Review of Biogeosciences manuscript #

As my review comes after the publication of M.W.I. Schmidt's comment, I used part of it when I considered that students who performed the reviewing training made constructive comments and added my own ones elsewhere.

This study is overall a good example on how prescribed fire regime, reconstructed from paleoecological data, can be used as input into a biogeochemical model in order to answer to several questions related to the long term dynamics of ecosystems. Authors wanted to see 1) whether past changes in fire variability impact biogeochemical processes and therefore soil C and NECB as compared to information from only one fire event (last one) or several ones but under a regular assumed fire regime (fixed), 2) for how long such impacts could stand, and 3) to assess the relative roles of fire and climate changes on such impacts on C stocks and fluxes.

Such modelling experiments are necessary to understand processes at play and to likely disentangle the impact of the different factors. The well-documented site used in the present study is a valuable data source resulting from the combined results of several studies that targeted different proxies over time with associated expertise. Therefore, such approach, while still rather rare, should be encouraged. Despite these general positive comments, there are few issues that need to be clarified, and that, in turn will require to remove the third objective or at least to present its results differently.

First of all, even though the authors refer to past published studies, they should present or document the reconstructed response of vegetation (changes or not) the site recorded at least with the same level of information as for the fire reconstruction they provide.

Secondly, and most importantly, I wonder why authors have used only the same fixed 30-year time series for climate data whatever the time frame simulated over the last 4500 years BP instead of using past climate simulations from GCM or ESM whose many have Holocene climate as well as Future climate runs. This would have prevent authors from saying that fires and climate are disconnected which is absolutely not true, or at least need to be tested for each ecosystem studied. Moreover, instead of just increasing the 30-year time series temperature by 2°C, they could have used the full climate time series for the 21st century simulated by the same climate or earth models that provided the Holocene runs. They even could have tested different IPCC scenarios and their impact of the NECB. The use of climate model data would have provided precipitation time series as well, whose changes could also have impacted soil nutrient (and C) leaching. Indeed, it is easy to show that fire regime change outweighs climate change when such climate change may be unrealistic or only taken into account through temperature increase whereas several studies have documented and discussed about the potential counter-effect of precipitation increase in compensating the effect of temperature increase on fire occurrences and spread. It is even more important in the studied system as authors suggested and used two types of high severity fires: those with and those without erosion. Stand-replacing fires (95% mortality) are not really severe fire if post-fire regeneration is occurring in the next following years from naturally adapted species. Fire severity would rather refer to the difficulty of post-regeneration encountered in special cases. Stand-replacing fires are usually very intense and fuel consumption includes all the litter and humus layers, leaving the mineral soil exposed. So, if erosion in the burned watershed occurs (towards the lacustrine receptacle), it is performed during (heavy) rainfall events. Therefore, this is another argument to show that it would have been valuable to use past simulated precipitation over the last 4500 years BP, in order to test if rainfall (even as mean annual rainfall) changes could have occurred contemporaneously to erosive events just after some fires as compared to others. Moreover, authors provide no information on the vegetation compartment modeled except the Net Ecosystem

Production for outputs, so we have no idea about which plant types are used for this site nor why 30cm deep was chosen as the targeted depth to analyze the site response. Finally, in the current version, except from NEP, we have not idea about the effect of vegetation change in terms of composition nor structure through time, we cannot see the direct as well as indirect effects of climate change on vegetation nor climate on fire as climate dataset was fixed and repeated along the 4500 years BP, even though fire ignition and fire spread conditions may have been more or less favorable.

For all these reasons I see two options that require to modify the manuscript:

Option 1: to do the modelling experiment exercise once again but using climate data that represent the studied Holocene period for the first part and the 21st century for the second part. Even though climate data come from GCM and are not perfect, they will still be better than present-day ones applied to past and/or future periods, especially if climate is tested and its relative impact compared to that of fire regime variability. In parallel to temperature and precipitation datasets, authors should explain how they deal with air CO_2 concentration as it should have been modified from 280 ppmv until 1750 to the historical recorded concentration until nowadays, and for the Future, at least a mean CO2 increase should be used if authors do not want to test several RCP scenarios. By keeping the CO2 at a fixed concentration could still be acceptable but once more, as they are tracking C pools, I think that the atmospheric C input should be taken into account.

Option 2: keep the modelling experiment in the current version but authors need at least to remove the third objective as climate has not been properly taken into account as compared to the fire regime factor. In such case, they should explicitly present this study as a first-step modelling approach integrating only the fire regime information and therefore only testing it. All sentences related to climate effect should be modified in order to rather present or discuss limit of non-using proper climate data. This would better fit with the balanced way results must be discussed. In such a case, the first two objectives are still OK. Results and conclusions should be fairly presented without omitting that the climate data used may be a limit to the interpretations done.

Otherwise, I found pertinent the improvements suggested in the M.W.I. Schmidt's comment posted for improvement definitions, more detailed explanations and improvement in figure quality so I encourage the authors to take them into account. They will facilitate the reading of the manuscript for people not fully familiar with model requirements and functioning such as the need of a spinup period, the use of several pools or compartments… If supplementary material is allowed I suggest to add such information there, even with a scheme presenting how the DayCent model works.