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Biogeosciences Discussions

Interactive comment on "Fire-regime variability impacts forest carbon dynamics for centuries to millennia" *by* Tara W. Hudiburg et al.

Anonymous Referee #2

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The authors developed a comparative analysis in which paleofire data and an equilibrium (spin-up) approach are used to constrain the initial conditions of a biogeochemical model simulation. The logic for doing so is that the legacies of past fire disturbances are ignored when employing an equilibrium approach, thereby misleading the conclusion about time-dependent trajectories of NPP, soil C, etc, and strengths of carbon sinks and sources. This could be particularly true in a forest system where the disturbance regime is driven by pulses of extreme events, as opposed to one where disturbances occur at regular intervals. The authors illustrate this problem by providing an example from the Rocky Mountain National Park. This is an interesting topic and a fairly nice application of the problem. The paper generally reads well. Still, there are some aspects of the study that I'm more or less comfortable with. But I'm quite confident that authors can improve these aspects. Also, aside from discussing the biogeochemical elements,

it could be interesting to also compare some of the ecological attributes like age distribution of forest stands between the paleoinformed and equilibrium approaches. Clearly the distribution of ages will be quite different, which could have implications if eventually model simulations become a tool for forest management guidelines aiming at sustainability of ecological services.

Specific comments

Introduction:

L87-93 Would this rather illustrate that many models that perform a spin-up period lack a validation of their simulated biochemical cycle?

Materials and Methods:

L165 What exactly is the size of the simulated area? Are fires spatially-explicit? Or just based on random selection of cells? Perhaps a few word on this. L176 So climate and radiation are constant. This may be problematic because in the eventuality that climate was different during the late-Holocene, as compared to the Anthropocene, likely the simulation will be misleading the productivity levels. So I guess this is another argument for doing the +2C and -2C simulation experiments (L217-224). Not using paleoclimatic simulation is an important weakness of this study and I would recommend that authors put more emphasis on the importance of this temperature sensitivity analysis. However, they should note that temperature is not the only driver of NPP; radiation and precipitation are also important. L182-185 More details are needed in regard to the validation dataset. What kind of datasets are these observations? How were they derived? Why select these over others? What do you mean by 'similar-aged'?

Results and Discussion:

L241 What are the plus and minus signs for? Standard deviation or confidence intervals? What is the sample size? Area under analysis? Seems that crucial details are missing. L274-278 This statement about disturbance free or intensified disturbance

periods is partly false, because DGVMs now have the capacity to run fire dynamics using paleoclimate simulations that feed into a dynamic fire behaviour and growth model (e.g., LPJ-LMfire). This removes the necessity to do the paleo-informed, but nevertheless paleodata comparison is necessary as a validation step. L294-298 This is not really new and has been known for decades. The impact of fire versus vegetation is quite obvious considering that fire has the potential to exclude treed vegetation from landscapes despite generally improving growth conditions with warming and CO2 L343 “the lack of paleoclimate data” : this is an important weakness of this study. A few sentences about this is needed here to help readers unfamiliar with this issue to understand what is meant by ‘paleoclimate data’.

Figures:

Figure 4 This figure is not obvious to read. Perhaps put on separate panels.

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