General comments

The manuscript entitled "Developing a tile drainage module for Cold Regions Hydrological Model: Lessons learned from a farm in Southern Ontario, Canada" submitted to EGUsphere introduces and evaluates a new tile drainage module for estimating surface and tile drainage flows using an 8-yr surface and tile drainage data set from a farm in southern Ontario. While the manuscript is generally well written, some important clarifications are needed throughout. Consistent terminology should be applied to soil water and groundwater flow processes. For example, the term 'soil water levels' is frequently used but needs to be consistently defined in the overall conceptual model of water flow. In several sections it is unclear whether the authors are implying unsaturated (e.g., matric potential) or saturated (shallow groundwater, Darcian flow) flow conditions. While considerable time is spent on the importance of the capillary fringe, there is an overall lack of discussion focused on the model predictions versus field flows and how they were influenced by annual weather variation or seasonally driven factors. It would also be beneficial for readers to see how the model performs for some signature (large) runoff events (maybe include some snowmelt events since the model accounts for these processes). It is not clear how modeled groundwater dynamics were used to adjust tile drain flows. The abstract does not mention how the module performed for the field data. It also mentions lateral flow to tiles but there is no relevant discussion of the results. Considerable time is spent on introducing the new module in the introduction compared to the relatively short time discussing model effectiveness for the field data. It would be beneficial to present some "next steps" for TDM module development based on the present findings and vision for further calibration/validation and improvement. Overall, the ideas and data presented by the authors are novel and well-conceived. The paper will benefit from a hearty revision. Please refer to line-by-line comments below.

Abstract

The tile drainage module's effectiveness for predicting field tile flows should be added. A stronger concluding sentence based on the implications of your most significant finding is suggested. Line 27: "Water level patterns". Clarify 'water levels' here and throughout, i.e., groundwater (saturated/gravity driven flow) or soil-water under tension (unsaturated, under tension/Richard's eq.)

Introduction

Line 116: Clarify "soil water level". Saturated or unsaturated conditions?

Lines 118-126.

Line 147: Does "pressure head" refer to groundwater elevation head?

Methods

Line 158-159. Tile water may include shallow groundwater flows not exclusively "infiltrated" from the study field.

Line 166-167. Clarify the last sentence more. How are surface and groundwater isolated for measuring and sampling if they outlet in the same area?

Lines 188-191: What's the size range of HRUs that can be modeled?

Line 207: "…soil water level (water table position). Please clarify. Is soil water level synonymous with water table elevation head?

Line 232: 31. 7 km seems far for assuming similar precipitation patterns.

Line 265: Clarify "soil water level observations"

Line 283: Clarify "water level" in conditions 3. Groundwater level?

Figure 2. The schematic looks good but it is slightly hard to follow.

Line 322: "…water level from soil moisture". Groundwater elevation head level?

Line 324: "…and was calculated as…"

Line 335: How is 'bottom of the soil' determined? Same as tile depth?

Line 362: $G_{y,I}$ is not defined in variable explanations. How were changes in groundwater elevation used to modify tile drain flows?

Line 387: Five different methods

Line 411: Suggest combining results and discussion

Figure 4. Add a period after "Figure 4". This graph looks good. Maybe enlarge somewhat if possible. SWE needs to be defined in the caption.

Figure 5. On the y-axis you have "Groundwater/soil water level (mm)", assuming they are indeed synonymous. In the caption, however, you refer to it as "water level in the soil". As stated, please revise this terminology consistently throughout to avoid the confusion.

Lines 456-457. Did you evaluate seasonal or interannual variability of model predictions? What about looking at other times when there was either good or not so good agreement between observed and modeled flows?

Table 2. Add a period after “Table 2”. Add time period over which modeling represents. Assuming it included all events over the monitoring period? What about interannual variation? Add a footnote explaining model performance acronyms (NSE etc.).

Figure 7a,b: Are these best fit lines or model predictions? Please clarify.

Figure 8. Add period. Define LON.

Figure 9. Clarify “soil water levels” in caption. Matric potential?

Line 552. It is not clear if groundwater is being used to adjust tile flows. How are SWL, groundwater elevation head and matric potential related over a range of soil moisture contents?

Line 554. Suggest revising the term “opportunity time” to residence time or a similar term

Line 579. Clarify SWL (matric or elevation head?)

Line 592. “soil water level depth”. Clarify

Line 599. Is one field representative of catchment scale hydrology?

Lines 624-629. More discussion should be added here groundwater dynamics and relationship to capillary fringe and tile flows. Including some recent references and how they relate to your findings would also benefit the paper.

Line 632. Modeling flows in one field does not capture the myriad of conditions found in larger, more complex catchments. Also, how does the model handle the high variability of saturated hydraulic conductivity, porosity and other spatially variable inputs? What about preferential flow to tile drains? How was this handled for the field site and was it a substantial component of overall flow (macro vs. micropore flow?).

Line 656. Do you mean the depth of tile drains below the soil surface?