Reviewer 2

Overall, the topic of this research effort is very important. Understanding the relationship between climate, topographgy, and land use/cover are critical for forecasting how critical migratory bird habitat in the future and helping management agencies strategize their conservation planning investments. The approach this group took is unique and uses large data sets to try and determine climate, land use, and topography variables that might correlate with the permanence class of a wetland. The lack of detail given in this manuscript makes it hard for me to understand the exact reasoning behind including such a large amount of covariates in this modeling exercise and the authors need to do a better job of explaining the proposed mechanisms of why and how different covariates would impact a wetland's permanence class. Currently the results presented have little utility for other researchers or managers.

We thank the reviewer for their feedback and will make revisions to the manuscript to better communicate our findings, per their suggestions.

Defining pond permanence is critical for establishing the utility of such a metric for managers and for creating more direct links between your plethora of statistical covariates and your response variable (categorical and static wetland permanence class). In your introduction you define hydroperiod (L20, L26), mention wetland water levels (L24) as well as ponded frequency (L22), and declines in pond permanence (L28), wetland sensistivity (L29), and wetland permanence (L57). You then skip to mentioning wetland permanence class (L62) without first defining that term or how wetlands are categorized into these different classes. In L70-71 you introduce Stewart and Kantrud's wetland pond permanence classes, but bury the details in the appendix. Please define those classes in the methods and move Appendix 1 to the main body of the manuscript. Or, at least some version of that appendix that allows the reader to understand what variables could influence a wetland from being defined in one permanence class or another. This would make a much more clear link between your model covariates and response variable.

We agree that defining pond permanence earlier in the manuscript would improve general understanding on what variables could affect water levels in a wetland. As such, in a revised version of this manuscript, we will define pond permanence in the introduction and move the table in Appendix 1 to the main text (i.e., Table 1).

Use of the term climate when only considering 11 months of temperature, precipitation, and one winter's snowpack data. There is a temporal mismatch between your response variable that is a very statically defined permanence class of a that is the result of many centuries of wetland ecosystem development in response to long-term (>30 years) climate variables as well as the topographic and land use/cover setting of each wetland. I have a hard time making this connection and the methods and discussion do not go into enough detail for me to be convinced that the data used to develop your covariates could sufficiently explain mechanisms of how climate can determine the permanence class of a given wetland.

Reviewer 1 also raised this important consideration and we give a fulsome explanation above. However, we agree that a revision of the manuscript must make clear our use of annual (2013-2014) data on climate variables rather than long-term average climate data. We will also address our reasoning for using the 2014 data in the manuscript, similar to what we explain above in response to Reviewer 1.

L89 2018 excludes a rapidly growing body of research

We acknowledge this limitation. With COVID and related delays, it is taking longer than perhaps it did previously to move from literature review to publication and new papers are always bringing new knowledge into the sphere of scientific discourse. However, we contend that the last three years has not materially changed our scientific understanding of the key variables characterising land cover/land use and climate, as it pertains to wetland permanence classes – at least not in terms that would alter the representative metrics we could calculate with available data.

L29- semiperms most sensitive…confusing and potentially irrelevant

We will better clarify why semi-permanently ponded wetlands are likely to see grater declines in their hydroperiods than other, shorter-hydroperiod wetlands.

L30 – change will to may experience as much as….there are many different accepted models for the future, see

McKenna, O. P., Mushet, D. M., Kucia, S. R., and McCulloch-Huseby, E. C.. 2021. Limited shifts in the distribution of migratory bird breeding habitat density in response to future changes in climate. Ecological Applications 00(00):e02428. 10.1002/eap.2428

Reviewer 1 also noted the lack of clarity here and we will revise.

L31 -wetlands "may be lost forever" unless this is talking about draining/filling the wetlands are not lost it is the ponded water that is lost. Depressional wetland basins persist if wet or dry.

Where we say "Furthermore forecasts suggest that many of the wetlands in the southern and western PPR may be lost completely, driven by drier climate conditions in these area..." we will revise to say "may lose their ponded water completely..." to more accurately reflect that the wetland boundary is not defined by the open water and that dry wetlands are still wetlands.

L40-41 different font "areas lower in the watershed"

We will make this change.

L57- This wording is a bit too strong. There are other examples of this in the southern PPR:

McKenna, O.P., Renton, D.A., Mushet, D.M., DeKeyser, E.S. 2021, Upland burning and grazing as strategies to offset climate-change effects on wetlands: Wetlands Ecology and Management, https://doi.org/10.1007/s11273-020-09778-1

McKenna, O.P., Mushet, D.M., Anteau, M.J., Wiltermuth, M.T., Kucia, S.R. (2019) Synergistic Interaction of Climate and Land-Use Drivers Alter the Function of North American, Prairie- Pothole Wetlands: Sustainability [Special Issue "The Importance of Wetlands to Sustainable Landscapes"], https://doi.org/10.3390/su11236581

In this statement we meant to emphasize that terrain is not commonly considered in studies that quantify the individual and combined effects of climate and land use on wetland permanence. We will revise for clarity.

Methods

Overall, much more detail is needed to understand how your response variable and your covariates are defined. By defining these with more detail and citation then the reader can better understand the mechanisms by which the different continuous covariates could potentially influence or correlate with a categorical permanence class

Reviewer 1 also requested more of the detail that we provide in Appendix B be incorporated into the main text. There, we provide a list of the key variables identified by our literature review, some citations of papers from which we derived them, metrics that we considered representative and a bit about why they are likely to affect wetland permeance class/wetland hydroperiod. We will incorporate more of this information into the main text.

L70 write out the permanence classes and what criteria are involved with classifying wetlands

We will make this change by moving the table in Appendix A, which details this information, to Table 1.

L73 add a citation for “Natural Region” boundaries.

We will make this change.

L81 add citation for your spatial layers used to map those boundaries in figure 1.

We will make this change.

L84-85 please explain why no wetlands were within 1000m of each other. This is potentially a huge limitation of this modeling approach. In some areas of the density of PPR wetland basins can be almost 10 wetlands per sq km See McKenna, O. P., Mushet, D. M., Kucia, S. R., and McCulloch-Huseby, E. C.. 2021. Limited shifts in the distribution of migratory bird breeding habitat density in response to future changes in climate. Ecological Applications 00(00):e02428. 10.1002/eap.2428. Prairie-pothole wetlands also can be connected to each other via surface flows that create wetland complexes. To only choose one wetland in a complex without classifying the rest of the wetlands seems to not have much utility for scientists or managers studying these systems.

The region we studied has complexes of wetlands, though in the NW edge of the Prairie Pothole region we see more arid conditions and less surface runoff or fill-and-spill connectivity among them than in less arid portions of the PPR.

We did not exclude wetlands that were part of a complex, rather we did not select wetlands as one of our focal study sites that were within 1000 m of another focal wetland. To do so would have given rise to a lack of spatial independence that would compromise our analyses of terrain and land cover variables because their surrounding landscapes would have overlapped. Spatial autocorrelation is often ignored, but this violates standard assumptions of statistical inference and has been shown to bias quantitative outcomes in ecology. Previous investigation into spatial autocorrelation in our study system identified a lack of spatial relationship (i.e., structure) among land cover and wetland conditions beyond 1000 m (Kraft et al. 2019). This range of spatial autocorrelation (i.e., threshold) has supported the analysis of spatially independent wetlands and wetlandscapes with a focus on open water wetlands (Ridge et al. 2021), topography (Branton et al. 2020), and land cover (Evans et al. 2017). Therefore, not only do we ensure a spatially independent sample of wetlands, but we also contribute to on-going research that has consistently operated at this dimension of spatial independence and strengthens the relationship among those scientific investigations.

We agree that connectivity to a wetland complex would influence a wetland's hydroperiod and incorporating this in future research would likely increase model fit, but excluding this driver of wetland hydroperiod should not have altered our conclusions about the relative importance of climate, terrain and land cover domains.

L89 I understand that limitations of this approach and the challenge of summarizing pertinent literature, but I think there are some key papers missed that are summarized in McKenna, O.P., Mushet, D.M., Anteau, M.J., Wiltermuth, M.T., Kucia, S.R. (2019) Synergistic Interaction of Climate and Land-Use Drivers Alter the Function of North American, Prairie- Pothole Wetlands: Sustainability [Special Issue "The Importance of Wetlands to Sustainable Landscapes"], https://doi.org/10.3390/su11236581.

The inclusion of some of these papers might have allowed for inclusion of soil moisture/drought indices variables that are much more appropriate uses of the term "climate" that the 11 months of precipitation and temperature variables currently included in the model. The current "climate" covariates may be decent predictors of the current year wetland inundation status, but do not seem to me to be appropriate for predicting a static categorical permanence class of a wetland.

Certainly, McKenna et al. (2019) is a relevant paper that our literature review would have captured if we redid our review in 2021. Unfortunately, as we describe above, we needed to complete the literature review to identify the variables to represent with metrics to us in the modelling, and this – with COVID related delays – took time. We contend that it would not have substantively altered which metrics we could use to represent different relevant variables because we are also constrained by data availability. Regarding the comment about annual data on climate variables vs. climate data, we agree this needs to be made clearer in the manuscript and will do so if given the opportunity to revise. We give our reasons for using 2014 data above and explain that fine tuning the climate representation would potentially strengthen the relative influence of climate variables, but given climate already emerged as the most important domain of variables it would not likely alter our conclusions.

L105 similarly, land cover data from one year (2014) seems to be on the wrong temporal scale of the wetland permanence class. I would suggest something more stable like a multi-decadal average

Based on work by Kraft et al 2019, we see that physiochemical conditions in a wetland (measured by soil and water chemistry) are quite congruent with surrounding land cover of that year in our study area. As such, we felt that using land cover data for that year would be most appropriate. More, attempting to take a multi-decadal average of nominal data introduces issues of weighting through time – should the land cover from 10 y ago be weighted the same as the land cover last year? Future research could investigate a defensible approach to do this, but based on our work published in Kraft et al. (2019), we contend that the result would not substantively change our conclusions about the relative importance of climate, terrain and land cover variables.

Kraft, A. J., Robinson, D. T., Evans, I. S. and Rooney, R. C.: Concordance in wetland physicochemical conditions, vegetation, and surrounding land cover is robust to data extraction approach, edited by D. G. Jenkins, PLoS One, 14(5), e0216343, doi:10.1371/journal.pone.0216343, 2019.

L106 more detail is needed to understand why distance to road would be included as a covariate in your model. I do not see the direct connection between that and permanence class. Much more detail could be made in your selecting variables section as well as in your introduction as you hypothesize how climate, land use, and topography

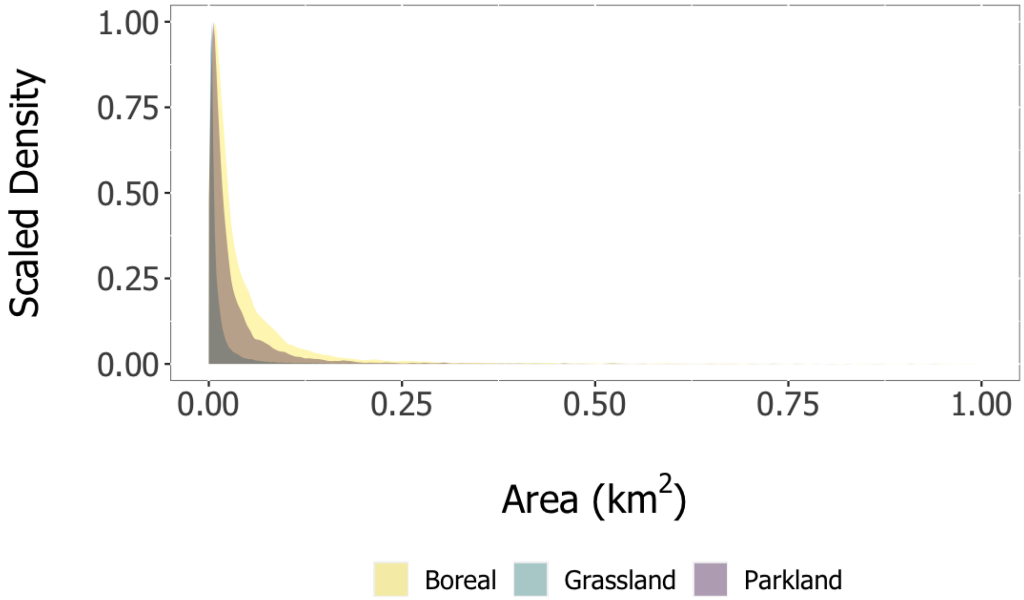
Though this information is included in Appendix B, where we briefly explain why each variable may influence wetland hydroperiod, we agree that expanding on this information may be helpful to readers. In Appendix B, we note that, as mentioned in Shaw et al (2012), roads divert surface runoff. Consequently, wetlands that are closer to roads, or perhaps, are in landscapes with higher road density, will have shorter hydroperiods. We will provide more background on this in the addition to the manuscript methods where we bring in some of the text from Appendix B.

L110-111: List a range of the size wetland basins and the catchments somewhere so the reader can determine if 25m DEM is high enough resolution compared to the size of the wetlands. In my experience 3m DEM is a much more appropriate resolution for prairie pothole wetlands. Also, list the different terrain variables here and allude to why they may influence wetland permanence class.

Unfortunately, a 3 m DEM is not available for our study area. As we describe in our response above, our metric selection (Appendix B) was also constrained by data availability and as new technology and data layers become available, future research could build upon the foundation we lay in this manuscript.

In a previous paper (Kraft et al. 2019) we compared the 25 m DEM to a 10 m DEM that was available for a small sub-section of our study area and found that smaller resolution DEMs led to little material difference in the strength of concordance with land cover from catchments and land cover extracted from symmetrical buffers between 200 and 500 m in radius, though catchments were typically smaller and more variable in size when defined with the 10 m DEM. We list which variables are global and which are local in Appendix B. However, we will include these details in the revised version of the manuscript to improve reader comprehension of our methods. We did not use the DEM to delineate the boundaries of the wetlands in this study – they were taken from the Alberta Merged Wetland Inventory, and so this would not be at play in determining the sizes of our study wetlands. Below, we show that while there are differences in wetland size by Natural Region, generally, wetlands were smaller than 0.025 km^2. This information will be integrated into a revised version of the manuscript.

Region	**Median (km^2)**	**Mean (km^2)**	**Standard Deviation (km^2)**
Boreal	0.0226	0.08008235	0.9564564
Grassland	0.0058	0.02209926	0.2327619
Parkland	0.0154	0.04341342	0.3077994



L114: which variables are global? At 100m resolution how can you relate that elevation, slope, etc. to an individual wetland basin?

L117: Since this is a stats model and not a mechanistic model my understanding is that you did not quantify relative contribution, you quantified correlation strength. Also, when you say, "land cover/land use and terrain for different wetland permanence classes" you need something between for and different.

We agree that, strictly speaking, regressions and their statistical siblings are measuring correlation, yet it is standard practice to assume directionality and describe one dependent variable as being influenced by the independent variable(s). This is also true in boosting models. For consistency with the literature using this modelling approach, we believe it is appropriate to say, "relative contribution" instead of "correlation strength," though we agree with the Reviewer that causality is not experimentally established in our study. We can add a caveat to this effect to the discussion.

Also, in this data analysis section please describe the relative importance methods and what the relative gains metric used in figure 2 means.

We will add a brief explanation as to what relative gains means in Figure 2.

2.4.1 Predicting wetland permanence class: This section could use more defense of why you used the covariates you did and help elucidate the mechanisms of how they could influence permanence class.

We understand the importance of this point and that we could improve our communication about the relationship between the covariates and the mechanisms they represent that influence permanence. A list of these variables and literature that cite them for their relevance is provided in Appendix B. We will improve our communication about these mechanisms as well as integrate aspects of Appendix B into the manuscript or better link the manuscript to the appendix.

Results

Overall, much more detail is needed. Currently, the results as presented in the figures 4-8 are extremely hard to interpret. More work is needed to consolidate results to communicate the most pertinent findings to the readers.

We will consolidate Figures 4-8 to make it easier to follow the main points in the partial dependence plots. Furthermore, we will revise the text in the results section to improve reader comprehension.

L142-143: How can you point this error to lack of correlation between covariates and response variable and not a mismatch of spatial and temporal scales between covariates and response variable? I would love to see this model re-run with improvements on selection of covariates and the inclusion of wetlands close to each other with better elevation data to map wetlands of different permanence classes in the same wetland complex.

As we describe above, we are unfortunately constrained by what data exist at a large spatial scale in our study area. Ideally, we would have a high-resolution DEM (e.g., 3 m), high accuracy data on hydroperiod, land cover/land use, and climate collected over the same interval simultaneously across the full study extent for all 40,000 wetlands. Yet we contend that the issues of climate change and habitat loss are urgent and the absence of perfect data should not prevent us from attempting to understand the drivers of wetland permanence class using the (admittedly imperfect) data that do exist: the Alberta Merged, South and Central wetland inventories, the 25 m DEM, and the AAFC land cover data. We can support these efforts by interrogating the effects of the data limitations we face. For example, we found only minor differences in outcomes between 10 and 25 m DEMs (e.g., Kraft et al. 2019); temporal mismatch is a function of remotely sensed data (e.g., landcover) that is typically reported annually; spatial mismatch will almost always be present across multiple variables represented across large spatial extents, and it is just as likely that if we ignored spatial autocorrelation (i.e., spatial independence in sample wetlands) that a reviewer would have requested we ensure our samples are spatially independent. Because our level of analysis covers a large spatial extent with data limitations we focused on the aggregate comparison between the domains (i.e., types) of variables: climate, topography, and land use/cover, rather than issues that can be resolved at plot or small-spatial extents.

In a previous response, we explain why we selected landcover and climate data for one year and we also explain that we did not exclude wetlands that were part of a complex, but did not select two wetlands to be part of our 40,000 from within 1 km of each other to defend against spatial autocorrelation.

L147-148: explain the directionality of this relationship between spring temp and permanence class.

We can add a sentence here and reference Figure 5 IV-VI, which shows the probability of the wetland belonging to each permanence class across the observed range of spring temperatures. The pattern was clearest in the Grassland (Figure 5VI) where warmer spring temperatures coincide with more temporary and less permanently ponded wetlands.

L152-153: move last sentence to the discussion.

This seems to refer to the sentence "Yet, unlike climate, land cover/ land use did not vary systematically among the three Natural Regions (Figure 4E-H)." Reviewer 1 caught that some of our references to figure numbers are incorrect and this is one such instance, for which we apologize. We will double check all figure numbers in a revised version. It should read "Figure 3E-H." We are inviting the reader to compare Figure 3 A-D vs. Figure 3 E-H to illustrate how the three Natural Regions are segregated in terms of their climate variables but exhibit high overlap in terms of their land cover/ land use metrics. Consequently, we do not believe this sentence should move to the discussion section.

L159: instead of “is sensitive” should read “correlates to observed differences in”

We will make this change, but emphasize also that this terminology is consistent with descriptions of regression and boosting models.

L161: higher snowpack amounts? There was only one season of snowpack, how is this plural?

We will make this change.

Discussion

Explore more what your results mean for your different climate, land use, and topography variables. There is much lacking for linking correlations to causality.

As described earlier, we will incorporate into the main manuscript text more of the Appendix B details where the selection of variables and the rationale for different metric choices are explained. This should give us the opportunity to better defend our interpretations as supported by both our model and the literature.

L168: This is a very bold leap based on your data to say, “our findings support the assertion that climate change will affect wetland hydroperiod” Your model does not simulate wetland hydroperiod and it only used 11 months of precipitation and temperature data.

We will revise this to more clearly say “our finding support the assertion of other published studies (e.g. Johnson and Poiani 2016, McKenna et al. 2019), which conclude that climate change will affect wetland hydroperiod or permanence class.” Given that our model to predict wetland permanence class identified annual data on climate variables to be the most important domain of predictors compared to land cover and terrain, we see this as support for other published studies that assert that climate change will affect wetland hydroperiods because permanence classes are assigned to categorize prairie pothole wetlands into bins of differing ponded water permanence. As we describe above, fine tuning our representation of climate in our models should only increase the importance of this domain of variables.

Johnson, W.C. and Poiani, K.A., 2016. Climate change effects on prairie pothole wetlands: findings from a twenty-five year numerical modeling project. *Wetlands*, *36*(2), pp.273-285.

McKenna, O.P., Mushet, D.M., Anteau, M.J., Wiltermuth, M.T., Kucia, S.R. (2019) Synergistic Interaction of Climate and Land-Use Drivers Alter the Function of North American, Prairie-Pothole Wetlands: Sustainability [Special Issue "The Importance of Wetlands to Sustainable Landscapes"], https://doi.org/10.3390/su11236581

L171: climate is not the only element driving. Replace with “element correlated with”

We will make this change.

L173: unpack the term “terrain” what aspects about terrain specifically were related to permanence class of a wetland?

The line in question says “Yet, climate is not the only element driving wetland permanence class in Alberta’s PPR - our analysis used a relatively coarse DEM (25 m), and we nonetheless found that terrain was important in predicting permanence class.” Here we are referring to the terrain metrics we analysed in our modelling and define in Appendix B. Their relative importance in the three Natural Regions are depicted in Figure 2. We will move the details from this Appendix to the main text. In general, this class of metrics includes both measures of terrain roughness/texture and measures of slope and curvature.

L176 see previous comment

L185: Figure 3B is Max temp in winter

Apologies, this references Figure 4, not Figure 3. It should be Figure 4-II. As described above, we will go through in our revision and correct these errors in figure references. They were not adjusted when we added a map to the manuscript and so are incorrect in a few places.

L187: your modeling exercise does not include future changes in climate and does not explicitly explore sensitivity to climate change

The sentence in question is "Because climate forecasts suggest that warmer springs and changes in precipitation timing are likely (Zhang et al., 2011), our findings support previous studies that suggest PPR wetlands are sensitive climate change (Johnson et al., 2010b; Paimazumder et al., 2013; Schneider, 2013; Viglizzo et al., 2015; Zhang et al., 2011)." As with the Reviewer's comments about L168, here we are not intending to suggest that our model involved climate forecasts, but rather that our finding that climate was the most important domain of variables predicting permanence class, and that this is in alignment with other published studies who also conclude that wetland permanence class or hydroperiod will be sensitive to climate change because climate variables exert an important influence on wetland hydroperiod.

L199: the "natural frequency" I think this should read the "classified frequency"

We will make this change.

L224. This fires sentence is misleading. All depressional wetlands occur in topographic lows by definition.

We will modify this sentence to clarify that we are speaking about topographic lows at regional scales. Yes, all depressional wetlands occur in local depressions, but at a larger spatial scale of consideration (e.g. 2 or 5 km) some depressional wetlands may form in lower topographic lows than others. Prior work looking at such networks or wetlandscapes finds that more permanently ponded wetlands tend to be in lower topographic positions at the wetlandscape scale, with more temporarily ponded wetlands more typically in higher positioned depressions.

L233: “which aligns with our model results” How does this align? I need a lot more description here to be convinced of that.

We will provide more context for this statement.

4.5 Model Error: Need to explicitly address how mismatch in “climate” variables, only one year of land-cover classification, and overly coarse elevation data could all contribute to model error.

We describe how we will address this concern in the revised manuscript in our responses above.

Figures

Overall, these figures need to be distilled to better visualize the main takeaway and results of your study. In Figure 3 it would also help to convert to a fractional frequency to standardize the differences in number of wetlands between Natural Regions and avoid visualizing differences in frequency distribution that are not relevant to your analysis.

We will revise Figure 3 to show scaled density. We will also revise our references to figures throughout the text to correct any misnumbering and better describe them.