Contrasting early- and late-Holocene vegetation and wildfire regimes in a high-value drinking water supply area, Canada

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Referee # 1:

This is an excellent, high-quality study. It provides a meaningful and timely contribution to paleoecology and paleofire research. The study is reasonably designed, with a rigorously applied methodology, and the data are extensive and convincing. Its main strength is its strong synthesis of multiple proxies (pollen, charcoal, and climate reconstructions) across a clear ecological gradient, addressing key questions about past and future fire regimes. The manuscript aligns with the scope of *Climate of the Past* by using a paleo perspective to deepen our understanding of current and future climate change impacts.

The manuscript presents excellent research that could be further enhanced by refining its storytelling. I recommend clarifying the introduction to clearly state the knowledge gap, using interpretive topic sentences to frame the results as a cohesive story, and starting discussion sections with a summary of key findings. To improve clarity, suggest synthesizing site details by emphasizing the east-west gradient and moving minor taxonomic data to an appendix. A final review to ensure concise, active language and a stronger conclusion that clearly connects the fire regimes to management implications would further increase the paper's impact.

Response: Thank you for your time in reviewing our manuscript and for the insightful comments provided. The feedback we received was constructive and an improved version of the manuscript is being prepared incorporating the various recommendations with a more active voice. For example, we have re-structured the Introduction and Study Setting sections to improve the clarity of knowledge gaps, objectives, and story cohesiveness. Further, as recommended, minor taxa have been removed from the body of the text where no direct link is evident to the main findings. The Results section has also been re-written to be more comprehensive, incorporating interpretations and key messages, with main findings highlighted in summary statements. The Discussion has also been revised to combine ideas and focus on the main arguments and implications. Lastly, the Conclusion is being revised, to emphasize the connection between reemergent fire regime characteristics and management implications.

Specific comments:

[**Introduction**]

The authors introduce the early-Holocene as a potential analogue but correctly caution that "the past does not provide a direct analogue." However, you can strengthen the connection by explicitly clarifying that, although not a direct analogue, the early-Holocene represents a climate state that may resemble near-future projections, making it an important period to study for understanding processes and sensitivities.

Response: We have strengthened this connection by re-structuring the Introduction. In paragraph three, we introduce the well documented and widespread early-Holocene climatic warming phase (Wanner *et al*., 2008; Renssen *et al*., 2012; Nakagawa *et al*., 2021). We then allude to current climate warming and the potential for past states to resemble current or future fire regime projections.

In the final objective paragraph, the phrase "to determine if past conditions could reflect evolving trends" (P2. L63) is somewhat vague. Do the authors mean "...to evaluate if past fire regimes provide insights into contemporary fire activity"?

Response: Yes, we do. Framing the early-Holocene as a potential indirect analogue for future fire regimes and contrasting that with the cool moist late-Holocene formed the foundation of this research. The "evolving trend" is the current fire regime in transition and projections that point to increased future fire activity (Flannigan *et al*., 2005; Westerling *et al.,* 2006; Wang *et al*., 2015,

2017; Hanes *et al*., 2019; Parisien *et al*., 2023). Thank you for your accurate synthesis of our research objective. We have revised the language based on your recommendations to clarify our intentions.

P2. L38. “Fishcer et al., 2015” is not in the Reference list.
Response: A reference has been added.

P2. L48. "None the less" to "Nonetheless"
Response: Revised, done.

P2. L53. "imply" to "implies"
Response: Revised, done.

P2. L57. " changing fire regime " to "a changing fire regime" or "changed fire regimes"
Response: Revised, done.

[**Study setting**]

This section is very detailed, but the authors could clarify the focus by separating the basic context from the methodological details. To strengthen the introduction, it should emphasize the rationale behind selecting the regional climatic gradient across the GVWSA, its three watersheds, and the key BEC zones (CWHxm, CWHmm, and CDF). The specific site-level details, such as topography, aspect, and detailed species lists for the shrub and herb layers at Frog, Begbie, Swanson, and Worley Lakes, are important but relate more “methodological” in nature.

Response: As recommended, the Study setting subsection has been revised so that it includes only important background information and introduces the Greater Victoria Water Supply Area (GVWSA). We highlight the rationale for selecting the regional climatic gradient. Fire regime characteristics follow this gradient, with higher fire frequency occurring in the dry eastern Douglas-fir dominated forests and longer FRIs with more variability in wetter western hemlock-dominated forests (Brown *et al*., 2002; Gavin *et al*., 2003; Wong *et al*., 2004). Also, site specific details for each lake have been moved to the Methods section and the paragraphs have been shortened by moving the descriptions of vegetation to Table 1. This allows for comparison of study site characteristics in a format that is easier to digest.

[**Material and Methods**]

The final paragraph (P8. L188~) is somewhat dense, and readers might need a more detailed explanation, particularly on how to reconstruct MAP. Additionally, if the paragraph becomes too long, the authors should clearly differentiate between what you reconstructed yourself (MAP from DWHI) and the data you extracted from published models (PALEO-PGEM and HadCM3) and other paleo-records (chironomid MJAT).

Response: As recommended, this paragraph has been revised to clarify how MAP was reconstructed using a pollen derived ratio of Douglas-fir to western hemlock, with the regression equation now added to the text. The two papers that are cited (Brown et al., 2006 and Brown and Schoups, 2015) describe the development of the transfer function, using surface pollen spectra and gridded mean monthly precipitation (PRISM; Daly *et al*., 1994). We also clarify that MAP and mean July temperatures were extracted from additional published models, including chironomid-based reconstructions.

P6. L142. “A minimum of 300 grains per slide were” to “A minimum of 300 grains per slide was”
Response: Revised, done.

[**Results**]

Although the results data are comprehensive, the text often reads like a list of observations. To better engage readers, the authors should directly incorporate interpretations and key messages into the results section. Each subsection on pollen and charcoal at the sites would benefit from beginning with a clear,

single-sentence summary highlighting the main findings. Furthermore, the narrative can be improved by reducing the taxonomic details in the main text. Overwhelming lists of minor taxa might be summarized in a phrase, such as "along with minor contributions from broadleaf taxa such as Acer and Quercus," with full details moved to supplementary diagrams. This allows the main text to focus on the main drivers of change. Finally, contrasts between functional groups should be highlighted as a story, not just listed. For example, instead of simply stating percentages, frame them: "This vegetation shift is reflected in the fire-adapted functional groups, which show a clear decline in 'invaders' (71% to 42%) and a rise in 'avoiders' (5% to 23%), consistent with a closing forest canopy."

Response: We have modified the Results to incorporate interpretations and key messages specific to the respective sites. This section is now much more comprehensive and reinforces the story cohesiveness, leaving the Discussion to highlight bigger picture fire and vegetation change. A summary sentence has been added to the beginning of each paragraph. Minor taxa have been removed unless they signal an important ecological characteristic that differentiates the site and/or fire regime. We have also used trends in fire-related functional groups to highlight changes in both vegetation and fire by bridging the two analyses.

The synthesis in Section 3.6 is the cornerstone of your Results and must be very clear to effectively present the overall story of the paper. To improve this, the authors should start by stating the main finding up front. After that, you need to go beyond merely reporting statistical values and include interpretation in simple language. For example, instead of just listing H and p-values, explain that the significant Kruskal-Wallis test indicates FRIs differ across sites and time, with follow-up pairwise tests showing the large increase at Worley Lake as the leading cause. Furthermore, the description of the composite charcoal curve should be clarified to highlight its key point: an early Holocene period of increased regional biomass burning followed by a general decline. Additionally, the climate data, although well-presented, can be more effectively integrated by explicitly connecting it to your fire records. Clearly state that the climatic shift to a cooler, wetter late Holocene aligns with your pollen-based precipitation reconstructions and is supported by independent model outputs and temperature proxies.

Response: As recommended, we have revised and re-structured this section to better present the key takeaways. We highlight the main findings and add simple interpretations of statistical outputs, reinforcing the methodological context for selected tests. Independent model outputs for climate are emphasized in their supporting role for pollen-based reconstructions of precipitation. We have expanded on how the composite curve represents biomass burning and connect the climate reconstruction to fire regime characteristics.

P.19 L343. “a pattern of asynchroneity” to “a pattern of asynchrony”

Response: Revised, Done.

[**Discussion**]

Although the data in this section is comprehensive, the main arguments and their implications could be presented more strongly and clearly. The manuscript would benefit from explicitly stating the central finding at the very beginning of the Discussion section, immediately providing context for the reader. Additionally, in Sections 4.1 and 4.2, the key conclusions for each period should be clearly summarized at the start of each section. This would make the main points more obvious and easier to understand, rather than requiring the reader to piece them together from the detailed evidence that follows.

Response: We thank the reviewer for this suggestion and have added a statement in the first paragraph describing the profound change around Worley Lake in the western GVWSA. However, regarding sections 4.1. and 4.2, we feel that the new information included in the first paragraph in the discussion appropriately summarizes these sections too. Thus, to avoid repetition, we opt not to add similar sentences at the beginning of these sections.

The discussion provides a robust analysis, but its impact could be improved by combining ideas to enhance clarity and rhetorical strength. Currently, the site-by-site vegetation descriptions, while detailed, may overwhelm the reader. An integrated approach that clearly highlights the strong east-west gradient in vegetation openness would be more effective. This concept of synthesis also applies to the

fire regime conclusions. The key finding that an east-west gradient was not evident in the early Holocene but became more pronounced later should be emphasized more prominently. Lastly, the various drivers proposed for late-Holocene fire variability represent a key complexity. Their influence would be stronger if organized into a single, focused paragraph that explores how these factors interacted as the dominant influence of a uniformly warm and dry climate started to weaken.

Response: We have modified the text to reduce the site-by-site comparisons, instead emphasizing trends and drivers. For example, instead of referring explicitly to changes at sites or attempting to compare nuances between sites (these details are now included in the results), we discuss how temporal and spatial climatic variation influenced fire regime overall, highlighting the key differences between more open and drier eastern forests versus moister western forests. We note, however, that the new text spans several paragraphs, detailing characteristics of climate, vegetation, and fire disturbance.

The analysis in section 4.3 can be enhanced by highlighting key insights. First, for the dry eastern sites, the main finding suggests that past and future changes are more influenced by shifts in fire severity and fuel connectivity than fire frequency. The paleodata show these sites have always experienced fire, hinting at a possible future with more frequent, lower-severity fires, but with a notable risk of high-severity events during climatic transitions, as evidenced by the period 500-1500 cal BP. Second, the moist western sites are most at risk of a complete regime shift. The record from Worley Lake acts as a key analogy, showing it was once as fire- prone as the eastern sites are today; this emphasizes that no part of this landscape is immune to major change, which is important for management strategies. Lastly, the influence of humans, from Indigenous burning to current suppression efforts, must be considered. Briefly explaining how human impacts interact with the climate-driven baseline will lead to a more complete and practical conclusion.

Response: We agree with this point and are modifying this section, highlighting findings for dry eastern areas as well as wetter western areas. Regarding Indigenous burning, we present some evidence that is tantalizing to interpret as being related to human burning. At the same time, we recognize that there is some debate about climate vs human fire signal in the region, and stress that the drivers of the slight resurgence in oak is not definitively established here. In fact, the important aspect of this work is that it documents a subtle regional increase in oak that has not previously been described or discussed.

[**Conclusion**]

While the distinction between top-down and bottom-up controls is an important and insightful part of the authors' conclusion, its current presentation could be more impactful. The argument would be clearer if you replaced the hesitant phrasing and vague reference to "intrinsic factors" with a more direct and explicit explanation. Specifically, the main finding is the feedback loop the authors observed: climate acts as the primary top-down driver of initial vegetation change, which then alters fuel structures in a bottom-up manner. This bottom-up change in fuels significantly modifies the fire regime itself, demonstrating a tightly coupled and dynamic system.

Response: We agree with this assessment. The intrinsic drivers that emerge can best be qualified by the persistence of Douglas-fir in areas where the feedback between fire frequency and fire adapted vegetation likely support a lower severity disturbance regime. Our hesitancy stems from well-meaning caution when interpreting fire regime characteristics, such as fire severity and/or fire size from the indirect charcoal-based fire reconstruction. We are improving our conclusion to explicitly state how extrinsic and intrinsic climate, vegetation, and fire interactions emerge in this tightly coupled system. Further, we have added a limitations section in the Discussion to underscore limits of our methods and interpretations.

The management implications are relevant but would be more compelling if linked more directly and authoritatively to your specific findings. The discussion of fire suppression, while important, currently feels somewhat disconnected from the paleodata just presented. To improve this, the authors could clearly frame the management implications through the lens of the two fire regime types they identified.

This study offers essential long-term context for understanding what defines a climate-driven fire regime across various landscapes; the conclusion should emphasize this unique contribution to move beyond general statements and provide concrete, evidence-based guidance.

Thank you for recognizing the valuable application of this paleoecological study in a management context, which goes beyond the foundational literature. To make our conclusion more compelling we purpose framing management implications directly with the Capital Regional Districts master plan. To do this we suggest adding a short sentence in the Study Site section that alludes to plans for water supply expansion into the western Leech watershed as population growth increases demand. In the Conclusion, we make the connection that these moister western forests, which have experienced little fire recently, will become more vulnerable to fire in the future. This knowledge is essential to water purveyors as they design and invest in future infrastructure.

Referee #2:

This is an interesting paper that is suitable for publication in Climate of the Past with a small amount of further work.

Response: Thank you for the evaluation of our research and the positive assessment. We have undertaken revisions to address all the issues raised and feel that the manuscript is greatly improved.

I recommend minor to moderate revisions, that will not take long to do. I attach a commented up copy of the manuscript so that you can see where problems occur, including the inevitable typos.

Please add a paragraph criticizing your approach and outlining its uncertainties and limitations. Leslie Anderson's 2022 paper in Quat. Res. showing that there are serious problems with using charcoal counts rather than area should be mentioned here. I am not recommending that you change your analysis from that of charcoal particle numbers to surface area, but I am asking you to be upfront about this problem in the discussion.

Response: The reviewer raises an important point regarding uncertainties and limitations in the approach. In response, we have added a paragraph to the manuscript addressing these issues, highlighting key considerations when using CharAnalysis and identifying factors that are necessary to robustly identify charcoal peaks. We contrast these with the limitations (e.g., violations of the Poisson assumption and edge effects) and recommendations made by Anderson et al. (2023). We then discuss studies that integrate multiple proxies to assess fire impacts across established charcoal peaks, seemingly capturing both terrestrial and aquatic post-fire signals that would suggest the peaks do indeed reflect fire events. We respectively conclude the paragraph by emphasizing that additional research is needed to resolve the methodological uncertainties.

As the paper is now written, it is targeted towards a very narrow specialized group of readers: pollen and charcoal people working in the Pacific Northwest who know that literature well. It needs to be made more accessible to the wider community, especially as Climate of the Past is a European journal. It can be greatly improved simply adding explanatory parenthetical phrases where commented. For example, define in Methods what are your tree classes: invaders, resisters, etc, instead of having the reader guess.

Response: To expand the appeal of the manuscript beyond specialized paleoecologists in the Pacific Northwest, we have made substantial revisions to improve overall clarity and to synthesize results without overwhelming taxonomic detail that detract from the main findings. On the advice of the reviewer, a few sentences are added to the methods defining the fire-related functional groups, which provides greater context for readers and supports interpretation of the results. Given that this work is being undertaken in a municipal water supply area, water purveyors are considering the results in program-level climate change adaptation strategies. Due to the traditionally foundational nature of paleocological research, we can also add a few

sentences, if the Editor wishes, describing how the research is being used to generate actionable outcomes, in this case managing forests and fire to preserve high quality drinking water.

In particular, the Methods section needs to expanded to fully explain what was done so that a beginning grad student is able to reproduce the various analyses or apply them to their own data. For example, fully explain how you standardized charcoal influxes and calculated the bootstrapped CIs. Explain what is the goal of each statistical test: "in order to ...."

Response: We have expanded our methods section to improve interpretation for replicability. The standardization of charcoal influxes and calculation of bootstrapped CIs adapts methods developed by Blarquez et al., (2014) with published R scripts and analysis overview. As recommended, we now provide step-by-step explanation in our methods, including how data were rescaled by min-max, submitted to Box-cox transformation with maximum likelihood estimation of lambda, then passed to Z-scores using the mean and standard deviation. The data are then binned by 20-year steps defined by a base period with beginning and ending ages (early- and late-Holocene). The composite curve is a smoothed local regression (locfit()) unresampled with bootstrapped confidence intervals (at 0.95 and .05) resampled from site replacement (5000 iterations). Throughout the methods, a few short sentences have been added to describe the purpose of various statistical tests.

Don't use acronyms unless you use them more than 7-10 times in the paper. They clog the paper and the reader gets cranky trying to look them up. CWHmm is such an example perhaps.

Response: Thank you for highlighting this issue. We do not wish to bog down the reader with acronyms and appreciate this being brought to our attention. Regarding biogeoclimatic ecological classifications, we feel they are readily represented in their acronym form (containing information about the location, dominant species, and climate variant). With only two subzones represented in the paper, we feel that including those particular acronyms is a descriptive trade-off. However, the paper has been revised to reduce other acronyms and improve readability. We re-write these classifications in full at the beginning of new sections so the reader does not need to refer to acronyms in the Introduction. In addition, we have chosen to write out arboreal and non-arboreal pollen throughout, instead of using acronyms (AP and NAP).

Figures and tables and their captions have to be able to stand apart from their text, so you should write acronyms in the captions out in full.

Response: We have revised the text as recommended.

Why did you use Tmax from the GCMs instead of Mean July air temp which is what you are comparing to?

Response: This is a valid point. The initial analysis considered Tmax to capture greatest seasonality in temperature trends with majority of fires in western Canada occurring during the mid- to late-summer. We have changed our analysis to present mean July temperature (Temperature_7), as this is more directly comparable to the MJAT paleo proxy reconstructions and still adequately captures the seasonality.

Need a more detailed explanation of how MAP is calculated from DWHI as they seem to be different but you also use them interchangeably.

Response: We have briefly expanded the methods to clarify how MAP was reconstructed using a pollen derived ratio of Douglas-fir to western hemlock. The two papers cited (Brown et al., 2006 and Brown and Schoups, 2015) provide detailed methodology on the development of the transfer function, a regression model using regional surface pollen spectra and gridded mean monthly precipitation (PRISM; Daly et al., 1994). We also clarify that MAP and mean July temperatures were extracted from additional published models including chironomid-based reconstructions.

Reorder Fig 2 lakes to follow your regular order. **Response: Revised, done.**

Did you play with the smoother type to see if you could get SNI > 3? Moving mode can work well.

Response: Yes. We adjusted the smoothing parameters and tried all of the detrending options available in *Tapas.R*. We used the sensitivity screening to optimize our smoothing window width. We found that the robust Lowess consistently performed the best. We address the low SNI in a new paragraph in the Discussion addressing limitations in the approach used. Reduced SNI can have meaningful interpretations and may emphasize causal mechanisms such as environmental conditions (bioturbation) or could be characteristic of frequent low-severity fires where there is little variation in charcoal production (Agee, 1993; Kelly, 2011). We caution interpretation of charcoal records with low SNI but feel confident that our interpretation within the context is justified.

Don't italicize family names. **Response: Revised, done.**

I don't see how you can conclude from peak size that the fires were bigger, they could have simply been closer. You are making this assumption several times and it seems not well founded as you do not have charcoal particle size data.

Response: This is a valid criticism. We agree that we can not conclude if fires were bigger or of higher severity based on peak magnitude. It is possible that proximity to the basin is influencing charcoal deposition and peak size. Notably, in interpreting peak magnitude, we attempt to include other proxies such as magnetic susceptibility and charcoal morphotype accumulation rates. However, given the reviewer's legitimate concern, we have revised statements to ensure no definitive conclusions are drawn where lack of evidence exists.

Other specific comments

Line 194 – explain in more detail the chironomid-based temperature estimates.

Response: A brief explanation has been added in parentheses.

Line 229 – have you tried significance tests correcting for autocorrelation (referring to changes in relative abundance of fire-related functional pollen types).

Response: We started conducting various unconstrained (principal components analysis, detrended correspondence analysis) and constrained (Redundancy analysis) ordination on pollen data and functional groups, but given the length of the current manuscript and multiplicity of analyses already included we felt additional analysis was getting outside the scope of the current paper. We hope to explore these relationships further in the future.

Line 378 – statistically or dynamically downscaled or both? Clarification needed.

Response: The PALEO-PGEM Series is statistically downscaled. It's derived from a statistical emulator of a GCM, spatially downscaled using anomaly-adjustment of modern baseline data (Barreto et al., 2023). This has been indicated in the manuscript.

Line 470 – Did local indigenous nations use the mast [of Garry Oak] to make flour?

Response: Good question. The association with local Indigenous nations and Garry oak is related to cultural management of forests in the area. Traditionally, fire maintained open oak savannas were used to promote important plant foods and game (Pellatt and Gedalof, 2014; Pellat et al., 2015). We are not aware of acorns being used for flour but we will reach out to local ethnobotanists to see if this association exists. Regarding Indigenous burning, we present some evidence that is tantalizing to interpret as being related to human burning. At the same time, we recognize that there is some debate about climate vs human fire signal in the region, and stress that the drivers of the slight resurgence in oak is not definitively established here. In fact, the

important aspect of this work is that it documents a subtle regional increase in oak that has not previously been described or discussed.

Line 520 – give range for comparison.

Response: Need further clarification on request. Is the reviewer asking for contemporary annual range in summer precipitation? If so, we can easily add these values.