Authors' Response

We are grateful to both of the reviewers and the associate editor for their constructive comments on our manuscript. We have revised the manuscript according to the points raised by both reviewers, and have modified the text and figures accordingly. Our responses to each reviewer's comment are listed below in blue italic font.

Reviewer one comments:

General:

The authors measured both DOC and POC. This information is somehow lost as only the TOC fluxes are reported in the abstract and most of the figures. In the abstract, the percentage of DOC should be indicated instead of "TOC fluxes [*: : :*] were dominated by DOC". The relative proportion of DOC and POC in the TOC flux should also be added in Figure 3

The percentage of DOC is now indicated in the abstract (Line 7: "(TOC) fluxes from the plantation second and third order drains were dominated (91%) by dissolved organic"

*A division has been added to Figure 5 (I think that was the graph the reviewer meant, now Figure 6) that separates the TOC flux into the relative DOC and POC contribution. An explanation has been added into the Figure 6 caption: "*Horizontal bar lines represent contribution of DOC (bottom segment) and POC (top segment) to the overall TOC flux".

Abstract:

The values of TOC flux reported in the abstract should be tempered by the fact that the study is based on a single year survey, strongly influenced by an el Ninõ event, and therefore lower discharge than usually observed.

*The observation that this study was carried out on a single year and during an El Ninõ event has been added in to Lines 9/10 in the abstract: "*These fluxes represent a single year survey which was strongly influenced by an El Ninõ event and therefore lower discharge than usual was observed."

Methodology:

Section 2.9: Specify the corer type you used.

The corer type has now been specified in Page 12 Line 25

Results:

Section 3.4 : is the correlation significant?

The correlation significance between watertable and radiocarbon data has been added into Page 15 Line 7: "Conventional mean DO^{14}C age was positively correlated ($p < 0.05$)…"

Discussion:

The discussion section on bulk density and carbon stocks is not clear. Since no car- bon content were measured, it is difficult to discuss the carbon density values. The differences

are only based on bulk density differences (higher in Sebungan). The link with higher TOC fluxes is not established. This section should be improved or removed from the manuscript.

*An extra sentence has been added into Page 17 Lines 4-6 to make it clear that as no carbon content was measured the link between peat bulk density and TOC fluxes cannot be fully established: "*However, as peat carbon content was not measured the link between peat bulk density and the TOC fluxes cannot be fully established."

Figure 1:

Figure 1 should be improved. A larger map of Borneo with the location of the site would be useful. On the detailed map, Lat/Long are not readable.

A new figure has been added that shows the location of the sites relative to the island of Borneo and clearer Lat/Long coordinates added.

Table 3:

SE3 and SE4: There might be a mistake in the % of time water table was below -60 cm (0 % for the mean water table of -92 cm, and 90% for the mean water table of -52 cm)

Thank you for highlighting the odd '% of water time below -60cm' data mistake. This data has been reanalysed and changed in Table 3.

References:

Some references cited in the text do not appear in the reference list (Jones et al., 2016, Gandois et al., 2014).

Gandois et al., 2014 was mistyped and was supposed to be 2013,. Gandois et al., 2014 has been removed from the manuscript and replaced with 2013. Jones et al., 2016 has been added to the references list.

Reviewer two comments:

Major points

1. Validity of the assumption of RE

The authors assume that meteorology, soil properties, and topology are similar among the plantations because they are located close to each other, and apply the same value of RE to all the plantations (P9 L15-18). However, bulk density was 1.8 times higher in Sebungan than in Sabaju (Table 1) possibly it is significantly different. The higher bulk density shows lower soil porosity, suggesting that different water storage and RE between SA and SE. Please add more discussion about the validity of the assumption of RE.

The reviewer raises an important point regarding our runoff assumption for all sites. We acknowledge that this is a limitation. However, without additional data (i.e. hydraulic conductivity values) it is hard to assume the water storage potential of these different plantations. Thus, for simplicity we assumed a uniform runoff for all sites and believe that is

adequate for addressing our main research aim. An additional sentence has been added into the text (Page 9, Lines 18-21) to acknowledge the simplicity of this assumption: "To do this, R_E was assumed to be the same for all plantation sites, based on the assumption that all sites were hydrologically similar in terms of the annual water balance. While this is a simplistic approach all sites had similar soil properties, topography, vegetation and management and were sufficiently close together such that they experienced very similar rainfall".

2. Temporal variation of discharge

The authors mentioned that the temporal variation of discharge is larger than that of the DOC concentration so that the temporal change in DOC flux is strongly influenced by discharge compared with DOC concentration. I agree with this opinion. However, the author did not show the temporal variations in discharge through the temporal variations in TOC concentration was shown. Thus, I would like to recommend to show the temporal variations of discharge, too.

A new figure has been added (Fig. 5) which shows the mean TOC concentrations across all sites alongside the mean discharge values for the different channels. This helps to highlight the relatively stable TOC concentrations in contrast to fluctuating discharge values. This figure is subsequently referenced on Page 14 Lines 19/20 Page 15 Line28/29.

Minor points

P3 L27-28: I don't know a paper that land compaction by heavy machine increase peat decomposition. But I know the opposite results, for example

* Melling et al. (2005) Soil CO2 flux from three ecosystems in tropical peatland of Sarawak, Malaysia. Tellus, 37B, 1445-1453.

* Othman et al. (2011) Best management practices for oil palm cultivation on peat: Ground water-table maintenance in relation to peat subsidence and estimation of CO2 emissions at Sessang, Sarawak. Journal of Oil Palm Research, 23, 1078-1086.

A recent paper by Tonks et al. (2017) closely links the degree of decomposition to the physical properties of peat namely bulk density, shear strength and porosity. This reference has been added to Pg 3 Line 28 and to the references list (Page 23 Line 6).

In the studies by both Melling et al. (2005) and Othman et al. (2011) the autotrophic (tree roots) and heterotrophic (peat oxidation) respiration emissions are not separated. As such, it is hard to draw conclusions about the total net ecosystem-atmosphere CO_2 exchange. The implications of this data are elaborated by Page et al. (2011; Review of peat surface greenhouse gas emissions from oil palm plantations in Southeast Asia. White Paper Number 15).

P14 L1: Is this calculated RE the mean or median of Monte Carlo simulation shown in Fig. S4?

This is the mean calculated RE, this has now been specified on Page 14 Line 5

P14 L6: Probably, "and" after 49.6 mg l-1 is not necessary.

This has been omitted Page 14 Line 13.

Table 3 & Fig. 7: Please explain what is SA 3.1, 3.3, and 3.6.

These are individua sample sites within the Sabaju 3 plantation. This has hopefully now been clarified within the Table 3, Figure 7 and Figure 8 captions.

Fig. 3: It seems that there is no relationship between the rainfall pattern and the temporal variations in water table depth, which is not common, possibly because the rainfall is monthly data whereas water table depth is weekly data. Please check whether there is the relationship between rainfall and water table depth, and improve the figure if the water table depth is influenced by the rainfall. If there is no relationship, please discuss why.

The lack of correlation is now explained on Page 13 Lines 21 – 24: "relationship between the rainfall pattern and temporal variability in the water table depth could not be drawn due to differences in the data resolution (i.e. monthly data = rainfall; weekly data = water table depth)".

Supporting information: Please explain what is SA 1.4 to SA 4.4

A sentence explaining what these codes represent has been added as an end sentence to the introductory paragraph of the supplementary material: "Individual monitoring sites across the plantation estates are quoted as follows: Sebungan (SE 1, SE 2, SE 3, SE 4); Sabaju 1 (SA 1.1, SA 1.2, SA 1.3, SA 1.4), Sabaju 3 (SA 3.1, SA 3.5, SA 3.6); Sabaju 4 (SA 4.1, SA 4.2, SA 4.3, SA 4.4)"