Reviewers' Comments and Respective Responses

on the

Draft Report Titled

"Water Quality Assessment of Wetland Drainage and Retention Scenarios"

Report Prepared for:

Saskatchewan Conservation and Development Association and the Saskatchewan Water Security Agency

Report Prepared by:

Dwight Williamson

Reviewed Report Draft Dated May 13, 2022

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Comments from Reviewer	Response from Dwight Williamson
(1).Reviewer did not have any comments on the water quality report.	(1). Okay. No additional revisions required.

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(2). Overall I found the reports each had clear objectives with the descriptions of the research and the application of methodology to be appropriate and quite comprehensive. However, I did find that the presentation in all reports was, at times, difficult to follow and not accessible to a reader who was not an expert in the field. While I understand that this is necessary to complete the work at the level of rigor required, I provide the following suggestions to enable a broader audience to engage with the work completed:	(2). Good. No additional revisions required.
(3). It was not apparent to me who the intended audience is for these reports. A clear statement of the primary audience would enable the reader to better position themselves as they interpret and evaluate the findings.	(3). This is a good comment, but no change is thought necessary. The reports were written for a scientific/technical audience since they were to be subjected to peer review, but the key messages were intended for government policy-makers in Saskatchewan. A plain language summary was provided to bring forward, in a coherent manner, all of the key findings and the key messages. It is thought that this should suffice for the water quality report.
(4). While I found all 4 reports well written, there were a small number of minor edits that I identified: Report 1 – page 113, the word "prairies" is misspelled; Report 1 – Figures 7 through 17 have extremely small font on axis labels and heading making it difficult to read these figures.	(4). The spelling error has been corrected. I have reformatted the figures throughout with larger fonts.

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(5). Each of the reports should include a detailed glossary of terms. I found that all reports used many terms that are likely not generally understood, and without a clear and precise definition the results can be difficult to interpret correctly. I do recognize that some terms (not all) are defined within the text but having a well-placed glossary that can readily be referred to will significantly contribute to reader understanding. I would also suggest that equivalent terms across reports should be defined and applied consistently. Below I provide an incomplete list of the terms I thought required definition in a glossary:	(5). A detailed glossary, while a good idea, will take a large amount of time to complete. It is thought not to be necessary since the body of the report is intended for a technical/scientific audience familiar with most terms and a plain-language summary has been included for those readers less familiar with the literature.
(6). All of the reports highlighted significant levels of uncertainty with respect to the results. This makes the interpretation and the application of the results difficult. This is clearly not unexpected in any analysis addressing more complex environmental issues. The Intergovernmental Panel on Climate Change (IPCC) has developed a terminology framework to address confidence and certainty with respect to climate science. I would suggest a similar approach applied to the findings of these reports could make the results more accessible and useable by relevant stakeholders. I have attached a basic summary of the terminology used by the IPCC to represent levels of confidence and likelihood of an outcome to illustrate this point.	(6). This is a good idea but because of time limitations, it is not possible to incorporate this recommendation at the present time. Information has been qualified throughout the report in terms of its uncertainty and a section had already been previously included outlining "Uncertainties and Limitations". It is recognized that this does not follow the IPCC standardized terms for expressing levels of confidence, but it is believed to be sufficient at this stage of scoping-level analysis. It is noted that the section "Uncertainties and Limitations" has been expanded as reviewers' comments were incorporated.

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(7). Report Concerns: In general I found the reports to provide excellent reviews of the science and together represent a good start at developing a more comprehensive understanding of the role of wetland drainage, wetland retention and wetland restoration in a number of downstream environmental costs. While my disciplinary background does not enable me to identify any specific errors or methodological concerns with respect to the individual reports, I did identify the following: The Steward and Kantrud wetland classification system uses vegetation patterns and plant species to determine wetland permanence and wetland class. Therefore, I felt that there should be more discussion about the impacts on the results of assuming that wetlands less than 0.5 acres are predominantly Class 1 and 2 wetlands, wetlands less than 3 acres are predominantly Class 3 wetlands and wetlands less than 5 acres are predominantly Class 5 wetlands. I understand the reason for using wetland size as a proxy for wetland permanence but the limitations of this assumption should be clearly stated.	(7). This is a good comment and is worth pursuing. However, it is probably best undertaken by Mr. Daniel Phalen, the author of the initial analysis between wetland size, permanence, and comparison to the Steward and Kantrud classification system. Since the relationship between wetland size and permanence does not play a large or pivotal role in the water quality analysis, the existing overview obtained from Phalen (2022) is thought to be sufficient at the present time. Additional work in this regard is warranted but is thought not to be needed to complete the water quality analysis report and as mentioned, is best done by Mr. Daniel Phalen as a continuation of his existing work in this regard.

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(8). Report Applications: Given the nature of the data and the methodology used to complete the analysis in the 4 reports I understand that most of the results were analyzed as average values for the subject basin or for the landscape addressed. However, there is a case to be made for a marginal analysis of wetland drainage. If wetland drainage is influencing water quality, water quantity or biodiversity the reports suggest that there are threshold effects such that a small increase (or decrease) in wetland drainage within a landscape can result in significant changes in these downstream or off-site impacts. I would suggest, if possible, completing focused marginal analysis, even as a preliminary case study, would be quite informative to management decisions and policy development. This analysis would better inform cost-effective policy approaches that provide levels of environmental quality.	(8). This is a very good suggestion. However, it would need to be undertaken as a separate project by Saskatchewan Water Security Agency as a follow-up to the current four studies. As a consequence, no change is contemplated at the present time to the water quality analysis report. Although I am uncertain of the scope of work, it is possible that the recommended marginal analysis may be included, at least in part, within Dr. John Pattison-Williams component of the wetland drainage scenario project - work which I understand may be underway.
(9). I recognize that an economic analysis of wetland conservation, wetland drainage and wetland ecosystem services was beyond the scope of these 4 reports. However, developing a better understanding of the costs of wetland retention and/or the benefits of wetland drainage would inform program and policy from the wetland supply side and informing incentives and dis-incentives to landowners. At the same time, improving the understanding of the downstream costs and/or benefits of wetland management in terms of changes in water quality, water quantity and biodiversity will also be essential to support policies and programs that contribute to social welfare.	(9). Saskatchewan Water Security Agency has commissioned Dr. John Pattison-Williams to undertake an associated economic analysis.

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(10). Each of the authors highlighted, more or less, the importance of interpreting the findings in context of the recommendation of the other research. I recognize that this has not yet been completed but I do want to emphasize the importance of this integration. For example, Report 3 highlighted the role of wetland vegetation management and removal to enhance groundwater recharge, while this management would appear to have significant negative impacts on the value of those same wetlands for wildlife habitat and/or nutrient management. While more than one approach could be applied to characterize, and perhaps quantify, this integration, tradeoff analysis may be a productive framework to apply. Developing tradeoff curves representing levels of ecosystem functions, ecosystem services, and potentially economic costs, would enable a more informed application of the research results. I have included an example of a somewhat relevant set of tradeoff curves that were published in the following article: Lang, Y. and W. Song. 2018. Trade-off Analysis of Ecosystem Services in a Mountainous Karst Area, China. *Water*. 10(3)	(10). This is also a good suggestion but undertaking a trade-off analysis, if pursued, will need to be done as a separate project by Saskatchewan Water Security Agency.

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(11). The reviewer did not have any comments onthe water quality report.	(11).Okay. No additional revisions required.

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(12). Page ii: Eutrophic lakes are of concern from some perspectives; however, the natural trophic state of lakes is eutrophic. The degree of eutrophication (the process) and the impact to lakes (e.g. algal bloom related) is not as well understood.	(12). Good. I don't believe additional revisions are necessary. I understand the comment - not sure that I agree that the natural state is eutrophic, but rather, the natural state is a continuum of eutrophy, depending upon a lake's age but one that is progressing towards eutrophic.
(13). Page ii: These terms have different meanings in different jurisdictions. Given this is a SK document, suggest defaulting to SK terms and noting differences where they arise in d/s jurisdictions. General surface water quality assessment (i.e. outside regulatory world) does not use the term 'standard' in SK. I recognize others do (e.g. North Dakota).	(13). Good, I have revised the text to now just use the term "Objectives", have created a footnote to more fully explain the various terms, then leave the detailed text unchanged with the terminology as per the cited publications, wherever this is applicable.
(14). Page iii: Potentially confusing b/c also using commas to separate 1000's.	(14). Agree, but the recommended change would also be confusing. I have left the lists unchanged recognizing that they create dense text in some cases, requiring a close and careful read.
(15). Page iii: spelling	(15). Good, correction has been made.
(16). Page 9: These were set as notional targets.	(16). Agreed - I only use the term "target" as it is the only term used by the Lower Qu'Appelle River Watershed group and I include the group's text in italics and within quotations. Nevertheless, it is agreed that its usage suggests a "notional" target.
(17). Page 9: Paleolimnological studies also indicate natural eutrophic state, e.g. Hall and Leavitt (1999 44:739-756) infer total chlorophyll for the chain of lakes. Apart from Pasqua, the d/s lakes generally show little change. See pasted picture below titled Inferred Algal Biomass Increase. There is reference in Gilchrist's diary to green scum on the Qu'Appelle lakes (see pasted images below from diary). Frederick Gilchrist(1859-1896). Fisheries inspector.	(17). Very good - helpful information. I make reference to the Hall and Leavitt study later in my report, so will not mention it here. The Gilchrist information is good as well, but I could not quickly pull a digital version of his diary, so will not make reference to it. I think the point that these lakes have historically been eutrophic has already been well made, so no further revisions are required. Nevertheless, this is helpful information.
(18). Page 10: Thompson Reservoir does have notably high nutrient levels.	(18). Good. That is essentially what is stated - high nutrient levels present a challenge to treating water from Thompson Lake. Good comment.

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(19). Page 14: As I recall, these reports focused on trend analyses of tributaries; might be better to include references of more direct specific assessment of the change in trophic state of L.Wpg. e.g. Schindler et al. 2012. JGLR 38:6-13 or work from Leavitt/Bunting.	(19). Good. I have added the Bunting reference. Also corrected the reference to Bourne *et al.* (2002) here and throughout wherever it appears as only two authors rather than three.
(20). Greg McCullough et al's 2012 paper in JGLR on hydrological forcing related to trophic change in L.Wpg would be an appropriate paper to cite here.	(20). Good. I have reviewed and added this reference.
(21). Page 15: It would be useful to include the same figure but with mean annual flow-weighted concentrations associated with the major inflowing tributaries. This is important with respect to understanding which tributaries have a 'concentrating' vs. a 'diluting' effect on lake concentrations (to keep it simple, there are other factors to consider). Arguably it would be beneficial to focus attention on reducing flows (so % flow contribution) of tributaries with nutrient levels that are greater than L.Wpg (and/or greater than the L.Wpg nutrient concentration guidelines).	(21). I would prefer not to make revisions to this figure since this is copied directly from the Manitoba (2020b) reference and would be difficult to change. There is an inset within the figure showing the proportion of mean annual flow contributions to Lake Winnipeg. This does allow the reader to pull some of the information that Dr. Davies is suggesting would be helpful. For example, the Red River supplies 68 % of the TP to Lake Winnipeg but only 15 % of the water whereas the Saskatchewan River supplies only 6 % of the Lake Winnipeg TP but 25 % of its water. Flow weighted mean concentrations would follow the same pattern.
(22). Page 16: This needs to be separated from the Assiniboine and Qu'Appelle rivers. Completely different system, enters in a different location, different volume and nutrient concentration.	(22). I understand that there are significant differences between the Saskatchewan, Assiniboine, and Qu'Appelle river systems. This is just a broad summary table extracted from Manitoba (2020a) and provided in the same form as it appears in the primary reference. Unfortunately, the limited timeline in the current project precluded re-working Manitoba's raw data files to more completely separate one river system from another.

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(23). Page 16: Please include corresponding flow volumes in this table so the relationship b/w flow and load is clear. Esp. since drainage is largely a matter of flow, these data are critical for interpretation.	(23). I agree that flow information is important. To a large extent, this information has already been provided in Figure 2, but I understand Dr. Davies' point. Unfortunately, this is the information as it is presented in the reference Manitoba (2020a) and given the timelines of the current project, I did not have sufficient time to go into Manitoba's data files to pull out this information.
(24). Page 16: Add a space b/w SK/AB and ON/US (consistent with table formatting). Makes it clear that these are not all together.	(24). Good. This formatting was done. The table is now clearer.
(25). Page 16: These both arise in SK and are completely different from the Saskatchewan River system so must be split out and treated separately. Also, not related to AB.	(25). Good. As with comment 22, this is just a broad summary table extracted from Manitoba (2020a) and provided in the same form as it appears in the primary reference. Unfortunately, the limited timeline in the current project precluded re-working Manitoba's raw data files to more completely separate one river system from another.
(26). Page 17: I understood these had been accepted by the MB gov as targets (i.e. are now enshrined, not just proposed). Perhaps I'm mistaken though.	(26). The regulation has not yet been passed, so they are still proposed.
(27). Page 18: Apart from internally drained basins, all the basins are shared. So arguably here, all water basins.	(27). Good, I have made this revision.
(28). Page 18: SK also has west and north flowing rivers (to AB and NWT); albeit these are well outside the prairie pothole region.	(28). Good, I have made this revision.

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(29).	Page 18: Apart from low flow years, a quick calculation of loads entering SK in the Saskatchewan River system (Battle, NSask, Red Deer, SSask) and the load leaving SK (Saskatchewan River) suggests lower loads at the MB border than at the AB border (i.e. there is a net retention of nutrients in SK). So, much more than a Cedar Lake effect in terms of nutrient retention on that particular system. A related question is what the loading might be without the large reservoirs \(i.e. due to the artificial nutrient reduction b/c of the reservoirs). The bigger question relates to the interaction b/w nutrient retention in the Saskatchewan River systems versus increased loads associated with drainage.	(29).	These are all good points which may warrant separate follow-up beyond the current water quality assessment of the wetland drainage and retention project. As Dr. Davies noted, and as Donald *et al.* (2015) and Bourne *et al.* (2002) quantify, considerable TP and some TN is sequestered in reservoirs in Saskatchewan and may be important to account for if additional loading occurs from future wetland drainage.
(30).	Page 19: Background objectives (nutrient) have an expected excursion rate of 10% over the long term. At least the higher objective does. Lower objective value is more challenging to evaluate what it means when it is not met.	(30).	Good. This is consistent with the statistical approach used to develop the objectives based upon the background approach. I had previously described the statistical approach using the 90th percentile under the section describing the PPWB's objectives, and made a minor revision to incorporate Dr. Davies' comment.
(31).	Page 21: Worth then stating other sites were not found to have a significant trend over time.	(31).	Good. I have made this revision.
(32).	Page 21: According to a presentation from Dave Donald in 2015, most of the wetlands in the area were below their spill elevations prior to 2005. This increased dramatically with the increase in precipitation ~2005. See accompanying figure I've pasted on the upper left-hand side of the page (from Dave Donald). When one transitions from a situation where ~10% of wetlands are at their spill elevation to one where in many years 40 to 60% of wetlands are at their spill elevation it presumably makes a large difference in terms of flows. Perhaps the picture is more complete by stating something to the effect of: water arising from the agricultural portion increased due to the filling and spilling of a significant proportion of wetlands during the wetter period, runoff from cropped lands under higher flow conditions plus incremental increases due to drainage.	(32).	Good. I have made the suggested revision.

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(33). Page 22: ... excursion of the PPWB's 90th percentile objective for phosphorus is now occurring at a frequency of more than 10% and trend analysis has found a significantly increasing trend, notably with increasing levels starting around 2005.	(33). Good, I have incorporated the suggested revision.
(34). Page 22: They are to suppose to be reviewed periodically; they have not been reviewed and updated for a long time.	(34). Okay. No revision is necessary since I indicated they are reviewed periodically.
(35). Page 22: was	(35). Good. I have made the revision.
(36). Page 22: ISRSB - was a temporary Board. It has completed its work.	(36). Good. I have made the revision.
(37). Page 22: Also the Poplar River basin with Montana. A bit more information here than is needed ... am just provided for frame of reference in case it is considered worth noting. There is no IJC Poplar River Board (at one time the Poplar was included with the Red River) but there is IJC reference to the Poplar (and as such it is eligible for funding assistance of its monitoring from the IJC\). The Poplar River Bilateral Monitoring Committee fulfills the responsibilities assigned by governments under the Poplar River Cooperative Monitoring Agreement, dated September 1980. The Agreement has been extended several times over the years, with the current extension going to 2027. There are water quality objectives on the Poplar, although sampling for most was suspended and the more recent focus has been on TDS and boron, which are calculated from Sp.Cond. Full table pasted in the upper left-hand corner.	(37). Good. I have included most of this information.
(38). Page 23: Maybe clearer as a footnote that the objective is 5 and values less than 5 do not meet the objective.	(38). I have incorporated the revision (but not as a footnote).
(39). Page 24:.......at the international border between Minnesota and Manitoba on the Red River …	(39). I have made this revision.
(40). Page 24: Not specifically related to SK since the Red River at Emerson is not affected by SK.	(40). I have added a statement to place the importance of the objective-setting exercise at this site into context with Saskatchewan.
(41). Page 25: ... particles or sediment-adsorbed …	(41). Good. I have made this revision.

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(42). Page 26: It would be helpful to develop this thought further. Given nutrient concentration changes are typically substantively less variable than flow, what are the situations where downstream concentrations are affected ... Especially in prairie systems where there may not be a stream with low concentrations? i.e. given the similar concentration in a stream with and without drainage ... what are the effects and what are the impacts with a bit more flow (load)?	(42). This is a good comment but one that I prefer not to address at this stage. I think overall, the thought that Dr. Davies suggested be expanded is addressed in later sections of the report and specifically, as it relates to wetland drainage.
(43). Page 27: Is this universal? As I recall studies looking at wetland nutrient retention have found some retain, some are approx neutral and some are sources of P (re: the latter, at least for periods of time).	(43). Dr. Davies comment is correct and his point is addressed, to a large degree at least, in later parts of the same paragraph. I think it is clear in the paragraph that there is considerable variability in nutrient retention and loss within intact and drained wetlands.
(44). Page 28: These are high concentrations (relative to what we typically measure in surface flows). Outside first flush events NO3 is often very low in SK streams. I wonder if there are differences b/w MB and SK?	(44). Correct, these are very high concentrations but they are in tile drainage flows and not in surface flows. Nitrate or nitrite concentrations in surface flows in Manitoba or Saskatchewan would be much lower. I have made a small edit to clarify.
(45). Page 29: What might this infer?	(45). Westbrook *et al.* (2011) thought that wooded areas tended to be at the top of the regional groundwater flow path where salts might be lower in concentration relative to cropped or grassed areas, so I have added this information to the report.
(46). Page 29: For permanent wetlands, could this also include increases in nutrients during winter under-ice hypoxic/anoxic conditions?	(46). Good. Yes, I have added this as a potential contributing factor as well.
(47). Page 29: Do you think this occurs regardless of wetland location on the landscape or do you think that wooded areas and grassland areas differ in terms of their characteristics because they are less suitable for cropping i.e. the historic process of choosing areas for cropping has selected for areas with different characteristics (e.g. productive soils) leaving less productive wooded/grassland areas?	(47). Not sure - could occur because of all of the reasons identified by Dr. Davies. I have not made any additional revisions in this regard.
(48). Page 30: Suggestion ......study, including nutrients, were …	(48). Good. I have made this edit.

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(49).	Page 31: Assume this is read off the same axis as TP Net Export Coefficient?	(49).	Yes, that is correct.
(50).	Page 33: Would be helpful to include export coefficients for flow	(50).	I agree that including export coefficients for flow would be helpful. Unfortunately, export coefficients are not readily re-calculated from information provided in the Armstrong (2018) reference.
(51).	Page 34: So a bit different than what was reported by Armstrong in Smith Creek? Is Smith Creek unique in some respects? Is the take home message more that, with respect to concentration, is this a matter of inherent variability among watersheds (and potential need for site-specific assessments)?	(51).	I agree that the Souris River work resulted in findings a bit different than Smith Creek. Not certain about the reasons for this but there is variability between watersheds and a need for site-specific analysis. I don't think I can make too many revisions in this section to reflect these points, but I do think they ultimately come through with the full read and overall message of this water quality analysis.
(52).	Page 34: Image from Table 11 above. Should these values in Table 11 equal those in the figure 4 (left-hand side)? Numbers seem slightly different, but it wasn't clear why.	(52).	Agreed. I have revised the graphs - not sure what went wrong with the original.
(53).	Page 35: Of course a challenge with this type of assessment is that flows change among years (as noted by Holly Annand in her work + many others). In low runoff years, there would be marginal increases; as I understand in very high runoff years there would also be marginal increases. So as I understand it's the in-between years where most effect is expected. Is it possible to work in the variability among years as noted in the hydrology work to match more closely to those findings and/or include discussion of this variability among years?	(53).	This is also a good point. I could work in the intra-annual variability but this would require considerable additional work for which time is too limited. Perhaps, this could be set up some time as a separate "case study".
(54).	Page 35: average? median?	(54).	Good. I think this should be "average", so I have made the edit.

Comments from Reviewer	Response from Dwight Williamson
(55). Page 35: Previous sentence suggest 162 t/yr increase over a 'background' of 239 t/yr. So a 1.7x increase in load. It would be interesting to test/explore this a bit further to know the load over the larger Souris Basin and see if a reduction of 0.6x makes sense relative to observations. Also pondering about effect of variation in flow among years and how that is captured in this work (e.g. dry years, median years, high flow years).	(55). Good comments. Inter-annual variability is captured reasonably well in Badiou's work in the Souris River, which was done for the years 2015, 2016, and 2017, but in the present water quality analysis for wetland drainage and retention for Saskatchewan, inter-annual variation is not very well captured (as identified in the "Uncertainties and Limitations" section. Because of time limitations, it is not possible to test a reduction of 0.6 times.
(56). Page 35: Pondering about relevance re: southern ON? How does this rate reconcile with export coefficients from Table 11?	(56). Good. The Page *et al.* 2020 study is interesting but may not be too relevant to the prairie pothole landscape. I have removed the description to the Page *et al.* 2020 study.
(57). Page 35: Presumably lower outflow?	(57). This comment also refers to the Page study which I have removed from the report.
(58). Page 35: Presumably greater outflow + higher concentrations in the wetlands during spring?	(58). This comment also refers to the Page study which I have removed from the report.
(59). Page 36: From my perspective, very much a scoping exercise so that needs to be considered when evaluating. One of the particular challenges with the data used on these streams is they were from an atypically wet period, so the measured loads are greater than normal. Had this study been done, for example from 2018-2020, the measured loads would have been a fraction of what was reported from that study (ratios would have ended up being <1).	(59). Good background information. I don't think additional revisions are necessary to make to the report since it is mentioned that it is a scoping-level study.
(60). Page 36: Atypically high flow years meaning flow would have included that from outside the effective drainage area; this variability in flow does complicate the story. This doesn't necessarily change the take home message you note below (re potential role of wetlands) but it does look at max potential effect so should be viewed from that perspective.	(60). Good background information. I don't think additional revisions are necessary since I already mention in my report that the Liu *et al.* (2022) is a scoping-level study.

Comments from Reviewer	**Response from Dwight Williamson**
(61). Page 41: Is it that the wetlands themselves intrinsically have different nutrient removal capacities due to their position (as seems to be suggested here .. or at least how I read it)? or is it the effect of wetlands on water quality at the outlet of a drainage basin differs due to their position (for any number of reasons)?	(61). Melles *et al.* (2010) concluded that it was the wetland's position within the watershed, so that is the conclusion I cited. Dr. Davies raises excellent points and there may be other reasons for the differing levels of reduction beyond position. Nevertheless, this was the conclusion of the authors of the publication, so I will leave the text unchanged. Nevertheless, the point is good.
(62). Page 42: I might not be interpreting/calculating correctly but this volume of water seems low: 0.0024 mm = 0.0000024 m over 10 000 m2 (=24 L). Correspondingly then the ave. concentration of, for example TP, would be relatively high (1.75 mg/L) (assuming I'm interpreting/calculating correctly). Perhaps given broad nature of analysis with various unknowns this is just a consequence of known/larger errors?	(62). I have re-checked the Yang *et al.* (2012) publication and my calculations, and everything seems to be correct as written. It is correct that the average TP would be relatively high.
(63). Page 43: Would be useful to include the additional flow as per Table 18 (annually 24 L per ha)	(63). I have made revisions to this section - the original sentences were unclear and potentially confusing.
(64). Page 48: re: load. Yes, this makes sense.	(64). Good. No additional revisions required.
(65). Page 48: To ensure clarity, note that this means behaviour similar to chemostats where the production of solutes is nearly proportional to water fluxes	(65). I have added this clarification.
(66). Page 48: What is the explicit implication of this as it relates to concentration of nutrients? Does this not suggest that concentration is relatively invariant? The follow up question being, what are ecological implications of higher or lower flows if concentrations remain similar?	(66). I have added considerable additional text to assist with interpreting the significance of chemostasis in a new section titled "<u>Chemostasis and Nutrient Loading Versus Nutrient Concentration</u>". Also, I set out four generalized patterns of nutrient losses centred around chemostasis, drawing on studies showing when it occurs and when it does not appear to play a role.
(67). Page 49: Apologies, I missed which point was first.	(67). Okay, the first point is a couple of pages earlier. I have made some edits to move the points closer together and to clarify which approach was selected.
(68). Page 49: Makes sense to look at longer term and average out some of the small scale variation.	(68). Good. No additional revisions required.

Comments from Reviewer	Response from Dwight Williamson
(69). Page 50: increase?	(69). Correct. I have made this revision.
(70). Page 53: Diazotrophic cyanophytes	(70). Good. I have made this revision.
(71). Page 53: Still need sufficient P, so there are more factors than N:P ratios. e.g. the proportion of cyanobacteria increases with P e.g. see Watson et al. L&O 1997 42:487-495. Also proliferation of non-fixing cyanobacteria can be an issue too (e.g. Microcystis). All that said though, the prairies have sufficient background nutrient levels to support large cyanobacterial numbers, whether diazotrophic or not.	(71). Good. I have made revisions and added reference to Sue Watson's paper.
(72). Page 53: Depends on a number of factors e.g. how much P of the TP is available plus various biogeochem processes. Also, depending on the system how often are nutrients are limiting per se (sensu stricto re Liebig) vs other factors.	(72). I agree there is considerable uncertainty with many factors coming into play. I use the term "potentially" to try capture this complexity without going into complicated detail.
(73). Page 55: Might be worth looking at the water quality report that reported the water quality data used by Liu et al. It can be found at: https://www.wsask.ca/about/publications/the-quappelle-nutrient-mass-balance-report. A previous comment noted the high flow conditions during the collection of these data. The long-term station at Welby shows this. 2011 was a high flow year and would have 'filled' the basin, 2012 had lower flows than average (albeit much lower than 2011) then the study years 2013, 2014 and 2015 (the last points on the figure pasted onto the bottom left-hand side of the page) were all high flow years.	(73). Good. I have made some edits to reflect the point that the Liu *et al.* (2022) study was conducted during high flow years.
(74). Page 55: Liu et al were really scoping maximum potential to assess which of the evaluated BMPs were most promising and what the maximum potential benefit might be (again, scoping).	(74). Good. I have acknowledged this in the revised report.
(75). Page 56: Similar to Liu - a scoping exercise.	(75). Good. I have acknowledged this in the revised report.

Comments from Reviewer	Response from Dwight Williamson
(76). Page 56: For flow-weighted concentrations to double the export coefficients must have doubled. It wasn't clear to me why. Perhaps it could be explained in a bit more detail	(76). This was discussed in the section describing the Armstrong (2018) study and to some degree, in the presentation of the case study on the Assiniboine River at Kamsack. Probably not too much more that I can provide here since this was intended just to compare my findings with those of others, where the results can be compared.
(77). Page 59: Since the Carrot River is tributary to the SK River in MB perhaps worth noting 05KB is within the Carrot R. watershed. Would be useful to include how/or where areas were calculated or derived. I just did a quick check of the 05KB polygon on ArcGIS and got a different number than that listed in Table 200 (Appendix summary of 05KB) i.e. 5512 (Arc) vs 4794 (Table 200).	(77). Good, I have added this edit. In terms of the difference between ArcView and this analysis, the differences arise because not all of the area within the sub-subbasins have been included in the wetland inventory. This point is made clear in the discussion surrounding the Assiniboine River case study.
(78). Page 60: Was this summarized within the context of this report or was it a separate analysis (separate report)?	(78). It is an overview table summarized within this report and not a separate analysis found elsewhere.
(79). Page 61: Is this an average or how does that work in an landscape with variable return flows/variable contributing areas? In high and low flow years is the load/year similar to status quo, with 'average' flow years having the greatest affect on loads?	(79). It is a long-term average. I discuss this point in the section on "Scaling from Watersheds to Larger Geographic Regions" and again in the section describing "Uncertainties and Limitations".
(80). Page 61: Is it possible to also put this in context of current loads (i.e. % change)?	(80). It is possible, but I have already described the 10 x increase in loads. Not sure that I need to go further since the change in loading can also be seen in Table 25 and again in Figure 7.
(81). Page 61: 10,818.79 + 2772.94 = 13,591.73	(81). Correct. This is what is reflected in Table 23.
(82). Page 62: Similar to a previous comment, it would be useful to include the changes in annual discharge (dam3/yr or similar units).	(82). I agree. Unfortunately, we were not able to integrate this work tightly with Holly Annand's hydrological analysis. While I made some flow predictions for two of the case studies, I had to do these using a different method than Holly's. If we are able to more fully integrate the work, then the flow values can be pulled into this and other tables. As well, background loading information was used only for the five case studies and not for the broader basin-wide analysis.

Comments from Reviewer	Response from Dwight Williamson
(83). Page 63: Figures are small. Perhaps re-arrange so there are only two across. It would also be helpful to add a change in flow figure (so four panels in total).	(83). Good suggestion, but I cannot add flow for the same reason mentioned in response to comment 82. I have reformatted the figures to make them larger.
(84). Page 63: ... L. Winnipegosis basin in Saskatchewan is relatively small ... ?	(84). Good. I have made this edit.
(85). Page 63: Is the combined total area for these two sub-subasins not greater than 4731 km2?	(85). Probably, but this is the only area covered by the wetland inventory, so this is all that I have from the inventory analysis.
(86). Page 64: Very difficult to read. Suggest enlarging, perhaps having more than one row as suggested above. Also, while there are advantages to keeping the same scale, suggest considering altering scale.	(86). I have reformatted the figures to make them larger. I do prefer to keep the figures with the same scale, but I understand Dr. Davies' point.
(87). Page 64: How does increase compare to current loads? or at least, what is the % increase in flow?	(87). This is a good comment but time limitations in the current project precluded obtaining or calculating current loads for this basin. Current loads have been determined from available data or published literature for the five case studies, but this was not done individually for the nine major basins. Unfortunately, we were not able to integrate this work tightly with Holly Annand's hydrological analysis. While I made some flow predictions for two of the case studies, I had to do these using a different method than Holly's. If we are able to more fully integrate the work, then the flow values can be added to this analysis.
(88). Page 65: As above, recommend increasing figure size.	(88). I have reformatted the figures to make them larger.
(89). Page 65: ... no historic records of water leaving …	(89). Good. I have made the suggested revision.

Comments from Reviewer	Response from Dwight Williamson
(90). Page 65: How does increase compare to current loads from the system? or at least, what is the % increase in flow?<	(90). This is a good comment but time limitations in the current project precluded obtaining or calculating current loads for this basin. Current loads have been determined from available data or published literature for the five case studies, but this was not done individually for the nine major basins. Unfortunately, we were not able to integrate this work tightly with Holly Annand's hydrological analysis. While I made some flow predictions for two of the case studies, I had to do these using a different method than Holly's. If we are able to more fully integrate the work, then the flow values can be added to this analysis.
(91). Page 68: Isn't this part of the Missouri?	(91). These were very small parts of sub-subbasins which were included in the original wetland inventory basin files provided by Saskatchewan Water Security Agency - I suspect they are present in the original basin data file because they may have been part of a few quarter sections at the basin edges.
(92). Page 68: ... of that lost from wetlands within the basin (?)	(92). Correct. I have made a revision to clarify.
(93). Page 68: These are outside the Souris. 05JF includes Regina, flows to the Qu'Appelle. 05JD flows to Old Wives.	(93). These were very small parts of sub-subbasins which were included in the original wetland inventory basin files provided by Saskatchewan Water Security Agency - I suspect they are present in the original basin data file because they may have been part of a few quarter sections at the basin edges.
(94). Page 69: Is this the best measure given different watersheds have different current levels of drained wetlands and different % wetlands that 'could' be drained. It would be useful to know the predicted increase in flow relative to measured flow to put this number in context (ideally, we would have loads to compare, but b/c we don't for most systems flow would be an informative proxy).	(94). This is a good comment. In terms of flow, as previously mentioned, we were not able to integrate this work tightly with Holly Annand's hydrological analysis. While I made some flow predictions for two of the case studies, I had to do these using a different method than Holly's. If we are able to more fully integrate the work, then the flow values can be added to this analysis. As well, background loading information was used only for the five case studies and not for the broader basin-wide analysis.

Comments from Reviewer	Response from Dwight Williamson
(95). Page 69: Because I have handy some SK River TP loading estimates from a number of years ago at the AB/SK border, thought I would compare. Lowest load year was 38 tonne/yr (2001). Highest load year was 3529 tonne\yr (2010). Ave 865/yr. In a low flow year there would be minimal to arguably no effective load originating from drained wetlands. In high flow year, it may not matter much if a larger portion of the basin is contributing \(that said ... most of the flow on the river arises in AB ... so there is that complication in assessing the SK river system). Ave flow year the 83.25 ~ 10% of load. In terms of effects on concentration ... SK also has its reservoirs. So there are a number of nuances in terms of predicting change.	(95). This is a good comment. I think the additional load identified in the current analysis seems reasonable in comparison to TP loads provided by Dr. Davies at the Alberta / Saskatchewan border during low, high, and average flow years.
(96). Page 69: 05HB ?	(96). No wetland inventory data were provided by Saskatchewan Water Security Agency for 05HB as part of the South Saskatchewan River Basin file. While 05HB is part of the South Saskatchewan River Basin, it is most likely that it was not covered by the wetland inventory.
(97). Page 70: Technically, ECCC monitors \(at the request of the PPWB\), different budget sources though.	(97). Good. I made this revision.
(98). Page 73: In which flow years would it have this effect (all flow years, low flow years, high flow years, median flow years)	(98). This effect would apply to all flow years, consistent with the limitations of the Wetland Area Fraction method which I used here to estimate flows.

Comments from Reviewer	Response from Dwight Williamson
(99). Page 73: This is more a hydrology clarification, but it is important for underpinning WQ. So the argument here is that if 60% of the wetlands are drained (where total historical wetland coverage comprises ~14% of the watershed are ... so drainage of 60% ~8.2% of the watershed area) will result in a 50% increase in flow?	(99). Yes, that is correct, at least as provided by the Wetland Area Fraction method used here to estimate flows. As mentioned in earlier comments, time limitations prevented a more complete integration with Dr. Holly Annand's hydrology study, so comparison with her information for these two combined sub-subbasins would be helpful. Dr. Annand would have to specifically run this analysis for these two sub-subbasins through her model, and this request has not been made. I have added a new note in the "Uncertainties and Limitations" section that in general, indicates that Holly predicts greater water yield for the Assiniboine River sub-subbasins than I found with the method I used, so my results are a bit conservative relative to Holly's. But yes, in terms of the information in the table, a 60 % loss of wetlands is predicted to result in a 50 % increase in flow.
(100). Page 76: Compared to flows from 2004-2018 (15 years) 2018 was a lower flow year (10 years in that period had greater flows). What are the implications of this return period for this analysis?	(100). Good comment. I have noted in my report that 2018 was a lower flow year and that the results would change or be different for other lower flow years or other higher flow years.
(101). Page 76: Note: For sediment expect 20% excursion rate (over the long term) b/c of how objective was derived. For TP and TN, expect 10% excursion rate over the long term.	(101). Good, I understand the implications of this. Thanks.
(102). Page 76: This may explain my earlier comment re watershed area differences.	(102). Yes, that is correct - this is the explanation.
(103). Page 77: Algae have some stoichiometric plasticity ... 26:1 is more in the 'suggestive of' rather than 'clearly'. Many other factors can proximately 'limit' algae.	(103). Agreed. This is a good comment. I have made revisions to this section.

Comments from Reviewer	Response from Dwight Williamson
(104). Page 77: I think it would be extremely insightful to calculate the loads; flow-weighted concentrations over the period of record at Kamsack. One can then undertake an evaluation of how concentration (flow-weighted\) changes under different flow periods. I presume that such an analysis will show an increase in annual flow-weighted concentration with flow. What I think would be extremely informative is to compare the slopes from measured historic data with that from this exercise (e.g. Figure 17). Do the two match well? If so, why? If the two differ significantly, why? Are assumptions of this exercise valid within a reasonably amount (e.g., export coefficients, etc)? This would provide a grounding of this exercise to real-world/measured data. One might reasonably expect deviations between the two but those deviations would serve as a useful basis for furthering the discussion a couple paragraphs below re: the 2018 Armstrong study.	(104). I agree, this would be a very worthwhile additional exercise to undertake and it would help to underpin the current work within the historical data-set for this site. However, time limitations prevent undertaking this work within the scope of the current project. Nevertheless, it is an excellent suggestion and if possible, it should be undertaken.
(105). Page 77: Presumably this statement assumes cyanobacteria must be diazotrophic. Many other considerations for dominance of cyanobacteria (including non N-fixers).	(105). Good. I have made revisions to this statement.
(106). Page 78: Ok, thanks for noting this ... I already made a comment related to this in the table below but good to see this in the text. Thanks.	(106). Good. No additional revisions required.
(107). Page 79: One can't compare annual flow-weighted concentrations to the PPWB water quality objective so question whether this should be included in the table. Greater loads typically occur during periods of greater flow, so the flow-weighted value is greater than an arithmetic average of just concentrations, which is clear in Table 31. Since the background objectives were based on a percentile approach of concentration (without consideration of flow or flow-weighting) one can't directly compare these values.	(107). Agree. Because of this, I got as close as possible in this analysis by then making comparisons to individual months - June and October. I identify this as a limitation but I think the analysis is good and it arrives at a fair comparison.

Comments from Reviewer	Response from Dwight Williamson
(108). Page 79: How do these modelling results relate to findings of Ehsanzadeh et al 2016 cited earlier? i.e. with respect to changes in flow observed in the Assiniboine?	(108). Good comment. Ehsanzadeh *et al.* (2016) could not find any statistical relationship between flow and wetland drainage in the Assiniboine River, so there is not a good comparison. In this regard, Ehsanzadeh *et al.* (2016) is a bit of an outlier from all of the other wetland-loss literature and impacts on flow. Ehsanzadeh *et al.* (2016) found that climate variables simply overwhelmed the analysis, so it is possible that the more subtle effects of wetland drainage were masked by significantly larger precipitation events.
(109). Page 80: or ... more stoichiometrically balanced	(109). Good. I have made this edit.
(110). Page 80: This might also pair well with a longer record of flow-weighted concentrations and assessing how they change with flow. More drainage = more flow, should correspond on average to greater flow-weighted concentrations. How do the relationships (PPWB data vs this exercise) compare? I suppose one could also do it for different months (not just annual) and see how those compare to this exercise described here.	(110). I agree that the same exercise could be run for other time-periods including a much longer period of record rather than just 2018. However, within the present scope of work, there is insufficient time to undertake this additional analysis. Nevertheless, it is a good suggestion and may be instructive to conduct.
(111). Page 83: The reservoir effect on nutrients is a good question. What is the concentration and load reduction from the reservoir i.e. does it lower concentrations and reduce concentration variation among years (different return periods\)? For existing data I suppose a quick check/starting point would be to compare flow-weighted concentrations above (Kamsack) and below (MB water quality monitoring site) the reservoir. This doesn't factor into the smaller question of how drainage effects nutrients at Kamsack but it is important for the larger question related to L.Wpg.	(111). This is a good point and it would be instructive to undertake. However, within the present scope of work, there is insufficient time to undertake this additional analysis.
(112). Page 189: Technically Lake Diefenbaker is a larger reservoir serving as a drinking water source (albeit communities directly accessing are relatively small).	(112). Good. I have made a small revision.

Comments from Reviewer	Response from Dwight Williamson
(113). Page 89: On average most water flowing into BP is from Diefenbaker. I'd have to check the numbers provided by our hydrologists but the number that comes to mind, as an average over the long term, is 80%. Some years almost all water. Some years (e.g. 2014) there was substantial inflows from the Qu'Appelle's own watershed (and it affected the water quality in the lake).	(113). Good. I have made this revision.
(114). Page 90: ... water has been transfered from the SSask to BP …	(114). Good. I have made this revision.
(115). Page 90: Approx. when pumping started.	(115). Good. I have made this revision.
(116). Page 92: The variable flows (return period ... vast majority of years local runoff makes up a small proportion of inflows as noted above) along with the flow management of 'better' water from Diefenbaker makes this system different. I think it would be appropriate to include some discussion of this.	(116). I think the existing text already describes the complicated water management regime on Buffalo Pound Lake. I agree that local inflow is relatively small, but in terms of wetland drainage, this proportion may change somewhat should additional drainage occur. Should local inflow increase, flows from Lake Diefenbaker may well be reduced, but this future response is unknown.
(117). Page 92: FYI. This study was continued, but only for the portion of the Qu'Appelle b/w Buffalo Pound outlet and Katepwa outlet (still in draft). I've pasted the system TP and TN load diagrams as a frame of reference for how much the flows and loads changed from the original 2013-2016 study.	(117). Okay, thanks. This is helpful information.
(118). Page 92: Note: Flows are highly managed on this system. If natural flows increase then diversions from L.Dief decrease in proportion as long as flows don't get too high. The system is managed to maintain in bank flows whenever possible. Thus increased flows due to drainage are not wholly additive in this system and should be accounted for. It would be best to discuss flow management with our hydrologists (e.g. Curtis Hallborg) to see how they would match with the increased flows associated with the drainage scenarios tested in this exercise.	(118). Similar to the above response, I agree that the system is highly managed and should local inflows increase with future wetland drainage, flows from Lake Diefenbaker may well be reduced in response. At this stage in this scoping-level water quality analysis of the potential impacts of future wetland drainage, I would prefer not to go too far in identifying how the system may be managed in the future.
(119). Page 92: Ok, thanks. Please ignore an earlier comment making reference to this study (data used for Lui et al 2022 and the Qu'Appelle work by Roste and Baulch).	(119). Thanks. No additional revisions required.

Comments from Reviewer	**Response from Dwight Williamson**
(120). Page 92: True	(120). Good. No additional revisions required.
(121). Page 93: I'm struggling with the concept of 'additional' loads given the atypical return period that would have greatly expanded the contributing area from a median year. For arguments sake let's say the whole watershed was contributing flows during this period. If that is the case then what additional area would have contributed to the loads via wetland drainage/increased hydrological connections? Another way of looking at it. What if the 2013-2016 study had occurred in a dry year and the loads were (for argument sake) an order of magnitude lower (e.g. TP outflow load from BP in the first study was 23.3 and in the second study 3.56 tonne/yr). How different would figure 22 be? As I'm sure you can tell ... I'm struggling with loads under variable return periods (here and previous comments) - both in terms of understanding increased loads but also in terms of how the data, collected during a period of high flow, affects interpretation of the Qu'Appelle system. Presumably a low return period year would have minor increases in load with drainage works in place since most of the watershed wouldn't contribute. Presumably a high return period year would have minor increases in load (relative to undrained \since most of the watershed would have contributed anyway ... I believe that was the message I heard in Holly's presentation (and elsewhere generally). If any clarity can be provided from that perspective I think it would be helpful to the report.	(121). I agree - there will be significant variability from one year to the next depending upon precipitation so Figure 22 would be different in a dry weather cycle relative to a wet precipitation cycle. I have identified this as one of the limitations of the present work in that year-to-year variability cannot be well captured since it was not well captured in the original modelling studies. However, there will be nutrient and sediment contributions arising from wetland drainage in both dry years and wet years relative to the undrained landscape resulting in a considerable range in the level of contribution for each. I also set some of this out in the literature review in the early sections of the report. I have added the following text in the "Review of Wetland Science, Background, Surface Water" section: "*While water is the solvent moving materials off the landscape, much less water moves off the land during a dry weather cycle, but following wetland drainage, even though the volume of water moving off the landscape may be less, it will be draining cropland which will lose nutrients and sediment at a greater rate relative to the original wetlands (Badiou et al. 2018a). Moreover, during a wet precipitation cycle, most wetlands are full and would spill in any case, so the volume of water moving off the landscape may not change significantly with or without wetland drainage, but again, the materials being transported in that water will change since cropped land is now being drained and will lose nutrients and sediment at a greater rate relative to the original wetlands.*"
(122). Page 94: WSA has in-lake TP and TN values for Buffalo Pound from multiple locations along the lake (2015 to present). If interested, please request. On average TP just d/s of Hwy#2 is ~0.082 mg/L and TN is 0.54 mg/L. Further downstream near the water treatment plant intake average TP ~0.057 mg/L and TN is 0.74 mg/L.	(122). Thank you. No additional revisions required.

Comments from Reviewer	**Response from Dwight Williamson**
(123). Page 94: In-lake concentrations also include other loading source esp. internal loading. So that becomes more challenging to predict.	(123). I agree. This is one of the reasons that I suggested an in-lake model such as QUAL2E would be required to more accurately predict changes in Buffalo Pound Lake arising from wetland drainage.
(124). Page 94: Previously BP was a low lying 'lake' in a productive landscape. Diefenbaker was a river. Transfer of L.Dief water (with lower nutrients) has arguably helped as well.	(124). Okay. This is a good point.
(125). Page 94: Also Hall et al. report dissolved nutrients (all of Leavitt nutrient concentrations are dissolved).	(125). Okay, thank you. No additional revisions required.
(126). Page 95: In lake ave 2015-2021 values put this a bit higher near Hwy#2 (downstream of the first 'basin') and higher ~30 nearer the d/s end of the lake, which speaks to the importance of inlake processes regarding TN:TP in addition to inflows.	(126). I agree. Thank you. No additional revisions required.
(127). Page 95: Arguably ... where all lakes changed the same way through time (without a reference system that doesn't change) it makes it more challenging to conclude the cause.	(127). Good. I agree it is challenging to identify the cause without a reference system that does not change.
(128). Page 95: Diazotrophic species. Other factors are also very important for determining algal spp. composition. Plus of course, non-fixing cyanos can also be important.	(128). I have revised this section slightly and no longer make reference to cyanophytes.
(129). Page 95: Julie Terry has published papers using CE-QUAL-W2 for Buffalo Pound under different flow scenarios e.g. Terry et al. 2022. Buffalo Pound Lake - Modelling water resource management scenarios of a large multi-purpose Prairie reservoir.	(129). Very good. I have made reference to this paper and how the model, with modifications, could also serve to better understand the implications of wetland drainage on the lake.
(130). Page 100: This started in 1977 with the tertiary treatment plant. Nutrient reduction from the 1970's and early 1980's was evident when the lakes were revisited in the 2000's (Pasqua levels almost halved - matched predictions made in the late 1970's). Certainly more recent upgrades are also expected to improve conditions in the lakes.	(130). Good. I have made a slight revision to the text.
(131). Page 100: Notional target.	(131). Okay. I have inserted this qualifier.

Comments from Reviewer	Response from Dwight Williamson
(132). Page 100: Depends how one defines limitation. Yup, it has 'lots' of P but it also has in proportion 'lots' of N. FYI. 2015-2021, on average, TN:TP for Pasqua/Echo/Katepwa was in the 24-30 range (molar).	(132). Okay. I have made revisions to this section.
(133). Page 100: There is also internal lake loading - this was particularly evident in 2021 (see pasted figure to the left) for Pasqua Lake. TP in the spring was relatively low into July then increased during the summer. This occurred after most inflow flows had subsided (albeit, 2021 was a very low flow year so didn't get the same inflow load as other years).	(133). Good. I have made a note of this in the discussion.
(134). Page 100: Maybe not inevitably, depends on the relative proportion in relation to other sources and in-lake processes. I think it very much depends on return period. Low flow years where more water originates from Lake Diefenbaker vs. some of the high runoff years (2011, 2014) where a high proportion of the basin is contributing is important.	(134). Okay, good comment. I have adjusted the text to change "inevitable" to "likely".
(135). Page 100: As noted previously.	(135). Good. I made changes throughout to reflect this.
(136). Page 104: Given the SK focus of this work on effects to nutrient loads/concentrations with Saskatchewan the breakdown should specifically separate out the Qu'Appelle and Assiniboine systems (i.e. from the SK system which you do below).	(136). I would prefer to leave this unchanged since it sets the stage and provides the overall Lake Winnipeg context against which contributions from wetland drainage in Saskatchewan, the focus of this project, can be compared. I do another level of analysis with just the Saskatchewan portion later in this case study.
(137). Page 105: Perhaps clearer if stated as: SK portion of L.Wpg watershed.	(137). Good. I have revised the caption.

Comments from Reviewer	Response from Dwight Williamson
(138). Page 109: Useful. Although how sequestration amounts change with flows/loads is an important question. How was this sequestration accounted for in the calculations? What about sequestration in other systems on the SK river?	(138). Sequestration was accounted for by applying a simple percentage as identified by Donald *et al.* 2015. I did not include sequestration in other systems - only Cedar Lake. This would be a complicated analysis since wetland drainage and flows would need to be routed from one lake or reservoir to another as the water moved downstream from Alberta from one system to another, then sequestration accounted for in each lake or reservoir. In the current project, I did not have sufficient time to undertake this analysis.

Comments from Reviewer	Response from Dwight Williamson
(139). Page i: Please be mindful of the number of significant digits.	(139). This is a good comment. However, because many of the values reported throughout the report arise from multi-layered calculations, often with initial input values being modelled data arising also from calculation, it is not feasible to report significant figures correctly. Rather, figures were reported with a consistent number of decimal places throughout. A footnote was added to the report in this regard and it was stated that this may suggest a higher level of accuracy than almost certainly exists.
(140). Page 1: Is there any particular reason for excluding the western most zone (about 150 km swath) of Saskatchewan? This seems to be an agricultural region.	(140). The analysis could only be performed in areas for which wetland inventory data were available. In areas such as parts of western Saskatchewan (and elsewhere as well), wetland inventory data were not available or were not sufficiently processed for use in this project.

Comments from Reviewer	Response from Dwight Williamson
(141). Page 1: How was this determined for individual wetlands (that is, how were intact, partly drained, completely drained, partly filled, constructed, and farmed but not drained determined for each wetland)? It may not be a straight-forward exercise. A brief explanation by a sentence or two will be useful.	(141). The methods are set out in the reference that is provided - Canadian Wetland Inventory Technical Committee 2016. The Reviewer is correct that it is probably not astraight-forward process but one that likely involves considerable best professional judgement. Because the methods are set out in the identified reference, it is felt that providing additional detail in the water quality report is not warranted.
(142). Page 2: Why is this region excluded?	(142). The analysis could only be performed in areas for which wetland inventory data were available. In areas such as parts of western Saskatchewan (and elsewhere as well), wetland inventory data were not available or were not sufficiently processed for use in this project.
(143). Page 4: This contradicts with Part 1 of the report (page 22), which says smaller wetlands are drained before any other size class.	(143). It is agreed that this is a bit confusing. The information on p. 22 describes the practice of drainage as it actually occurs on the landscape whereas the information on p. 4 sets out the methods to simulate “drainage” in each of the modelling scenarios. I have added a few additional phrases to try to make clear that in this context, we are simply trying to describe our “drainage” choices in the simulations or scenarios.
(144). Page 4: See my comment above.	(144). It is agreed that this is a bit confusing. The information on p. 22 describes the practice of drainage as it actually occurs on the landscape whereas the information on p. 4 sets out the methods to simulate “drainage” in each of the modelling scenarios. I have added a few additional phrases to try to make clear that in this context, we are simply trying to describe our “drainage” choices in the simulations or scenarios.
(145). Page 14: This figure is a bit confusing without further explanation. Please see my comment on the figure caption.	(145). Agreed. I have added additional information in the figure caption as the Revieweri recommended.

Comments from Reviewer	Response from Dwight Williamson
(146). Page 15: These mass balance numbers are a bit confusing. First, what does red and blue indicate? Does 'deposition' indicate atmospheric deposition? What does 'fixation' indicate? If blue represents the output, where does the rest go? These need to be explained in the figure caption.	(146). Agreed - I have added additional information to the figure caption as recommended by the Revieweri.
(147). Page 57: Were there any attempts to verify model-predicted coefficients using field data? Without rigorous validation, recommendations based on modelling alone may mislead management decisions or policy making. This point should be addressed somewhere in the report.	(147). I agree with the reviewer's comment in general that without rigorous validation, modelling results may mislead management decisions. Near the end of the section titled "Scaling from Watersheds to Larger Geographic Regions", I compared my findings to two other modelling studies and to one field study but no side-by-side modelling / field study validation could be done given the project's time-line.
(148). Page 59: Significant digits.	(148). This is a good comment. However, because many of the values reported throughout the report arise from multi-layered calculations, often with initial input values being modelled data arising also from calculation, it is not feasible to report significant figures correctly. Rather, figures were reported with a consistent number of decimal places throughout. A footnote was added to the report in this regard and it was stated that this may suggest a higher level of accuracy than almost certainly exists.
(149). Page 59: Please be mindful of the number of significant digits.	(149). This is a good comment. However, because many of the values reported throughout the report arise from multi-level calculations, often with initial input values being modelled data arising also from calculation, it is not feasible to report significant figures correctly. Rather, figures were reported with a consistent number of decimal places throughout. A footnote was added to the report in this regard and it was stated that this may suggest a higher level of accuracy than almost certainly exists.

<u>Comments from Reviewer</u>	**<u>Response from Dwight Williamson</u>**
(150). In addition to providing comments in the studies provided, I have also provided a brief narrative summary for each study. They are found further in this document.	(150). Good. No additional revisions required.
(151). An important general question that came to mind in the course of this review is, "Who is the audience?". If the audience will extend beyond highly technical people schooled in the areas of review, many readers will be challenged to wade through the material. If these reports are to be read beyond a narrow group of experts, more thought needs to be given to definitions of terms used, simpler and higher-level summaries of key results and the analytical decisions behind them i.e. why one approach was used over another and how those decisions affect the resulting analysis. In most cases that information is available, but it is often buried in technical discussion and will not be easily discerned by most readers, and I would put myself in that category in a number of sections of the reports I read. I would expect that policy analysts and other decision-makers will welcome more clarity here.	(151). As per response to an earlier comment from Professor Belcher, this is a good comment, but no change is thought necessary. The reports were written for a scientific/technical audience since they were to be subjected to peer review, but the key messages were intended for government policy-makers in Saskatchewan. In the water quality report, a plain language summary was provided to bring forward, in a coherent manner, all of the key findings and the key messages. It is thought that this should suffice without additional revisions to the report.
(152). To reinforce my above point with a concrete recommendation, put more work into the summaries and recommendations, ensuring that critical analytical approaches are highlighted along with the uncertainties that go with those decisions. As for the results themselves, ensure that they are communicated clearly, along with the uncertainties and biases inherent in the numbers.	(152). Agree. As per response to an earlier comment from Professor Belcher, information has been qualified throughout the water quality report in terms of its uncertainty and a section had already been previously included outlining "<u>Uncertainties and Limitations</u>". Additional information has been added to the "<u>Uncertainties and Limitations</u>" section. It is thought that no additional revisions are necessary.

Comments from Reviewer	Response from Dwight Williamson
(153). In the three studies that used results from the Saskatchewan Wetland Inventory, I made specific comments. An apparent underestimation of historic drainage – at least to this reviewer, but was also highlighted in the hydrology study – is perhaps the most significant shortfall in the entire set of analyses. If the WLI significantly underestimates historic drainage, it affects this entire exercise.	(153). Agreed. The "Uncertainties and Limitations" section has been substantially expanded to include reference to Dr. Holly's observation in her hydrology study regarding under-estimation of the current level of drainage and the implications of this to the water quality study have been described in detail. As noted in the expanded "Uncertainties and Limitations" section, the exercise of simulating impacts arising from drainage in 10 % decrements began at the historical level of wetland coverage, not at the current level. Consequently, while extremely important, the overall results may not be significantly affected assuming that the historical level of wetland coverage has been correctly estimated.
(154). Lastly, some thought should be given as to how these reports integrate, not only from a design and layout standpoint, but in terms of a synthesis of relevant information from each into a coherent summary. This was not in the terms of reference for anyone at this time, but it will be a key consideration going forward.	(154). My understanding is that the Project Manager, Dr. Merv Fingas, may be pulling key points from each of the three reports to integrate, at least at a high level.
(155). Generally speaking, and with the exception of the water quality analysis, the reviews downplayed interprovincial considerations, other than those required by treaty (Souris River) or agreement (Prairie Provinces Water Board). The water quality paper was explicit is assessing issues of concern in Manitoba basins. It would have useful to see more explicit discussion along these lines in the hydrology paper as well.	(155). Good, no further response necessary for the water quality report.
(156). The consultant is to be commended for the comprehensive approach to a complex subject. The review of relevant studies is thorough, as are his efforts to rationalize results between studies. This is not an easy task, given the range of results reported from watersheds that have significant differences between them.	(156). Good, no further response is necessary.

Comments from Reviewer	Response from Dwight Williamson
(157). Overall, the structure of the report works well. It establishes the Saskatchewan context – landscape, issues and study scope – takes a deep dive into the relevant literature and then takes a structured approach to estimating impacts of wetland loss on key water quality parameters. There is a tremendous amount of useful information here	(157). Good. No additional revisions required.
(158). Further the summary and brief discussion sections well written and effectively capture an extensive and complex analysis. They are consistent with the overall analysis, and they communicate well. Given that most readers will not get beyond these sections, this is an important accomplishment.	(158). Good. No additional revisions required.
(159). This estimate historic wetland losses mentioned in the summary (30%) seems to me to be quite low. It has been generally accepted that wetland loss across the prairies has been in the 40-70% range (e.g. Federal State of the Environment Report, 1991). Recent estimates of annual prairie wetland loss rates are 0.5 to 1%	(159). Agreed. The "Uncertainties and Limitations" section has been substantially expanded to include reference to Dr. Holly's observation in her hydrology study regarding under-estimation of the current level of drainage and the implications of this to the water quality study have been described in detail. As noted in the expanded "Uncertainties and Limitations" section, the exercise of simulating impacts arising from drainage in 10 % decrements began at the historical level of wetland coverage, not at the current level. Consequently, while extremely important, the overall results may not be significantly affected assuming that the historical level of wetland coverage has been correctly estimated.
(160). Re: the fundamental consideration of scaling from watersheds to basins, the author's decision to apply coefficients for N and P expressed as kg/ha/year for drained or restored wetlands (page 76) across the entire study area is reasonable. The scale of the study, and limited availability of data across watersheds, would seem to preclude other approaches.	(160). Good, no further response is necessary.

Comments from Reviewer -	**Response from Dwight Williamson**
(161). In the examples where alternate approaches presented, it would appear that this approach used in this study biases to providing estimates that are somewhat conservative. If that is an accurate assessment, could such a statement be made more explicitly?	(161). In comparison to the two other available modelling studies for the Qu'Appelle and Moosomin Lake, the estimated losses from the current study are conservative, but are fairly close to the field study of Armstrong (2018) for Smith Creek. As stated in the report in terms of the modelling studies, it is not certain which is correct since all use numerous assumptions, many of which are second-order. I noted that it could be simply that the Qu'Appelle and Moosomin landscapes are high-yielding areas whereas I used average loss coefficients for my work in the water quality report, which is thought appropriate when considering all landscapes within the wetland inventoried region of Saskatchewan. In addition, I have added text arising from Dr. Davies comments that the Qu'Appelle study was done under high flow circumstances, so may have yielded greater nutrients relative to my analysis, which were assumed to be for long-term, average conditions. Further field validation would be required to determine which approach yields the most accurate predictions, but this is beyond the scope of the current work. It is thought that additional qualifying statements as per Reviewer's suggestion are not necessary beyond what is presently identified in the expanded section on Uncertainties and Limitations.

Comments from Reviewer -	**Response from Dwight Williamson**
(162). This reviewer is not familiar with the details or methodology in the Saskatchewan Wetland Inventory but, compared to other studies of wetland loss in the great plains region, the WLI-derived estimates appear to be low – in some cases where agricultural development is considerable, estimates appear to be very low. In some of the more specific studies discussed in this paper, estimates of drainage by others (e.g. Badiou 2018b as well as data from Smith Creek) are significantly higher than comparable WLI data.	(162). Agreed. The "Uncertainties and Limitations" section has been substantially expanded to include reference to Dr. Holly's observation in her hydrology study regarding under-estimation of the current level of drainage and the implications of this to the water quality study have been described in detail. As noted in the expanded "Uncertainties and Limitations" section, the exercise of simulating impacts arising from drainage in 10 % decrements began at the historical level of wetland coverage, not at the current level. Consequently, while extremely important, the overall results may not be significantly affected assuming that the historical level of wetland coverage has been correctly estimated.
(163). What appears to be consistent under-reporting of historic drainage in the Saskatchewan WLI is the most important limitation to this analysis, in my view. That said, I understand that the consultant worked with the data that was provided to him.	(163). Good. The "Uncertainties and Limitations" section has been substantially expanded to include reference to Dr. Holly's observation in her hydrology study regarding under-estimation of the current level of drainage and the implications of this to the water quality study have been described in detail. As noted in the expanded "Uncertainties and Limitations" section, the exercise of simulating impacts arising from drainage in 10 % decrements began at the historical level of wetland coverage, not at the current level. Consequently, while extremely important, the overall results may not be significantly affected assuming that the historical level of wetland coverage has been correctly estimated.
(164). Given that the consultant focused on the impacts of drainage going forward, perhaps this is not a critical consideration, but if the WLI systematically underestimates historic wetland loss, as I believe it does, it would significantly underestimate the impacts of drainage on current TP, TN and TSS loads.	(164). Agreed. As per response to previous similar comments, the "Uncertainties and Limitations" section was expanded and it was noted that the exercise of simulating impacts arising from drainage in 10 % decrements began at the historical level of wetland coverage, not at the current level. Consequently, while extremely important, the overall results may not be significantly affected assuming that the historical level of wetland coverage has been correctly estimated.

Comments from Reviewer -	**Response from Dwight Williamson**
(165). The detailed estimates of the impacts of additional drainage on specific watersheds of interest in Saskatchewan is useful. It would be more telling if, as part of the discussion of each scenario, more explicit information concerning the state of water quality was provided: degree of impairment; frequency of algae blooms; challenges to the use of the water by communities of interest, etc. The Moosomin Lake example states that any additional loading will exacerbate existing water quality issues. This is useful information in the context of the analysis.	(165). This is a good suggestion but the implications of the identified additional contributions from drainage could only be reasonably included for the five case studies. Implications on a watershed scale could only be broadly identified.
(166). The case for wetland restoration and conserving existing wetlands as being the most effective agricultural landscape BMPs re: management of nutrients has been effectively made in this review	(166). Good comment. No additional revisions required.
(167). Page i: This estimate would be seen as quite low. It has been generally accepted that wetland loss across the prairies has been in the 40-70% range. Recent estimates of prairie wetland loss are 0.5 to 1% annually.	(167). Agreed. In my existing "Summary", I do identify the higher estimates of wetland drainage observed in other studies. I have also made some minor additions to the summary to reference the higher levels of drainage given a test re-calculation and the "Uncertainties and Limitations" section has been expanded in this regard as well.
(168). Page ii: The word "nuisance" diminishes the outcome. I think the better word is "significant".	(168). Agreed. I have made the suggested revision.
(169). Page ii: I think "will" is the appropriate word here.	(169). Agreed. I have made the suggested revision.
(170). Page iv: suggest inserting " ...significant anthropogenic contributor. "	(170). Agreed. I have made the suggested revision.
(171). Page v: Overall, the summary is well done.	(171). Good. No additional response is required.
(172). Page 6: Agree with DW that the question of the impact of tile drainage remains open in the Canadian prairie context.	(172). Good. No additional response is required.

Comments from Reviewer -	Response from Dwight Williamson
(173). Page 7: Is there any relationship between eutrophication and proliferation of pathogens, even as a co-factor? Perhaps the author can comment, assuming this question is considered in scope.	(173). I am aware of some literature suggesting that reproduction of pathogens such as *E. coli* may occur in natural waters should pollution levels reach the extent of conditions typically observed in the gastrointestinal tract. This, in my view, would be extremely rare. Because this would be extremely uncommon, I would prefer not to make reference to the possibility in this report. Consequently, no further revision in this regard is required.
(174). Page 9: Interesting historical information but, ultimately, distracting. The focus should be on anthropogenic impacts. The fact that the baseline condition of these watersheds is, ultimately irrelevant and distracting from the fundamental question of the water quality trajectory resulting from anthropogenic impacts to date and going forward.	(174). I agree that the focus of this report needs to be on anthropogenic contribution of nutrients and sediment from additional wetland drainage, which I believe, is the current focus. However, I think the historical context is also important. References to the early observations of trophic conditions are well-known, so this forms the background or ambient conditions against which any additional nutrient contribution needs to be assessed. The focus still remains on the additional water quality changes arising from wetland drainage, even if the receiving bodies of water were eutrophic in the pre-settlement period - they can still be made worse. Consequently, I would rather not make any revisions to this section.
(175). Page 21: This is an interesting contradiction, if I am reading the sentences accurately. It would appear that PPWB is saying that 90% of the CR basin wetlands have been drained, while the WLI analyses, used as the basis for this review pick up 25.9% If I have read the sentences properly it suggests a significant under-reporting of drainage, a fundamental consideration in this entire analysis.	(175). Overall, this is a correct reading of this section. The Ehsanzadeh *et al.* (2016) study does report about a 90 % loss of wetlands in the Carrot River watershed whereas the current work suggests a loss of about 25.9 %. There has been some suggestion from a Study Team member that the 90 % loss reported by Ehsanzadeh *et al.* (2016) is an over-estimation based upon methods used whereas, our study may have under-estimated losses, as identified above in responses to other comments. I don't think additional revision to the report is required.

Comments from Reviewer -	**Response from Dwight Williamson**
(176). Page 26: This conclusion seems to be contradicted by the work of Pomeroy's group on Smith Creek, where they have concluded that the combination climate change AND (emphasis mine) wetland drainage has created hydrological regime change in Smith Creek with a dramatic increase in streamflow volume and runoff generation efficiency and the development of unprecedented rainfall induced and summer flooding in the last 20 years (from a powerpoint prepared by the study principals).	(176). Yes, this is a correct reading of this paragraph. The work of Ehsanzadeh *et al.* (2016), in work done on the larger Assiniboine and Saskatchewan river basins, lead to a different conclusion than that of Pomeroy's group, specifically done in the smaller Smith Creek watershed. In this section of my report, I am simply laying out the key relevant scientific studies, many of which resulted in similar findings but some, resulted in contrary conclusions. I don't believe additional revisions are required.
(177). Page 49: given the size of the study area, the author's decision to take a more simplified approach is reasonable.	(177). Good. No additional revisions required.
(178). Page 53: This interpretation would suggest that the relationship between drainage blue-green bloom frequencies is non-linear. If algae blooms are a fundamental consideration, then the impacts of additional drainage could be approached as being non-linear as well.	(178). Yes, this is a good comment. It is agreed that the relationship between nutrient availability and stimulation of algal blooms may well be non-linear in many cases since many other factors can also be controlling. I do make a point of identifying the non-linear response between water yield and wetland restoration as identified by Pomeroy *et al.* (2014). I don't think additional revisions in this regard are required in this section.
(179). Page 56: The difference here is troubling. Differences in estimating wetland loss are significant, but nonetheless a major driver of predicted impacts from various drainage scenarios.	(179). Agreed. These differences have been recognized and addressed in the "Uncertainties and Limitations" section.
(180). Page 58: This is an important paragraph. It points to a significant number of assumptions that would affect estimates.	(180). Agreed. However, no further revisions are required.
(181). Page 58: At a larger scale, an underestimate of the impact from high yield landscapes will likely be more significant than underestimating impacts from low yield landscapes. Averaged out, it would downplay the impact, provincially.	(181). Good comment. I think, however, on a provincial scale, it will depend upon the area of high-yielding landscapes relative to low-yielding landscapes. If the areas are generally the same, the effect will not be downplayed. Nevertheless, it is a good comment but I don't think additional revisions are necessary.

Comments from Reviewer -	**Response from Dwight Williamson**
(182). Page 59: This reviewer is not familiar with the details or methodology in the Saskatchewan Wetland Inventory but, compared to WLI results from other studies in Manitoba and elsewhere in the great plains, these estimates appear to be low. In some of the more specific studies discussed, estimates of drainage by others (e.g. Badiou 2018b as well as data from Smith Creek) are significantly higher than equivalent WLI data. If this analysis is focused on the impacts of drainage going forward, perhaps this is not that important, but it would significantly underestimate the impacts of drainage on TP, TN and TSS currently.	(182). Agree. The "Uncertainties and Limitations" section has been substantially expanded to include reference to Dr. Holly's observation in her hydrology study regarding under-estimation of the current level of drainage and the implications of this to the water quality study have been described in detail. As noted in the expanded "Uncertainties and Limitations" section, the exercise of simulating impacts arising from drainage in 10 % decrements began at the historical level of wetland coverage, not at the current level. Consequently, while extremely important, the overall results may not be significantly affected assuming that the historical level of wetland coverage has been correctly estimated.
(183). Page 59: It would be useful, as part of the discussion of each scenario, to have included more explicit information on the current state of the scenarios under study: degree of impairment; frequency of algae blooms; challenges to use of the water by people, etc.	(183). This is a good comment which I agree with. However, time limitations prevented a more detailed discussion in each of the basins of their water quality circumstance as recommended by Reviewer. Nevertheless, it is a good suggestion but would have taken considerable time to incorporate into the report. For the same reasons, revising the report at this stage is also precluded, so additional changes will not be made. In presentation of the five case studies, I provide additional detail on the background water quality issues for each, but not for the nine larger basins.
(184). Page 61: Given that the assumptions are linear, the estimated loads from additional drainage is also linear. Does this not contradict studies discussed which point to impacted landscapes over-contributing to TP TN and TSS?	(184). This is a good observation. Unfortunately, it was not possible to partition the Saskatchewan basins (or perhaps sub-subbasins) included in the wetland inventory into low, medium, and high nutrient yielding landscapes (or some other classification system). Future analysis could include such partitioning, then loss coefficients could be adjusted rather than using a single average coefficient across all landscapes. This would require additional detailed analysis which was not possible initially because of time constraints. It could be done now, but only as a separate project.

Comments from Reviewer -	Response from Dwight Williamson
(185). Page 70: It is understandable that the most recent year would be selected. Is it a representative year for this river at this location and does that make a difference to the analysis?	(185). 2018 is a reasonably representative year but it was relatively dry and was preceded by several other dry years. I make a note of this in my report in the discussion of the findings of the Assiniboine River case study. The results will differ from year-to-year based upon flow, landscape water yield, background ambient water quality, etc.. Because I have already included this note in my report, I do not believe additional revisions are required.
(186). Page 85: The assumptions and methodologies in the Saskatchewan WLI, which underpins this study, are key considerations that affect this entire project	(186). Agreed. The "Uncertainties and Limitations" section has been substantially expanded to include reference to Dr. Holly's observation in her hydrology study regarding under-estimation of the current level of drainage and the implications of this to the water quality study have been described in detail. As noted in the expanded "Uncertainties and Limitations" section, the exercise of simulating impacts arising from drainage in 10 % decrements began at the historical level of wetland coverage, not at the current level. Consequently, while extremely important, the overall results may not be significantly affected assuming that the historical level of wetland coverage has been correctly estimated.
(187). Page 95: Given the importance of this water body as water source, I would have expected more discussion as to water quality impacts from a drinking water perspective. Not being from SK, I also don't know if there are water quality issues currently.	(187). This is a good comment but unfortunately, time constraints precluded a more detailed analysis which could expand on the potential impacts to this source of drinking water. Additional analysis could be done but would need to be a separate additional project.
(188). Page 105: this last little bit is important context. When reviewing impacts to Lake Winnipeg, the issue is more than TN, TP and TSS loading, but flow as well.	(188). Agreed. Because I already make a note of the importance of increasing flow to Lake Winnipeg arising from potential future wetland drainage, I do not think additional revisions are required in this section. I also include reference to Manitoba's targets for Lake Winnipeg which include the need to reduce flow to assist in achieving the nutrient standards proposed for the lake.

Comments from Reviewer -	**Response from Dwight Williamson**
(189). Page 112: The paragraph and the next strongly reinforce the view that wetland restoration and conserving existing wetlands are the most effective agricultural landscape BMPs re: management of nutrients.	(189). Agreed. No additional revisions required.
(190). Page 113: Given the relative size of urban vs rural agricultural landscapes, and the relative scale of the nutrient inputs in each, it seems that this would be a minor consideration at the watershed or basin scale. If this is to be considered an offset mechanism or BMP, some supporting data would be welcome.	(190). Agreed. Because of time constraints, I was not able to more fully explore this option in the current project. Should Saskatchewan desire, it would be worthwhile undertaking more detailed work on identifying potential offsets and trade-offs within its urban environments. Time constraints preclude undertaking additional analysis on this issue in the current project.
(191). Page 114: I think this statement should be reconsidered. Climate change is increasingly understood as being as much about climate variability as it is about warming. Given an increasing frequency of extreme events, including flooding, do you really want to make this statement? Massive floods are massive movers of nutrients.	(191). Agreed. I have deleted the statement.
(192). Page 116: I am not sure what this paragraph is trying to say. The top half seems to suggest that nutrient reductions may negatively affect fisheries. The bottom half suggests otherwise. We are dealing with systems that are already enriched through anthropogenic nutrient loading. I would recommend removing this entire paragraph.	(192). Agreed. I have removed this paragraph.
(193). Page 116: This summary is well written and effectively captures an extensive and complex analysis. It is consistent with the overall analysis and communicates well.	(193). Good. No additional revisions required.
(194). Page 119: The term "nuisance" in the context of algae-blooms may trivialize the issue in the eyes of some. I recommend not using that modifier in any of the discussions in this report. If a bloom is of sufficient size and/or duration, or blooms are occurring at greater frequency than natural background scenarios, then a better modifier would be "significant".	(194). Agree. I have made this revision throughout except in two places where “nuisance” is specifically mentioned in Saskatchewan’s guidance documents.

Comments from Reviewer -	Response from Dwight Williamson
(195). Page 121: This would be in line with regulations established in Manitoba and, from a regulatory viewpoint, would be less difficult to implement.	(195). Good comment. Agreed. No additional revisions required.

Comments from Reviewer	Response from Dwight Williamson
(196). I have reviewed and commented on each of the report sections, focussing the majority of my time on the hydrology/flooding and water quality chapters. The four sections vary considerably in their clarity, particularly in the methods employed. One central concern in my review of these documents are that the uncertainty and the assumptions associated with the approaches employed remain relatively undocumented, (most notably for the water quality section), and should be addressed to improve the robustness of this synthesis for use in conversations regarding wetland policy development.	(196). To address Reviewer's comment about uncertainty and assumptions, I have expanded the "Uncertainties and Limitations" section. This may meet most of Reviewer's concerns.
(197). This includes a more comprehensive documentation of the strengths and weaknesses of the CWI and the use of recent data in this approach as the underpinning of this work.	(197). Agreed. This would be useful but was not done individually by the authors of the four reports. Perhaps this documentation has already been compiled by Saskatchewan Water Security Agency and could be made available as a companion piece to the four reports.

Comments from Reviewer	Response from Dwight Williamson
(198). Additionally, it is worth noting that thresholds for wetland drainage was an underlying theme in the various sections, but it remains difficult, if not impossible to identify these thresholds in the absence of a defined target (see more detailed comments on water quality section). In this context it is unclear how thresholds beyond which drainage should not occur can be identified without first knowing what targets for (change to) wildlife, water export and flooding, groundwater recharge, or elemental and sediment transport are. This seems, at least on my reading, to be an important omission. Section-specific comments with an aim to improve the clarity and robustness of the sections are provided on the pages that follow.	(198). This is a good point. However, within the water quality report, I identify in one main section and subsequently reference throughout the water quality standards, objectives, guidelines, and targets in place within Saskatchewan and in neighbouring downstream jurisdictions. These are the targets that should not be exceeded. In my synthesis sections, I then conclude that the thresholds at least for nutrients have already been reached or exceeded and that further addition of N and P should not occur from additional wetland drainage without offsetting reductions elsewhere. I strengthened the language around this in a couple of places in the report to make it as clear as possible. I do not think additional revisions are required on this point.
(199). Page 9. Is this an excursion of objectives, or excursion of water quality from the targets?	(199). "Excursion of objectives" is the same as "excursion of water quality from the targets". I do not think additional revision is required.
(200). Table 8. There seems to be a problem here in that the objective for TN is less than that for nitrate, and so it is possible to meet the nitrate target while failing to meet the target for TN. This warrants further explanation/discussion.	(200). Agree, it is possible to meet the nitrate objective but not the one for TN. This arises because the objectives are set to protect different water uses - nitrate to protect aquatic life and TN to maintain the river water quality close to the existing ambient condition. I have added footnotes to the table to indicate which water use is being protected by each objective. I believe this should add the necessary clarification.
(201). Page 21. When trends are reported, it should be explicitly stated whether the trends pertain to concentration, load, or both.	(201). Good comment. Agreed. I have made revisions to make it clear that the trends refer to changes in concentration.
(202). Page 30. 'Obvious' and 'somewhat apparent' are generally not robust descriptors. Please report whether there is statistical evidence for such relationships, and what the strength of these relationships is.	(202). Agreed. I have added regression statistics between the extent of wetland drainage and flow-weighted mean concentrations to identify the strength of the relationships.

Comments from Reviewer	**Response from Dwight Williamson**
(203). Page 32. I am unable to follow the methods employed here, as a reproducible description of the methods has not been provided. Without improved description of the methods it is not possible to evaluate the data provided in tables 11 and 12.	(203). Good. I have slightly revised this section to try to make it clearer. However, it still remains that I simply re-aggregated Armstrong's (2018) results so that the information could be broadly summarized and partitioned into four categories of wetland drainage extent. Because I provided all of the information in my Appendix 1 and make reference to Armstrong's (2018) Appendix Table B.4 as the origin of the data, the results should be reproducible. It is a good comment and with my minor revision, I think it should be clearer. I do not think additional revisions are required.
(204). Page 34. Greater retention of P in drains relative to what?	(204). The sentence may be a bit awkward but it is stated that greater retention is for "…some but not all runoff events…". I have re-phrased the sentence to try to make it clearer.
(205). Page 35. Please define what is meant by 'early-season'.	(205). Good comment. I have revised to "early summer" rather than "early season".
(206). Page 37. It is important to note that a fundamental reason that nutrient export increases following wetland drainage is because the annual runoff volume increases. These export coefficient derived modelling exercises are notably limited in the regard, as there is no connection to changing runoff patterns and the magnitude of these changes, or interannual variability. This warrants further consideration.	(206). On this point, I respectfully disagree with the Reviewer. The SWAT models explicitly link changes in connectivity following wetland drainage, with changes inflow, and with changes in nutrient export. This was most clear in the Yang *et al.* (2012) models for Smith Creek where three broad scenarios were modelled with differing results largely being explained by changes in connectivity and resulting runoff volume as wetland drainage was simulated in differing parts of the watershed. Perhaps the Reviewer may have inferred that other models such as those being developed by Dr. Pomeroy and his group may better capture change in interconnectivity following wetland loss. I agree that the models do not well account for inter-seasonal or inter-annual variability but represent averages over the modelling time-period. I do not think a revision is necessary in this section.

Comments from Reviewer	**Response from Dwight Williamson**
(207). Page 38. Again here, results (Tables 13-16) are presented without adequate description of the methods. Notwithstanding the limitations highlighted in the previous point, it is not possible to evaluate these results without understanding the methodological approach used to derive these numbers.	(207). This is a good comment but I think all of the detailed information on methods can be reasonably located - virtually all of the information in Tables 13 to 16 was extracted directly from the primary references, which are cited in the table captions. All of the methods are contained in the primary reference source. I have described in the text those cases where I had to re-calculate the information provided in the primary reference tables in order to convert changes in TP, TN, sediment, and flow to a per hectare of drained or restored wetland - this occurred in cases where the changes were originally expressed on a broader basis (for example, the original authors may have provided results for the "restoration of 50 % of the drained wetlands", so this was converted to changes on a per hectare basis). I do not think additional revisions are required in this section.
(208). In this section, a more thorough discussion of the limitations of treating all wetlands as the same is warranted. Biogeochemical processes (notably for P, but also for N) will differ across wetlands with different chemical conditions. So, for example having a large wetland near the basin outlet can act as a hydrological gatekeeper, but whether the chemical nature of this wetland is likely to lead to phosphorus reaction with silicate minerals or reaction with iron, aluminium or calcium will also be important.	(208). I agree with all of the reviewer's comments and do understand these various relationships are important. Unfortunately, most are fairly site-specific and cannot be accounted for in the broad scaling-up process for the province-wide wetland inventory. Nevertheless, the comments are good and could be the subject of another more focused project.
(209). On page 43, further explanation of what the trend is is needed, as this is not clear from the data in table 17. Additional clarification as to whether there was a test for a trend, or this is simply a loose pattern being described should also be provided.	(209). Agreed. I have made a slight revision to delete the word "trend" and just refer the change to an "apparent increase". Reference is also made to the interpretation that the principal authors make to this finding as well, so I don't think further revisions are required.

Comments from Reviewer	**Response from Dwight Williamson**
(210). For Table 20, additional explanation is needed to help the reader understand what is being shown. Of note are that the estimates of load reduction don't correspond to the baseline condition categories. The reasons why this would occur, and detail on what these conditions are meant to represent should be provided.	(210). I have revised the first column to be clearer and have removed reference to "baseline", since I agree this was confusing. Additional details regarding methods can be found in the original reference (Yang *et al.* (2014)) from which the information was obtained. I do not believe additional revisions are required.
(211). On Page 48 it is stated that: "there is increasing evidence of chemostatic behaviour and biogeochemical processes". It isn't clear what is meant by biogeochemical processes here. What does there being 'increasing evidence' of processes tell us?	(211). I agree this was unclear. I have made fairly major revisions to the text to make it clearer. Reference to "biogeochemical processing" was not too helpful, so this phrase was deleted as well. A new section was added, which pulls together all of the chemostasis information, and places it into a new context - the new section is titled "<u>Chemostasis and Nutrient Loading versus Nutrient Concentration</u>".
(212). There is room to present a clearer line of evidence here. As written the section moves from chemostatic behaviour to chemodynamic behaviour and back, and risks losing the narrative for the reader.	(212). Good. Hopefully, my significant revisions to the discussion on chemostasis in the new section titled "<u>Chemostasis and Nutrient Loading versus Nutrient Concentration</u>" makes discussion clearer.
(213). Page 48 (last line): Why would flow volume increase as wetlands are restored?	(213). Correct. This was an error and has been corrected - "increase" should have been "reduction".

Comments from Reviewer	**Response from Dwight Williamson**
(214). The argument is made that an approach that favours land area–based loading coefficients is the preferred approach. The evidence for such an approach being favoured has not been clearly illustrated. The limitations of such an approach, including with respect to what is known about hydrological behaviour and variability in runoff generation (see the examples in the hydrology), and the suitability of extending watershed derived coefficients beyond the boundaries of a watershed have not been adequately explored. Draining wetlands that don't change the effective area can be expected to have a different outcome than draining wetlands that will regularly contribute streamflow. At a minimum, this approach should be quantitatively compared and contrasted with other methods (e.g. chemostatic/chemodynamic behaviour in association with CDA derived estimates of changes in annual runoff volume) and the limitations of both explored in detail prior to recommending one approach over another (or combining estimates to better document uncertainty associated with individual methods).	(214). I agree in that these are all very good comments. Ultimately, time limitations precluded a more thorough or more detailed comparison of potential methods to scale modelling information from watersheds to geographical regions. While the Reviewer refers to the approach used here as a "land area-based", I prefer to characterize the way I derived the coefficients as a "wetland area-based" approach - of course, the original models considered changes in land use and wetland connectivity in their work, and this is embedded in the coefficients that I derived from their work. A brief comparison was made between studies in which three different approaches (two different modelling methods and one field study) were used, and this comparison is found in the section titled "Scaling from Watersheds to Larger Geographic Regions". While this comparison does not go as far as the Reviewer recommends, given the timeline of the current project, it is thought to be initially sufficient. Ultimately, the approach used here may be attractive for application elsewhere in which case, additional comparisons can be made.
(215). For the case studies provided, a rationale as to why these particular locations were used, rather than applying the methods to larger basins or sub-sub basins (as explored in the hydrology chapter) is missing. As was described for instances above, the methods for the work shown in the case studies are inadequately described.	(215). This is a good comment. I have added some additional detail to explain why these five case studies where selected. Unfortunately, because of time limitations, I am not able to provide too much more detail on the methods used in each of the case studies - I believe I have provided sufficient information in the existing text for readers to gain a broad understanding of the methods used such that, if desired, the work could be tested or replicated.
(216). Page 56: It is stated that both drainage and restoration scenarios will increase flow weighted TP and TN concentrations, but this (restoration increasing concentrations) is not in agreement with much of the earlier information provided. Further explanation on this point is necessary.	(216). I agree that this is unclear. I have revised the text to make it clearer.

Comments from Reviewer	**Response from Dwight Williamson**
(217). While confidence intervals are presented, these CI it seems were established for select basins from which the coefficients were derived, and therefore fall short of a more complete uncertainty analysis that would be key to exploring to robustness of the estimates made in this analysis. Explicit variability within larger watersheds, and between large watersheds is needed.	(217). In terms of the confidence intervals, I also agree that the confidence intervals used fall short of a more robust analysis of uncertainty, but given the time limitations of the project, this approach was the most scientifically appropriate and consistent manner to represent some degree of uncertainty associated with the analysis.
(218). In the final section of this report (page 116), the topic of drainage thresholds is raised. As with the other three topics, which all have attempted to quantify (with varying strengths) change in different indicators associated with wetland drainage, it is difficult, or near impossible to identify such a threshold without a target for change. Only with a target for nutrient loading (or other indicators) identified, can such an analysis be used to identify thresholds beyond which wetland drainage should not occur. Once identified, an analysis (or analyses) could be used to support identification of such thresholds, provided that the uncertainty associated with the approach is well-documented (a notable limitation here).	(218). This is a good point. However, within the water quality report and as per an earlier response, I identify in one main section and subsequently reference throughout the water quality standards, objectives, guidelines, and targets in place within Saskatchewan and in neighbouring downstream jurisdictions. These are the targets that should not be exceeded. In my synthesis sections, I then conclude that the thresholds at least for nutrients have already been reached or exceeded and that further addition of N and P should not occur from additional wetland drainage without offsetting reductions elsewhere. I strengthened the language around this in a couple of places in the report to make it as clear as possible. I do not think additional revisions are required on this point.

Comments from Reviewer	**Response from Dwight Williamson**
(219). Overall: Notwithstanding the limitations of the approach (both in describing the methodological approach itself, but also characterizing the fundamental strengths, weaknesses and uncertainty) to quantitative analysis (see above), the conclusions of the report, namely that wetland drainage, even at low levels, will yield elevated nitrogen, phosphorus and sediment export, are consistent with the wide body of literature on this topic in the Prairie Pothole Region, which suggests that the single most valuable BMP available for nutrient/sediment management and maintenance of water quality is to retain (and restore) wetlands. There remains a need to more robustly quantify changes in nutrient and sediment export associated with drainage prior to identifying (tolerable, or low risk) thresholds of drainage to meet *a priori* targets.	(219). Good. I don't think additional revisions to the report are required. Additional study could be undertaken in the future to more robustly predict nutrient and sediment export associated with drainage, as suggested by the reviewer