The aim of the study is to demonstrate using laboratory incubations how additions of nutrients (N and P salts) affect CO2 and CH4 production in subarctic bog and fen peat. This would indicate the effects of wildfire induced increase in nutrient content in peat. To get more information on the effects of fire on soil processes is relevant because wildfires will increase in northern latitudes with global warming and the control of wildfires in remote areas is difficult.

There are aspects in the text, methods and information given which should be considered when evaluating the output of the study. Enclosed comments.

Title: Title directly suggests that the effects of increasing wildfire and permafrost thaw were studied. The study was conducted with non-fire affected peat samples (?) and merely shows effects and fate of added nutrients in peat. Of course you can discuss the possible links to the wildfire. The title could thus be modified to avoid misleading.

Methods:

Some more information about the sampling sites and peat cores taken should be given. Now there is no data if the cores taken had vegetation cover, or were just bare surfaces sampled?

Line 91: The text indicate that the initially frozen peat cores were placed after thawing in plastic containers and mixed with hands. There are two aspects to be considered, points which can have impact on the results obtained.

The layers of the 0-25 cm peat profile were mixed, and possibly with vegetation? The plant material are serving easily decomposable material for microbes and could enhance their activities and differently in bog peat (moss dominated?) and fen peat (sedge vegetation?). Short-term effects on peat microbiology were studied here without impact of vegetation. If the response of the overall ecosystem is the topic then the measurements should be conducted with intact peat cores including vegetation and primary production. Response of vegetation to nutrients surely has effects also on the microbial activities. The lack of plant activity (carbon release and nutrient uptake) is causing inaccuracies when the aim of the incubations is to mimic non-growing and growing seasons. Or can we assume that in this transition phase plant activity is minor and does not have a great importance? Then it has to be stressed in the aims, even in the title, that the transition phase with minor plant growth is studied here.

The second comments on the mixing of 0-25 cm peat cores is the possibility that layers of different oxygen status in situ were mixed. Then the anaerobes in deeper profile had not their optimum growth conditions in the incubations. Also the populations of aerobic microbes, including methane oxidizers, could have been "diluted" and their real activity was not included to the net release of methane. The water table of the sites should be given. Water status of the peat incubated (lines 110-111) is not clear. Does 80 and 100 % mean water content related to the water holding capacity of the peat?

A basic question for the conclusions is the number of replicates. The two replicates do not allow prober statistical analyses to compare the treatments.

Other comments:

The effects of N and P salts on CO_2 and CH_4 evolution from two different peat were studied. The measured CO_2 evolution reflects CO_2 production from anaerobic and aerobic microbial processes. As pointed out above CH_4 evolution is the sum of CH_4 production and consumption. The peat studied here is taken from subarctic peatlands. There the effects of added nutrients on CO_2 and CH_4 evolution have not been intensively studied. However, we can well assume that the results on CO_2 and CH_4 from boreal peatlands treated with nutrients are useful when discussing the results obtained here - the basic mechanisms are the same. However, the literature from boreal peatlands has not been considered here. Especially the effects of N on CO_2 and CH_4 dynamics have been studied intensively.

A point which could be considered is the effects of salts as such, without any nutrient effects. There are results showing that extra salts can decrease microbial activities in acidic soils. Could this have an impact in the rather short-term incubation experiments with naturally nutrient poor soils? Please note that with fen peat the fumigation extraction method gave negative microbial biomass at the end of the incubation. This means that the amount of extractable organic C was higher before the fumigation than after the fumigation (end of page 10). Would extra salts have been destroyed a substantial part of the microbes releasing their carbon in incubated salt-treated fen peat? Some references if exist could be given about the experiences to apply fumigation extraction method for peat. Are there reports on problems, e.g. negative biomass? If the negative biomass for fen after the incubation is not a result of biomass decomposition, then we can ask if the method has inaccuracies to determine microbial biomass in peat in general (also for bog here)?

In the Fig. 4 microbial biomass carbon and nitrogen are used in the calculations also for fen peat although there is a comment at the end of page 10 that after the incubation the microbial biomass could not be determined for fen with the fumigation extraction method? Did the method give positive biomass for fen before the incubation (initial biomass) but negative biomass just after the incubation (see the comments above for the possible salt effect and methodological problems).