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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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This study reports on the impacts on wildfire on water quality and CO 2 fluxes from a boreal forest catchment in Southern Sweden (which had been monitored pre-fire) by using paired before-after measurements for the decade prior to the fire and three years post-fire to construct elemental budgets. I did enjoy reading this paper and this work appears poised to make a valuable contribution to the literature of the effects of wildfire by leveraging existing pre-fire measurements. As the authors point out, studies on the effects of forest wildfire recovery often lack pre-fire measurements and rely on space-time substitution as a proxy for 'pre-fire' and 'post-fire' conditions, which carries its own set of nebulous assumptions which are avoided in the present study design here. The novel partitioning of post-fire solute fluxes into fast and slow decay

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pools should be of wide interest as a normalised metric of water quality recovery to baseline post-fire across environments. It appears the authors have been forthcoming with the history of this manuscript as submitted to a previous journal for peer-review and, as a result, had made substantial revisions and provided a thorough response to previous reviewer comments. I recommend this paper for publication following some primarily minor revisions, focused around language, clarity, and more explicit outline of assumptions and methodological choices throughout.
RESPONSE: Thank you for reviewing our manuscript and we are happy to see that you found our study valuable. In our revision we will aim to improve language and expand the text around the methods used (also needed to respond to criticism raised by reviewer 2).

Abstract: Might be worth including range of study years (including pre-fire monitoring) and year of wildfire in abstract?
RESPONSE: We agree.

Pg 1 Line 18 – 'during the first 12 months' – the first 12 months-post fire?
RESPONSE: Correct. Will be added.

Pg 1 Line 20 – curious of this terminology, 'ecologically relevant' increases – what criterion is used to determine this? Perhaps (if statistically applicable) 'significant'? Not that statistical testing is required, but if it were carried out, this may be the appropriate venue to specify.
RESPONSE: What we mean is a change that potentially can have a non-negligible impact on biota. This is based on joint judgement of previous studies but no specific criteria are used. The point is that the change is not tiny and we chose this terminology instead of listing all the numbers of these elements.

Pg 1 Line 22 – does the partitioning of these pools into 'slow' and 'fast' and the values of these half-lives apply to all analytes?
RESPONSE: Yes, at least for the ones we tested. We will clarify which analytes we

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tested.

Pg 1 Line 24 – given this is a study largely of using pattern to infer process, perhaps a stretch to say 'biogeochemical cycles have largely returned to...' and rather best to comment on what precisely was measured in this work, ie, 'dissolved fluxes of nutrients have largely returned to....'
RESPONSE: "Biogechemical cycles" is probably a stretch. We will reword this and the suggestion is good.

Pg 2 Line 35 – Perhaps best here and throughout introduction/discussion to quantify 'long-term' (one year, ten years, 100 years?) and contextualise in fire return interval for the cited study regions
RESPONSE: Good point. Long/short-term is ambiguous and should be better defined. We will check the manuscript and improve clarity on this. Fire return intervals may be harder to provide (varies over time and space) but will try to do so when possible and relevant.

Pg 3 Line 4 – 'runoff' vs 'run-off' inconsistently stylised throughout
RESPONSE: Will be fixed.

Pg 4 First paragraph – unclear to be how the second half of the first objective (i) "hydrologically exported C, N, S, Ca, K the first three years post-fire," differs from the second objective (i) "post fire water quality trends in five streams...." – are these two separate objectives?
RESPONSE: The first part here, "hydrologically exported", refers to the total amount while the second part focuses on concentrations. It is probably clearer if we add "the amount of hydrologically. . .".

Pg 4 Lines 25 – While topography is certainly a consideration in hydrology this statement might either be reinforced by citation to evidence, or, rather stated as an assumption for watershed delineation, given that in other boreal environments, perhaps 'topography is the last thing to consider' (ie, Devito et al., 2005)Devito, K., Creed, I.,

Gan, T., Mendoza, C., Petrone, R., Silins, U., Smerdon, B. (2005). A framework for broad-scale classification of hydrologic response units on the Boreal Plain: Is topography the last thing to consider? Hydrological Processes 19(8), 1705-1714.

RESPONSE: Using topography for watershed delineation is not exact but it should work pretty well. The paper by Devito et al 2005 is focusing on the whole hydrological response (and the controls) and for flow and balance we are employing a well-tested model (S-hype) which indeed considers other factors. In a revised version this should be more developed than in the current version.

Pg 5 Line 6 – Given the attempt in the paper to perform an elemental balance, is there any concern that this first major precipitation event post-fire may have performed some flushing mechanism where a considerable proportion of the post-fire elemental budget for any analyte in this study may have been exported from the catchment while this event was not sampled? Perhaps worthy a caveat in the discussion of why this may or may not be likely?

RESPONSE: This was also raised by reviewer 2. We agree that ideally you want to start to sample the day the fire has been put out. However, logistically this is rarely possible (if we have missed studies that have done this we would like to know to get an idea how large concentrations can be). There are a few reasons why these first 2-3 weeks are unlikely to be important. First, the amount of precipitation was not very large. Second, some catchments showed their concentration peak a few weeks after the first sampling point, indicating that flushing (at a catchment scale) often is delayed due to buffering in the system. To further strengthen our assumption that this first period had a small impact on our results, we have performed a sensitivity analysis and re-calculated an upper estimate of the amounts that could have been exported if the flush started earlier. The implications of this analysis will be discussed in the manuscript. Here we describe an example of the sensitivity analysis for the Gärsjöbäcken catchment. If we assume that the carbon and nutrient concentrations one week after the fire were double the values measured as the first time point (about 3 weeks after), then the impact on the annual budget is an underestimation of 0.5% for carbon and 1% for nitrogen. This

should be viewed as an extreme (unrealistic) scenario in our opinion but gives an idea of how small the impact is.

Pg 5 Line 8 – "high temporal resolution", "longer intervals", "lake was sampled slightly less frequently", here and elsewhere, define each of these precisely. Hourly? Daily? Weekly? Monthly? Was the sampling regularly spaced or focused around precipitation events? Was the sampling design/frequency rooted in literature? Based off or paired with the pre-fire sampling frequency? Given the objective was to estimate export, sampling design can have a significant impact of these estimates (and varies by solute of interest), see for example: Johnes, P. J. (2007). Uncertainties in annual riverine phosphorus load estimation: Impact of load estimation methodology, sampling frequency, baseflow index and catchment population density. Journal of Hydrology, 332(1-2), 241-258. Richards, R. P., Holloway, J. (1987). Monte Carlo studies of sampling strategies for estimating tributary loads. Water Resources Research, 23(10), 1939-1948. Aulenbach, B. T., Burns, D. A., Shanley, J. B., Yanai, R. D., Bae, K., Wild, A. D., ... Yi, D. (2016). Approaches to stream solute load estimation for solutes with varying dynamics from five diverse small watersheds. Ecosphere, 7(6), e01298.

RESPONSE: Thank you for pointing out the poor description of our approach to estimated loads. Sampling design was aimed to start "as often as possible" after the fire (of course it is not easy to quickly set up the sampling with limited resources and time, and wildfires are intrinsically hard to plan for) and ranged from a few weeks at the start after the fire to more like monthly. Some consideration was taken to capture potential peaks (for example spring flood). Given that it appears that we captured the post-fire decline in concentration fairly well, we think the sampling intensity was sufficient to produce estimates with good precision. We estimate that annual loads should not be off by more than 5-10% (based on Aulenbach et al. 2016). Our overall approach, using a period-weighted method to estimate load, is what is recommended by Aulenbach et al. (2016) when there is a weak concentration - discharge relationship. In a revised version these things will be properly explained and referenced, and we will also add a discussion on how large our load estimates can be.

Pg 5 Line 21 – What is meant by each 'intersection'? Were the 300 m x 300 m grids divided into subgrids, every, say, 50 or 100 m?
RESPONSE: Poorly worded. We mean each grid point (i.e., 300 m between each sampling point).

Pg 5 Line 31 – Glad to see the careful considerations and limitations of this method which appears sound and consistent with literature. Is there a quick and transparent back-of-the-envelope calculation that could be included here to contextualise this 'likely small' overestimation of carbon loss (ie, as a potential error) relative to the estimated values, even to just to give a rough order of magnitude, to inform if we are roughly in the territory of, say, 0.1%, 1%, or 10% overestimation?
RESPONSE: Yes, it is our understanding that this is the normal approach as it should have a minor impact compared to other sources of errors but is time-consuming to estimate accurately. Reviewer 2 expressed serious concerns about this and we have indeed tried to run some sensitivity analyses to estimate the effect. We used published data on charcoal carbon content and charcoal weight from another study from the same burnt area (Perez-Izquierdo et al 2020 J of Ecology). We can now show that we likely underestimate carbon loss by treating this thin charcoal layer as an organic soil, but only with maximum 45 g m-2 (or roughly 1% of the total loss).

Pg 8 Line 11 – inconsistent formatting throughout of ions - use of sub-scripts/superscripts, and including charge, ie NH4 vs NH 4+
RESPONSE: we should probably keep it to the correct NH4+.

Pg 8 Line 18 – What was the basis for model selection following ruling out a single (simple) exponential decay surve? Ie why the partitioning into exactly two pools of fast- and slow-decay superimposed on the baseline – why not three pools and include a 'medium'-decay? Is the two-pool model rooted in literature? Does some information criterion inform that two pools is superior to three (or more) on an added complexity cost analysis? How sensitive would the analysis be to additional complexity?
RESPONSE: The two pool model was based on observed solute behaviour; most

showed a period of very rapid decline from the immediate post-fire peak, followed by a more gradual decline to baseline levels over around a year. A single-exponential model was unable to reproduce both the rapid initial decline and the longer-term decrease, whereas a two-pool model generally gave a good fit to multiple solutes (e.g. Figure 5) and appeared to be mechanistically interpretable, as discussed. A three (or more) pool model would have over-fitted the data. To our knowledge, the two-pool approach to post-fire solute behaviour is new (and thus not rooted in the literature) but we believe it offers some valuable mechanistic insights, and may be of value to other researchers in future. We will expand our justification for the approach and the discussion of its wider application in the revised version.

Pg 9 Line 1 – It appears pH measurements taken to validate this model, but no detail given in methods? Were these measurements in-situ, coincident with the water samples?
RESPONSE: Yes, coincident with the water sample. This will be added.

Pg 9 Line 8 – Presuming, then, that extended surface water coverage was not an issue at these sites then in terms of pixel removal?
RESPONSE: Sorry but we don't understand this comment.

Pg 10 Line 9 – Perhaps for clarity change "Nitrate and ammonium increased..." to "Nitrate and ammonium concentrations in streamflow increased..." and similarly throughout
RESPONSE: Good point. We should be more specific what we mean.

Pg 10 Line 31 – I am wondering back to the initial question on sampling frequency (Pg 5 Line 8) and how the resolution of sampling overlays with this estimate of the 'fast' decay pool (4-20 days). Would more high-frequency sampling during what seems to have been identified as a critical short-term post-fire period yield finer estimates of this critical period length? Further, is it possible that the omission of the first post-fire precipitation event (Pg 5 Line 6) from the sampling design yielded a considerable portion

of this 'fast' pool that was unaccounted?
RESPONSE: We agree that there are uncertainties associated with the initial post-fire period, and more sampling points would always be better, but as noted above 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. As discussed elsewhere we have undertaken a sensitivity analysis of the maximum solute export that could have occured if an earlier peak had occurred (the maximal potential impact on the annual fluvial loss is probably an underestimation of 0.5% for carbon and 1% for nitrogen), and the implications of this analysis are discussed.

Pg 11 Lines 2-6 – Were these sequences of inequalities statistically assessed? Perhaps including values of each of these peak/baseline ratios here would be informative and a useful normalised metric for other post-fire studies to compare against.
RESPONSE: Good idea and we did consider it ourselves. However, with only 5 catchments a statistical evaluation seems unwarranted. We can check if there is a metric that can be used to normalise, e.g. mean catchment residence time can be another option.

Page 11 Line 16 – Hanging parentheses
RESPONSE: Thanks for noticing.

Page 12 Line 5-6 – This may be a stretch to generalise from two studies, if no other annual-basis studies of NEE are available.
RESPONSE: Will be reformulated to "These values are strikingly similar to our two sites (155 to 165 g C m -2 yr -1 over two years), but further research is needed to establish if such values are typical for boreal uplands post-fire."

Page 13 Line 2 – Is this meant to read 'first year' singular?
RESPONSE: Good catch. Should be "first year".

Page 14 Line 14 – an interesting observation on similar impacts from such different types of disturbance– what mechanisms would be responsible for these similarities?
RESPONSE: The main reason would be less plant uptake and sometimes in combination of increased mineralisation. This can be briefly mentioned in the discussion.

Figure 2 – perhaps the fire could be delineated as a horizontal line on the figure as similar to Figure 3?
RESPONSE: Yes, we can add that for clarity.

Figure 6 – this inclusion of methods/assumptions (text on right of figure) is an excellent contribution to laying out the fluxes in an integrated way such as this.
RESPONSE: Thank you. Even if it has limitation, we think our box diagram helps our understanding of the main post-fire nutrient and carbon flow paths.

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