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Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
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Comment on hess-2022-128

Anonymous Referee #1

Referee comment on "Simulating the hydrological impacts of land use conversion from annual crop to perennial forage in the Canadian Prairies using the Cold Regions Hydrological Modelling platform" by Marcos R. C. Cordeiro et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-128-RC1, 2022

General comments:

The manuscript presents modelling results to evaluate the potential of replacing arable crops by forage crops to reduce eutrophication problems in the Canadian Prairies. It approaches the topic from a hydrological perspective by investigating to which degree the different crops affect runoff formation causing nutrient losses. This topic fits the scope of HESS. The manuscript reads well and is generally easy to follow. Nevertheless there are a number of critical issues that need to be resolved before the manuscript is ready to be published.

Unbalanced discussion and literature review. The Introduction and the Discussion is not very balanced regarding potential advantages and disadvantages of forage crops. Advantages of forage crops are highlighted, disadvantages such as observed increased nutrient concentrations in runoff are neglected despite referring to articles (Liu et al., 2014) that point out these aspects in very clear manners (see below). A more comprehensive discussion is needed to provide the reader with broad and differentiated arguments. It might be also useful to touch upon the question what such a large-scale land use change might imply for the agricultural sector. I am aware that the authors aren't the specialists for that aspect. Nevertheless, it may be useful to at least refer to that aspect to avoid naive views on the problem. This broader view may also be relevant for asking relevant questions for hydrological research in the future to address the topic from a more interdisciplinary perspective.

Additionally, there are a number of parameters for which it seems that the authors have subjectively chosen numerical values (e.g. stomatal resistance, L. 331 - 332). Given that the water balance at the soil surface has a major impact on the model result I had expected to see a sensitivity analysis for parameters that the authors have selected based on their expert judgement.

Detailed comments:

- L. 17: "resulting in lower water yield and concomitant export of nutrients": From a nutrient balance perspective: where would the nutrients not lost end up in the system?

- L. 17: A related aspect: what are the nutrient budgets for the two alternative crops (fertilization rates, yield export)? This is important for the long-term effect of any given crop choice.
- L. 29: Introduce abbreviation upon first use.
- L. 33: Which nutrients? N or P or both?
- L. 35: Which kind of intensification took place?
- L. 37 – 38: The way of referencing is somewhat misleading. As written, the citation evokes the impression that Liu et al. (2014) proposed this conversion (based on their scientific findings). However, these authors describe that "Conservation initiatives on the Canadian Prairies are attempting … by promoting conversion of annual cropland to perennial forages" (Liu et al., 2014, p. 1645). Actually, the authors formulate based on their empirical findings some warnings regarding this suggested conversion: "When nutrients are released from plant residues by freezing, the introduction of perennial forages to a crop rotation may increase P losses in surface runoff during snowmelt." (Liu et al., 2014, p. 1654). Such a framing puts this manuscript into quite a different perspective.
- L. 39: Agronomic practices are neglected.
- L. 41 – 42: This sentence gives the impression that conversion to perennial crops were a better alternative than arable crops. However, given the findings cited above (Liu et al., 2014), this implicit assessment is not necessarily true.
- L. 68 – 69: How is it possible to achieve "physical realism of hydrological processes without the need of parameter calibration to achieve accurate simulations."? This holds especially true for parameters such as soil hydraulic parameters at the spatial scale of HRUs. The statement is also in contradiction with (He et al., 2021), which states: "… were initialized based on the soil textures in WGC basin, and then slightly adjusted using trial and error based on the NSE and logNSE values of the streamflow simulation in the calibration period." (He et al., 2021, p. 5).
- L. 129: What are possible reasons for the poor performance under drier conditions?
- L. 138 – 144: This seems to indicate that a major change was introduced apriori to the model structure!? Does this not lead to the situation that the model results simply reflect the initial hypothesis?
- L. 140: the use and motivation for the parameter "fallstat" is obscure to me. Should the degree of saturation of the soil not result from the water balance simulations of the antecedent period? "Defining" a degree of saturation will generally induce a water balance error, wouldn't it? Please explain and clarify.
- L. 145 – 146:] I suggest to extend the range between 0 and 70\%. This allowed to assess the vegetation effects on SWE separately from the effects on soil properties (i.e. infiltration capacity).
- L: 174 - 180: How have the meteorological point data extrapolated in space?
- L. 210, Fig. 3 (and following): The figures differentiate between the two crops with green and red colors. Given that about 8\% of the male population is color blind, I strongly recommend to change the color code and potentially also use different symbols to avoid readability problems.
- L. 235: Can the larger SWE for forage crops be fully explained by reduced sublimation? It seems that transport and wind erosion would not cause such differences because in a scenario with one land use only (arable crop or forage only), any transport and erosion would lead to snow deposition somewhere else in the catchment without a net change of the surface water balance. Can you comment on that?
- L. 285: The assumption of constant nutrient concentrations contradicts the empirical findings by Liu et al., (2014) reporting substantial increase of several nutrients upon a change from arable crops to perennial forage. This puts the results in quite a different perspective.
- L. 285 – 302: This section seems biased in that only results are reported that favour a transition from arable to forage crops. Conflicting findings are neglected despite the fact that one of such papers (Liu et al., 2014) is cited.

- L. 312 - 313: The mechanism of how the macropore flow is mimicked by the model is not very clear. Please provide more (technical) details.
- L. 330 - 340: These aspects should be investigated with a sensitivity analysis. This should be straightforward and would provide more robust information how relevant this parameter might be for the overall results.
- L. 347: The term \textit{uncertainty framework} seems misleading. If I got it right it simply consists of a sensitivity analysis by varying one single parameter in a pre-defined range. The notion of \textit{uncertainty framework} suggests a conceptual novelty which as absent.
- L. 348 - 349: This outcome seems rather trivial: empirical evidence at field scale has been conceptually be incorporated into the model and applied to a larger scale. Therefore, the model results are no independent test whether the local observations hold true if scaled up. Actually, the question whether

References:

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