We thank the reviewer for the comments and kind suggestions to improve this manuscript. We have carefully considered them all and revised the manuscript accordingly. In the following, we have provided point-by-point responses (red) to the reviewer's comments (black).

Reviewer #2

Summary:

The authors have presented a study in which they modify the CoupModel to consider the effects of dissolved solutes on the soil-water freezing point depression. The study is, for the most part, a valuable contribution to the field of cold regions hydrological modeling. However, the structure of the manuscript makes it hard to follow in many places. A clearer description of the model processes is needed, and the results and discussion need to be split into two separate sections. There are also many small grammatical errors throughout, I have highlighted some but there were too many for me to individually address.

Response to comment: We have reorganized the structure of the manuscript and split the section "Result and Discussion" into two separate sections.

I also have somewhat of an issue with the entire approach to this kind of modeling. The authors incorporate a new process in the model, but rather than systematically testing the impact of the new physics on the model outputs against simple test cases that isolate the effect of one process on another, they authors try to simulate a field experiment with a multitude of different and interacting processes. An alternative approach would have been to simulate a much simpler, highly controlled experiment (for e.g. the authors could have simulated the soil column experiment of Stähli and Stadler (1997)), to test the performance of the newly implemented freezing-point depression relation. After that the authors could have applied their model to their field site. However, I admit this is a philosophical difference of opinion and there are other researchers who would advocate for this approach. Thus, I only bring up this point for the consideration of the associate editor. I believe the content of the article fits well within the scope of HESS and could be useful for the targeted audience. However, in my opinion, the manuscript could be suitable for publication in HESS only after major revisions are done to the structure and quality of writing of the manuscript.

Response to comment: Yes, we agreed with the reviewer that the soil column experiment, for instance, similar to Stähli and Stadler, (1997) may be a simpler and straightforward approach to test the freezing-point depression curve in relation to the salt concentration, however, experimental studies often encounter limitations, like shorter periods of implementation, and making assumptions of simpler boundary conditions, which is contradictory to our field conditions, where the processes related to snow, soil evaporation, and groundwater table depth and their interactions with soil heat and water transfer provide more complicated upper and bottom boundary conditions. Moreover, the advantages of our approach (a Monte-Carlo based calibration) not only ensure parameterizations are efficient to describe the observations but also identify the relative importance of parameters to the model performance.

Lines 131 – 135: Did this pre-calibrated relationship consider the in-situ soil salinity already present and its effect in the electrical potential of the soil water? If the soils at both sites are affected by salinization issues, then wouldn't in-situ salinity be an important consideration? More discussion of this is warranted. Furthermore, was any in-situ salinity measured? This could potentially a major issue in the field data collection. Can the authors give reason as to why this was not attempted? Possibly due to equipment trouble?

Response to comment: We have made the pre-calibration in the laboratory by using soil samples with different water contents setups. Yes, when we use this pre-calibrated relationship in the field, it will result in some uncertainties since in the field the salinity of soil will change due to the dynamics of water and salt. This has been discussed in this revision.

As to the measurement of in-situ salinity, we have tried by using some instruments. Unfortunately, the instruments did not work properly even with a pre-calibrated relationship because the salinization of soil made the instruments eroded.

Besides, there is still an issue even if the instrument works in the field. Since we calibrate the instrument in laboratory with unfrozen soil, when we apply it to frozen soil, the changes in liquid water content due to soil solution condensation will greatly influence accuracy of measurements of soil salts in the field. Thus, currently the salt contents measured from the collected soil samples is more reliable than the in-situ measurements we have tried to attain. Nevertheless, it is worth mentioning that we are developing the TDR probes for simultaneously measurements of soil water, temperature and salt and have tested them in the laboratory. Hopefully, we can use them for future in-situ measurements after a proper calibration.

Lines 158 – 164: Was a sensitivity test carried out to ensure model discretization and timestepping choices did not affect the numerical solution? In my experience, when modeling water partitioning at the soil surface, node spacing may sometimes have to be smaller than 10 cm.

Response to comment: The size of model discretization and the number of iterations per day (i.e. timestep) is based on previous modelling experience of CoupModel (e.g. Wu et al., 2016, 2018). In this study, our results (Fig. 7) have indicated that the size of model discretization is sufficient to resolve model precision against measurements. The depth of the top soil layer is 10 cm, which corresponds to the measurement at 5 cm depth, assuming that the soil texture within each layer is homogenous.

Lines 258 – 260: Why was initial salt concentration, precipitation salt concentration and irrigation salt concentration used calibration parameters? Couldn't these numbers be obtained from the site measurements? I have an issue with the initial salt concentration being used a calibration parameter, it will obviously influence the tracer transport. This brings me back to my earlier point amount measuring the in-situ salinity of the soil. It is also possible to estimate soil salinity using TDR (e.g. Stähli and Stadler, 1997). One could also envision the initial salt concentration profile being estimated from the change in electrical conductivity after the initial application of the Br- tracer.

Response to comment: We used the initial salt concentration, precipitation salt concentration and irrigation salt concentration as calibration parameters since we found there were large differences in the field on the initial salt concentrations when we measured the initial salt profiles at various points of the field. The model is sensitive to the initial salt concentrations. So we set the mean values from different points as the initial values and also put it in the model calibration. For precipitation salt concentration, we did not have measurements, so we just used the model default values. But in the model sensitivity analysis, we found the model is sensitive to this parameter, so we also put it in the model calibration. As to irrigation salt concentration, we measured the irrigation water several times during the irrigation period, and noticed that their values were not stable. Model is also shown sensitive to the irrigation salt concentration. Thus, that is why we used the measured values as prior, and also put them in the model calibration.

We agreed that TDR has shown a promising potential in estimating soil salinity by Stahli and Stadler, 1997. In our case, we have tried it in laboratory. Unfortunately, we failed due to a quite high salinity in the field soil samples which increased resistance of TDR probes and even made the measurements of soil water unrealistic. Therefore, we decided to adopt the TDR probe in laboratory to accurately measure soil salinity.

Section 3: This section in general is written in a rather convoluted manner and is not the easiest for the reader to understand. In general, I think the entire section 3 needs to be rewritten to address the many grammatical issues and lack of clarity in the description of the processes and model calibration/validation approach.

Response to comment: We thank the reviewer to point out the language issue of Section 3. We have rewritten it.

Section 3: I think the authors need to more clearly emphasize that they employ both Esbey's (1992) bypass flow routine as well as Stahli et al.'s (1996) dual-domain hydraulic conductivity concept to simulate water flow in frozen soil, i.e. you have high and low flow domains in the soil matrix and additional by-pass flow through macropores. The authors need to careful to clearly identify the different processes being simulated as there is overlap in the terminology used to described different processes in the model. For example, in lines 182 – 184 you use the term high-flow domain to describe the bypass routine's ability to route water directly in the underlying soil layer when the infiltration capacity of the soil matrix is reached. However, you also you the term high-flow domain in lines 196 – 199 to describe the frozen soil hydraulic conductivity model of Stahli et al. (1996). Thus, you need to be very clear about which frozen soil flow process you are describing. It must also be noted that this approach, while rather complex, still has its conceptual limitations as refreezing of infiltrated water can also occur in macropores (Watanabe and Kugisaki, 2017). While the concept of Stähli et al. (1996) has widely been used to incorporate the effects of preferential flow and refreezing of infiltrated water, you also employ a by-pass flow routine which does not consider the effect of refreezing along macropores. Some discussion of this is warranted. See the recent review of Mohammed et al. (2018) on macropore flow in frozen soils.

Response to comment: Thanks for the comment. We find that this is a good point which will be clarified in the revised manuscript. We have read about the three papers suggested here and referred them in our discussions in the revision.

Section 4: This section needs to be split into two separate Results and Discussion sections.

Response to comment: We have done it in the revision.

Line 447: The authors did not develop a new relationship, other frozen soil models (e.g. SHAW - Flerchinger Saxton (1989)) have long incorporated the relationship between osmotic potential and soil freezing temperature.

Response to comment: We have deleted this sentence.

Lines 452 – 453: Again, it is not enough to simply state that they impact soil heat and water transport. How specifically does it impact heat and water transport?

Response to comment: Thanks for the suggestions. We have added some explanations on how salt affects soil temperature and soil water.

Lines 511 – 513: More discussion the model misfit at lower depths is needed. From figure 8, at one site the model overestimates soil water at depth and at the other site it underestimates it. What are the reasons for this? Is it an improper representation of the vadose zone flow processes (as the authors include quite a complicated representation of the soil water flow processes) or a misrepresentation of the lower boundary condition and influence of groundwater, or some combination of both?

Response to comment: Thanks for the suggestions. It is really a good point. We have added more discussions on this. Yes, we noticed that in figure 8, soil water was overestimated at Site Qianguo and underestimated at Site Yonglian. For these two sites, their hydrological processes are quite different. At Site Qianguo, the near-surface soil water is more influenced by the snow cover, which means that the infiltration of snowmelt will affect soil water transport. Therefore, as the reviewer suggest, the model bias might be due to an improper representation of boundary conditions and soil infiltration. At Site Yonglian, the major attributor can be the ice cover formed after irrigation. The irrigated water did not fully infiltrate into the soil profile as the model simulates, thus causing an overestimate in the model. This has led to a large influence on the surface water and energy balance in the field. We have discussed these issues in more detail in the revised manuscript.

Lines 527 – 538: More discussion of the large difference in the model and measured salt storage is needed. What was the mass recovery of the applied Br- tracer relative to the application?

This would give some insight into the flow processes affecting solute transport. For example, very little BR- was measured at the Qianguo site… was this due to leaving due to preferential flow to groundwater or increased retention of the tracer near the surface? This could also be a possible reason for your mismatch of soil water storage at deeper depths (see comment above).

Response to comment: That's a good point. We have calculated the mass recovery of Br- in another paper (Wang et al., 2016, Soil Science) and found that the recovery was above 80%. We appreciate the insight of reviewer on the flow processes affecting solute transport and thus we have added a discussion on how the preferential flow may explain soil water and salt storage mismatch.

Additional references:

Espeby, B. 1992. Coupled simulations of water flow from a field-investigated glacial till slope using a quasi-two-dimensional water and heat model with bypass flow. Journal of Hydrology 131:105–132.

Flerchinger, G.N., and K.E. Saxton. 1989. Simultaneous heat and water model of a freezing snow residue–soil system: 1. Theory and development. Transactions of the ASAE 32:565–571.

Mohammed AA, Kurylyk BL, Cey EE, Hayashi M. 2018. Snowmelt infiltration and macropore flow in frozen soils: overview, knowledge gaps, and a conceptual framework. Vadose Zone Journal 17(1).

Stähli, M., P. Jansson, and L.C. Lundin. 1996. Preferential water flow in a frozen soil: A twodomain model approach. Hydrological Processes 10:1305–1316.

Stähli M, Stadler D. 1997. Measurement of water and solute dynamics in freezing soil columns with time domain reflectometry. Journal of Hydrology 195(1-4):352-369.

Watanabe, K., and Y. Kugisaki. 2017. Effect of macropores on soil freezing and thawing with infiltration. Hydrological Processes 31:270–278.

Technical corrections:

Abstract:

Line 15 – 16: Delete 'In this context' from the sentence, unnecessary.

Response to comment: We have deleted it.

Line 17: '… influences of salt on cold region hydrology' is too vague a statement. Reword to be more specific, for example: '… influences of soil salinity on soil water and heat transport'.

Response to comment: We have revised it as the reviewer suggested.

Line 18: Modify sentence to 'We modified the CoupModel to simulate the impacts of salinity on soil freezing point depression'.

Response to comment: We have revised it as the reviewer suggested.

Line 21: Delete words 'into CoupModel', unnecessary.

Response to comment: We have revised it as the reviewer suggested.

Line 26: Change 'provided' to 'provides'.

Response to comment: We have revised it as the reviewer suggested.

Introduction:

Line 35: Awkwardly worded sentence, change to something along the lines of 'Knowledge on soil freezing and thawing is needed to better understand mechanisms…'.

Response to comment: We have revised it as the reviewer suggested.

Line 48: Change to '… in the two same agricultural fields in this study…'

Response to comment: We have revised it as the reviewer suggested.

Line 61: Should be '… agricultural fields'.

Response to comment: We have revised it as the reviewer suggested.

Line 62: Modify to '… and other cold region ecosystems'

Response to comment: We have revised it as the reviewer suggested.

Line 63: Should be 'However there are large uncertainties…'

Response to comment: We have revised it as the reviewer suggested.

Line 64: Modify to 'and coupled transport processes'.

Response to comment: We have revised it as the reviewer suggested.

Line 64 – 65: Modify sentence to '… uncertainty analysis methods have been utilized by…'.

Response to comment: We have revised it as the reviewer suggested.

Line 67: modify to '… is a commonly used…'.

Response to comment: We have revised it as the reviewer suggested.

Line 70: Modify to ‘GLUE is performed…’.

Response to comment: We have deleted it.

Line 75: Delete ‘… in the northern part of China.’ Redundant.

Response to comment: We have revised it as the reviewer suggested.

Line 86 – 87: Modify sentence to ‘We modified the CoupModel to consider the impacts of salinity on soil freezing…’.

Response to comment: We have revised it as the reviewer suggested.

Line 89: Modify to ‘… 2) perform a sensitivity analysis on the new model’.

Response to comment: We have revised it as the reviewer suggested.

Line 90: Modify to ‘… in modeling hydrological process in seasonally frozen soils.’

Response to comment: We have revised it as the reviewer suggested.

Material and Methods:

Line 93: Should be ‘… in northern China.’

Response to comment: We have revised it as the reviewer suggested.

Line 95: Should be ‘Field experiments at…’.

Response to comment: We have revised it as the reviewer suggested.

Line 128: Should be ‘During the soil freeze-thaw period at…’.

Response to comment: We have revised it as the reviewer suggested.

Line 192: you not describe what the pF value is.

Response to comment: We have revised it as “pF value (i.e. the logarithm of the absolute value of soil matric potential)”.

Line 229: Change to ‘latent heat transfer:’

Response to comment: We have revised it as the reviewer suggested.

Line 257: Lateral boundaries? Isn’t Coup a 1-D model? Why would it need a lateral boundary condition?

Response to comment: We have deleted “and lateral”.

Line 285: You refer to equation (5), but are talking about the surface energy balance, I think you meant equation (15).

Response to comment: Yes, we meant to equation 15 and we have revised it.

Line 291: need a citation for the Richardson equation as readers may not be familiar with the relationship.

Response to comment: We have added the following reference to the Richardson equation.

Richardson, H., Basu, S., and Holtslag, A. A. M.: Improving stable boundary-layer height estimation using a stability-dependent critical bulk Richardson number, Bound.-Lay. Meteorol., 148, 93–109, doi:10.1007/s10546-013-9812-3, 2013

Line 323: I think you mean H is the total sensible heat stored in the soil, not total energy?

Response to comment: Yes, H is sensible heat flux and calculated from the total heat storage E.

Results and Discussion:

Figure 3: Figure 3a should be modified to use solid circles like Figure 3b. It would make the Fig. 3a easier to read.

Response to comment: We have revised it as the reviewer suggested.

Conclusions:

Line 543: Should be '… are coupled in agricultural fields'.

Response to comment: We have deleted this sentence.

Line 566: Replace 'would be very necessary in investigation' to 'is still needed to improve understanding of

Response to comment: We have revised it as the reviewer suggested.