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Interactive comment on "The impact of wildfire on biogeochemical fluxes and water quality on boreal catchments" *by* Gustaf Granath et al.

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The impact of wildfire on biogeochemical fluxes and water quality on boreal catchments(Granath et al., 2020, Biogeosciences Discussions) This study reports on the impacts on wildfire on C dynamics and water quality from a boreal forest catchment in Southern Sweden, using paired (before-after) measurements on fire areas. To be honest, I was hoping quite a lot from the paper, as the topic seems really interesting and promising(as both pre fire situation and post-fire conditions were supposed to be included. It would be quite unique possibility to describe quite exactly the C dynamics related withfires (pre fire conditions, combustion, and post-fire conditions), and all this in relatively large scale. Unfortunately, at this stage the paper misses many explanations, and actually entire research is missing some of the needed measure-

ments. Thus, at this stage the authors were not able to convince me that some of their statements are actually valid. At this stage I have the impression, that by leaving the pyrogenic material measurements (charred material, charcoal, ash) out from the research, the authors are overestimating the C losses through combustion. Also, as the water measurements started weeks after fire (and one week after first rain), the authors are underestimating the fluvial C movements. As the authors have not been explaining how they have been using eddy data (they are presenting net ecosystem exchange (NEE) results, that also includes the photosynthesis (carbon uptake), but they haven't been explaining the proportions of the photosynthesis and respiration, the authors have not been convincing me that their numbers behind different C fluxes are correct. The authors are also completely ignoring the fact (would expect it at least in discussion) that (at least some of) the areas were logged after fires.

RESPONSE: Thank you for taking the time and performing a very detailed review. We will here respond to the four main listed concerns. These points are partly repeated in the detailed comments but we will sometimes refer to the response here.

<ins>Charcoal and other carbon pools:</ins> Also reviewer 1 asked about the impact but were less worried that it compromised our conclusions. We do take this issue seriously and have conducted sensitivity analyses to better evaluate the effect of our approach. Fortunately, we have access to data carbon content of the soil charcoal layer. Using data from a recently published study from the same burn (https://doi.org/10.1111/1365-2745.13529, some of the authors were involved in that study as well) we can conclude that the carbon content is roughly 20-25% in this layer. This is lower than the non-burned organic soil, and the bulk density of this charcoal layer suggests that this layer is less compact as well. Consequently, using non-burned organic soil values for carbon bulk density we likely underestimate carbon loss rather than overestimated as we previously stated in the manuscript. If we assume a charcoal layer of 1 cm (reported in older pine forest for studied burn [https://doi.org/10.1111/1365-2745.13529] but for the whole area the thickness is smaller), this underestimation is roughly between 2-45

g C per m2 (or about 0.01-1% of the average calculated loss).

We did not include losses from downed wood as this is a small component in a managed landscape like the one that studied here. The burnt area has around four m3 per hectare of downed wood (Jonsson et al 2016, http://dx.doi.org/10.1016/j.foreco.2016.06.017). With a stem density of 418 kg m-3 for Scots pine (Macdonald, Gardiner and Mason, 2009), and 50% carbon content, the maximum loss from downed wood is about 80 g carbon per m2 (or circa 1.5% of total C loss). However, this maximum value is very unrealistic as downed wood rarely was completely consumed by the fire.

Losses from standing trees were not estimated. It is very hard to make reliable quantifications of such losses (amount of fine branches and needles consumed) and they contribute little to the overall losses in the studied area. For example, after the fire (charred) needles were still present in the burned crowns. We do have data on crown fire severity (crude % scale) that can be used to calculate potential losses from needles and fine branches.

Taken together, in a revised version we aim to better discuss uncertainties and provide potential losses associated with the above carbon pools. If anything, our carbon loss estimates are conservative and not an overestimation.

Early hydrological losses: We agree that there are uncertainties associated with the initial post-fire period, and more sampling points would always be better, but we did not have the time, access/permits or budget to start sampling sooner, or at higher frequency, with no advance warning of the fire. The fact that some solute peaks occurred after our first sampling visit (in some cases two months later) strongly suggests that we did not miss a major flushing event during the immediate post-fire period. We have undertaken a sensitivity analysis of the maximum solute export

that could have occured if an earlier peak had occurred, and the implications of this analysis will be discussed in the manuscript. Here we describe an example of a sensitivity analysis for the Gärsjöbäcken catchment. If we assume that the carbon and nutrient concentration one week after the fire were double the values measured at the first time point (about 3 weeks after), then the impact on the annual fluvial loss is an underestimation of 0.5% for carbon and 1% for nitrogen. This should be viewed as an extreme scenario in our opinion but gives an idea of how small the impact is.

NEE: We focus on NEE as this is the carbon balance and the response of main interest here. We write C emission because NEE showed a net C release. Maybe we should be clearer here and not mix terms, but it is clearly important to note that the ecosystem was losing carbon overall *despite* vegetation regrowth during the first three years post-fire; this suggests a large and sustained loss of carbon from soils and dead organic matter. Either the reviewer has misunderstood how NEE data are interpreted here, or we have misunderstood their point. To try to avoid confusion we will revise the terminology and define NEE in the methods so that it is as clear as possible what these results represent.

Logged areas: We will include a discussion of the potential impact of salvage logging. We didn't see a clear effect and therefore it was not discussed. Note that our two focus catchments were not salvage logged.

Below are my[reviewer 2] detailed comments:
P2 L11: What about Scandinavia? Emissions are bigger or smaller compared to North America, as the fires are completely different in these two regions.
RESPONSE: We are not aware of any data of carbon loss from Scandinavia. Our study is likely one of the first, but we are happy to be corrected.

P2 L12-13: Compared to what areas? North American areas?Upland soils vs. peatlands?
RESPONSE: Compared to boreal upland soils. This should be clarified.

P2 L23-24: New study by Rodríguez-Cardona et al 2020(Scientific Reports volume 10, Article number: 8722) shows clear post-fire decrease(although they are using longer chronosequences there).
RESPONSE: Thank you for pointing us to this article. This study has a much longer time-scale (decades), but we see merit in referring to it in the manuscript.

P2 L24: What is POC export?
RESPONSE: POC=particulate organic carbon. We missed writing out the abbreviation.

P4 L1: It should be stated somewhere here that (at least some) the areas were logged after fire!
RESPONSE: We are not sure that comparing logged and unlogged areas can be tested in our study, and it was never our intention to do so. That is why we did not add it as a separate question/aim. However, we will discuss this, and consider if it could have influenced post-fire solute behaviour.

P4 L1: In intro there is a lot of talk about drained peatlands and/or peatlands.Is this the case also here, are the areas mainly forests on drained peatlands? I think some kind of description of the area would be good to include here.
RESPONSE: We presented data on the percentage of open and forested peatlands in Table 1; total peat cover was around 15-30% and yes, most forested peatlands were drained. The catchment attributes were discussed at the start of the methods, but we notice that this part is lacking a reference to Table 1, and we will add that.

P4 L9: Any expectations/hypothesis?
RESPONSE: Good point. We did have an idea behind testing this and a sentence should be added to better reflect this.

P4 L16-17: Would expect more of the area description. How old was the forest? Was it similar through the area or there was many different stands with different age and tree species?
RESPONSE: Study area description was a bit too brief. We will certainly expand on

this. In short, forest consisted mostly of even-aged pine dominated stands, varying from clear-cuts up to >100 years forest stands.

P5 L5-8: Can it be that due to late start you have been actually missing some of the C movement (it is washed through different soil horizons with days after rain)?
RESPONSE: See main response at the beginning.

P5 L28: Is this the same as the "ash layer" mentioned earlier?
RESPONSE: Yes. We will change this to “charcoal layer” throughout the manuscript.

P5L30: It can be also up to 60% or even higher (Wiechmann et al 2015. PloS one, 10 (8),e0135014-e0135014). P5 L 30-31: how were the charred logs/snags/stumps treated?If you haven't been measuring the pyrogenic carbon (charcoal, ash) separately, you are probably overestimating a lot.
RESPONSE: Thank you for pointing us to Wiechmann et al 2015. However, they report % C of the charcoal particles and not of the charred/charcoal layer of the organic horizon (O horizon). It is the O horizon that is of interest for us. As we have written in the above response, we now have such data and they show a carbon content around 20-25%. Downed wood was not included in our estimates as this component is rather small in these managed forests (see response above regarding downed wood).

P6 L9-10: Based on Figure1, these transects and sample plot locations are not similar to the burned area. Please specify how these reference transects were located (how far from each other, etc.).
RESPONSE: The transects were chosen to reflect the variation within the burnt area and we believe we succeeded rather well in placing these transects to achieve this goal. Using forest composition and wetness/topography maps we selected similar combinations of forest types, wetness and topography as found across the burnt area. Originally we wanted to model organic soil depth across the landscape but because the mean organic soil layer varied so little between sampling plots and did not correlate with predictors like soil moisture, we decided to use the mean value.

P6 L14: "...three to five soil cores...". Per transect? Per plot?
RESPONSE: That should be per plot. Thanks for noticing this.

P6 L19-21: If stated like this, then my question is what about Europe and Scandinavia?
RESPONSE: We are not aware of any studies estimating organic soil loss during fire in northern Europe (but we might have missed studies of course). The method should not be continent-specific and this can be reworded to describe the method in general, rather than pinpoint it to a specific geographical location.

P7 L4: With eddy, I assume you are measuring net ecosystem exchange (NEE) (including C uptake by photosynthesis and release by respiration). If we assume that everything was killed during fire (but you were saying that at the beginning the fire was not stand replacing) then you would measure the respiration (decomposition, etc.), but the vegetation comes back quite quickly after fire,so I would still say that you are measuring NEE. How you are able to talk about the C emissions? As you are not explaining how you were separate the respiration (C emissions) from the photosynthesis. How big and to what direction is the footprint area of the eddy systems. I would assume that the winds from the west are dominant in these areas, but this way the southern eddy is not measuring fire area (at least most of the time)? Also, the eddys are placed so that you are not able to combine the Closs measurements and eddy data (as they are most probably not overlapping). Any specific reason why the eddys were placed as they were?
RESPONSE: First, we write (P4, L18) that the fire WAS a stand-replacing fire - i.e. everything died more or less. Second, we do indeed focus on NEE as this is the carbon balance and the response of main interest here. We say C emission because NEE showed a net C release. Maybe we should be clearer here and not mix terms, but it is clearly important to note that the ecosystem was losing carbon overall *despite* vegetation regrowth during the first three years post-fire; this suggests a large and sustained loss of carbon from soils and dead organic matter. Third, towers were 2.5 m high. The southern tower is located about five hundred meters from the unburned forest

and is indeed measuring only over the burnt area. Fourth, the location of the towers were chosen based on proximity to roads (but still in a closed off area to avoid theft of equipment), representativeness of the area, tree height (tall trees near the tower can fall over and damage the equipment). We did not know the exact delineation of the catchments when the towers were set up. The fact that the tower happened to be placed just outside the catchment where we did the carbon loss measurements does not invalidate its use for comparing data; we combined the flux tower measurements, the multiple catchment measurements and the distributed soil measurements to seek to understand whole-ecosystem responses to the fire at the landscape scale. To our knowledge, few if any studies have previously obtained such comprehensive data.

P7 L18-20: This is really big assumption! Taking also into account that you actually haven't been taking the formed pyrogenic carbon (charcoal, charred material, etc.) into account (not analyzed it separately), your C loss calculations might be overestimated.
RESPONSE: Regarding pyrogenic carbon, see response above. The assumption seems supported in our view. Erosion is negligible in this system and downwards transportation of carbon particles is likely tiny compared to the amount lost in the fire. Other data on soil carbon that we have collected at some selected sites in the same burnt area did not indicate an increase of carbon in the mineral soil, further strengthening our assumption that changes in carbon stock can be ascribed to gaseous emissions (Pérez-Izquierdo et al. 2020 J of Ecology, https://doi.org/10.1111/1365-2745.13529).

P9 L13-15: So you are saying that 95% of the C emitted during the fire was coming from O-horizon? You had high severity, stand replacing fires on areas (high intensity), all the trees killed, vegetation removed, and then more than 95% comes from O-horizon? On table 2 there is only one value for emissions during the fire, and no separation by vegetation and/or soil.
RESPONSE: In boreal forests the organic soil is a large carbon pool, and most of it was combusted during the fire. As mentioned above, we did not include downed wood in our estimates, but this pool is very small in managed forests. We also did not estimate loss

from trees (needles, fine branches). The 95% statement was comparing belowground and the forest floor. This will be corrected and we will discuss the potential contribution from downed wood and trees (see earlier response). Table 2 only gives the sum as we wanted to focus on the overall picture. However as the review has queried this interpretation we can provide disaggregated estimates of carbon loss in the text.

P9 L23-25: I still think that you were actually missing the biggest fluvial losses (the pyrogenic material that is washed away with first rain event).
RESPONSE: See our response earlier.

P10 L1-2: Base on the figures you have been measuring NEE with eddy. Unfortunately, there is no data available about vegetation recovery (biomass, coverage), but I have the impression that 3 years after fire, there is already some new vegetation also in areas with high severity. So one can't talk anymore about C loss when interpreting the NEE values.
RESPONSE: Yes, for sure there was vegetation recovery in the flux tower footprints, and throughout the study area. The flux towers measured this as part of the NEE, i.e. the measurements represent the balance of vegetation carbon gain and ongoing soil and biomass carbon loss. These measurements clearly show sustained positive NEE over the 3 years post-fire, i.e. the ecosystem was a net source of CO_2 to the atmosphere despite vegetation regrowth. Either the reviewer has misunderstood how NEE data are interpreted here, or we have misunderstood their point. To try to avoid confusion we will revise the terminology here and define NEE in the methods so that it is as clear as possible what these results represent.

P10 L4-5: Now the talk is about C uptake (my previous comment). But the vegetation regrowth data is not presented, and it is still not explained how you separated the respiration and uptake data from each other.
RESPONSE: Vegetation growth is presented as leaf area increase. We are not sure we follow the reviewers comment here as we do say "net carbon uptake", i.e. the balance of vegetation growth and soil/dead biomass loss as noted above. We did not attempt

to separate uptake and respiration in our presentation of the data.

P11 L11-12: Sorry, but based on your results and talk, I'm not convinced! By not taking into account (analyzing separately) the amount of charred material and charcoal, you are overestimating the direct emissions from the fires.
RESPONSE: See earlier response.

P11 L16-18: Sorry, but you missed the first rain event (if the first samples were taken week after first post-fire rain), and with that probably also DOC that was washed away (or washed to deeper soil horizons) from the areas. So, I assume you are underestimating the fluvial C loss.
RESPONSE: See earlier response on the early post-fire period.

P14 L23: Discussion is completely missing the fact that the areas(at least some of them) were logged after fire. How the mixing of soil (pyrogenic material and soil) by machinery would affect the emissions and water quality? How the logging (removing the material that would start to decompose on areas) could affect the C fluxes?'
RESPONSE: It is correct that a large portion of two catchments were logged, and one other catchment experienced some logging. Only older stands were salvage logged. Note that the two focus catchments were not logged (except a tiny part on one edge of the catchment and along some roads). Logging started in general in the spring the year after the fire (in the manuscript it says logging was done within 3-6 months after the fire, but this is incorrect for the investigated catchments). Interestingly, when examining the water chemistry the logged catchment does not stand out much from the other catchments. The impact may actually be smaller than expected (the absence of extreme topography may have limited the impact). Removal of singed older trees probably had little impact on carbon emission at the site over the first years. While the trees were killed, most of the stemwood remained intact after the fire (and most trees that were rooted into mineral soil remained standing). This woody material is slow to decompose (particularly when singed), and (in areas that were not salvage logged) it was still present by the end of our study period. The gradual decay of this material

and charred needles will have contributed to measured NEE in these areas, which our results suggest made a relatively modest contribution to overall carbon loss during the three years of measurements. Clearly, decomposition of dead biomass will continue to contribute to CO2 loss for many years to come, so the proportional contribution of this C pool to total losses might be expected to gradually increase over time; we have noted this in the revised manuscript. Where salvage logging occured it is clearly a more rapid and substantial pathway for C loss as the wood is removed. But again, our two focus catchments did not experience logging.

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