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Interactive comment on "High greenhouse gas fluxes from grassland on histic gleysol along soil carbon and drainage gradients" *by* K. Leiber-Sauheitl et al.

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The paper is very interesting considering the greenhouse gas (GHG) emission from histic gleysols which is a soil group with relatively low number of investigations. However, in many countries of the boreal zone and northern regions of the temperate zone these soils cover significant area. Thus, data gathered from these soil types are very important to fill gaps in relevant models of GHG emission. The paper is written in a clear manner and all the methods as well as most of the scientific outcomes are acceptable. However, there are some discussable aspects I would like to touch. First, the second hypothesis ("...peat mixed with mineral subsoil and resulting lower Corg

concentration emits lower amounts of GHG than unmixed peaty soil with a high Corg concentrations") is not very clear to me because the anthropogenic mixing of peat and subsoil which is always related to drainage will always enhance the mineralization of organic material and thus, leading to higher soil respiration. The C content in peat is probably not so important than the quality of organic material: in many peatlands the lower layers of peat have recalcitrant organic material which does not decay easily. Mixing this recalcitrant peat with mineral material should normally increase the CO2 fluxes. Therefore, it would be important to touch at least shortly the problems of the C quality in this particular peatland area. It is also related to the long-term history of drainage of this peatland. Most possibly, the quality of organic matter has been changed during the long-term disturbance. On the other hand, I completely agree that the rewetting of this area is a recommended way of restoration and due to the lower potential of CH4 release will be also less climate-loading than those system are normally during the first decade after the restoration. The problem of the C quality leads to another question which I would like to discuss. Second, there was a very low, if not to say, zero N2O emission from this peatland. In analogous soil conditions (if the term "histic gleysol" really mens the same: up to 30 cm peat layer above the mineral subsoil) drainage causes a significant N2O release which is also a main reason of high N2O potential of some forests on these soils (Mander et al 2010 Landscape and Urban Planning). Thus, it is possible that the denitrification in this system is complete and all the N2O will be transformed to N2. The less calcitrant C available to denitrifiers (which is probably the case!) is a good presumption for that. Third, despite of a very correct and detail statistical arguments you provided, I am doubtful about the usefulness of using non-linear function for calculating CH4 and N2O fluxes from chambers. Normally, non-linearity indicates some failure in chamber or measurement technique which always can happen. I am wondering whether the main results would remain the same if excluding these 27% of CH4 and 16% of N2O data calculated on the base of non-linear model? Fourth, the last sentence of the Introduction "We show, however, that even in shallow histic gleysols and in histic gleysols mixed with mineral soil, GHG

emissions remain as high as in deep peat soils and are driven by water table" is already a result and belongs to The Discussion or Conclusions. Finally, rewording (may be even skipping) of the second hypothesis and including the C quality analysis, as well, as the analysis of results without the doubtful CH4 and N2O data would improve the paper's quality.

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