect on ER, this is maybe due to Lafleur et al., 2005 only having the early data available, before the beaver dam raising, but I would like to see this addressed. At the moment this paper is not cited.

Response: *We thank the reviewer to point this out. We have investigated this further and will include discussions in our revision to make it clear. We believe the different relationships between these two studies can be explained:*

First, Lafleur et al. (2005) study was conducted in the earlier years of the site, and in their studied five years, water table depth fell to 60 to 70 cm beneath the surface in late summer. At this depth, the peat is relatively well decomposed and changes in the water table do not mean pronounced aerobic-anaerobic transitions. Thus, a decline in the water table would not increase respiration much.

Second, these two studies have different temporal scales. In the Lafleur et al. (2005) paper hourly ER data (also note only nighttime data were included) were used but Fig. 7 in our study represents the average of 21 years of fluxes (ER were partitioned from NEE and thus included daytime and nighttime). In addition, the earlier study does not include the transition from low BP water levels to higher BP water levels, of which ER responses to changes in the water table more pronouncedly. Thus, a direct comparison between those two is not straightforward.

Additionally, I have a comment about the measured data. I do not believe (nor would it be correct) that the methodology used to process the fluxes is the same as in Roulet et al., 2007. There have been several large changes in best practice for flux processing in the last 15 years that I am sure the authors are aware of. I would like to see the detailed method for flux processing included in the SI.

Response: *For consistency, we largely retained the flux processing methodology from the 2007 paper. The main difference with methods commonly used today and facilitated by LI-COR's EddyPro software is that spectral corrections were not applied. Spectral corrections increase the magnitude of the 30 min fluxes but also add the potential for bias, particularly on shorter towers where closed path instruments and where maximization of the covariance to assess time lags are employed (e.g. Peltola et al.* Atmos. Meas. Tech., 14, 5071–5088, 2021*). We will add a section in the SI to describe the method for flux processing and the potential uncertainty associated with that processing during our revision.*

Could the authors state why the flux simulations in 2013 and 2017 performed so poorly compared to other years?

Response: *We believe the reviewer was commenting on the 2012 and 2016 years which both have measured and modeled the lowest summer WTD (Fig. 3c) but show lower uptake compared to the measured NEE data. The model simulates the drier years in the high disturbance period but show deviations for these two years, which have the highest WTD seasonal fluctuations of the 21 years. The measured NEE data suggest over these two dry years NEE was reduced but not as much in the model. We suspect this can be caused by the parameter uncertainty (e.g., vegetation, roots distribution, water uptake, etc.) which was also raised by the second reviewer. We will include those parameters in a GLUE calibration and discuss the model-data biases in detail in the revision.*

I wish the authors the best of luck with the revisions.

Figure 2: Should be clear what is generated data and what is measured. Suggest the generated data is presented in a different colour/style The measured water level at the peatlands also looks rather spiky and a bit suspect (2006 and 2007). Please check for and remove outliers if present. It would be good to state the temporal resolution in the caption.

Response: *We will revise the figures and figure caption in the revision.*

Other than this I have some minor comments the authors may wish to consider.

L9: should be “feedbacks”

L12: consider “lateral flow of water”

L29:30 This range listed is too low, see the following. Suggest an upper range of ~200 g m-2 yr https://doi.org/10.1111/gcb.13424 https://doi.org/10.1111/j.1365-2486.2010.02378.x.

Response: *We will edit these in the revision.*

L54-L55: There are a few analogous studies looking at road construction and how that have raised and lowered water table levels for instance: https://doi.org/10.1007/s10021-016-0092-x

Response: *We will add this reference in the revision.*

L83: Should mention that it is a downward slope (being a bog I would assume so…)

L101 Please state the total number of periodic measurements (n=?)

Response: *We will edit these in the revision.*

L103: This is probably fine, given the change in mean across treatments. Please differentiate this data somehow in the plots (particularly in Figure 2)

L207: Really nice to see these numbers!

213: suggest “the disturbance level”

226: Water table is relatively meaningless over winter, not really a problem.

Response: *We will edit these in the revision.*

240: Again, it would be worth discussing (briefly) how the shrub dominance of GPP at Mer Bleue might cause it to respond differently to other peatlands, see discussion in https://doi.org/10.1016/j.scitotenv.2018.11.151

L339: Other sites see very large changes, see https://doi.org/10.1111/jvs.12602

L341: suggest also the following as a site where there has been major changes in GPP and plant cover following WT lowering https://doi.org/10.1016/j.scitotenv.2019.134613

Response: *We will revise the texts and refer to the reference in the revision.*

L384-385: These models are almost totally useless without incorporating feedbacks.

L387: This is a reasonable statement, I agree

L390: I’d say this is not as well established as it might be believed there are odd sites such as pocosin and restiad peatlands where C accumulation can be high even under a very low water table again see: https://doi.org/10.1016/j.scitotenv.2018.11.151

Response: *We will rephrase to make it clear in the revision.*

Lafleur, P., Moore, T., Roulet, N., and Frolking, S, 2005, Ecosystem respiration in a cool temperate bog depends on peat temperature but not water table, Ecosystems 8, 619-629.

Peltola, O., Aslan, T., Ibrom, A., Nemitz, E., Rannik, Ü., and Mammarella, I.: The high-frequency response correction of eddy covariance fluxes – Part 1: An experimental approach and its interdependence with the time-lag estimation, Atmos. Meas. Tech., 14, 5071–5088, https://doi.org/10.5194/amt-14-5071-2021, 2021

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On behalf of all authors