Reviewer One:
The author explained most of the comments of reviewers. I feel that there is still a problem. The whole country boundary of China and Taiwan province of China should be shown in Figure 1 (including country boundary of China on the ocean)

We thank the reviewer for their feedback and are happy to hear our previous responses were acceptable. The authors' intention with the inset in Figure 1 is to provide a geographical context for readers who may be unfamiliar with the location of the Heihe River Basin on a global scale. To improve on this with the reviewer's feedback in mind, a new map has been selected which expands on the surrounding geographic area and includes the areas noted above. In addition to the new map, the text in the figure description has been updated to make the authors' intention clear.

Reviewer 2:
The manuscript aims to predict the impact of cryosphere changes on groundwater-surface water interactions in the Heihe River Basin. Overall, this manuscript has been improved, and is more valuable. However, I think it's better to provide more details about the model initialization and conclusion. Specific suggestions are as follows:

We thank the reviewer for their suggestions and appreciate their positive review. The additional minor revision comments were very helpful in continuing to improve the clarity and scientific relevance of the manuscript. All minor comments were addressed and fixed in the manuscript text. Full responses to the specific suggestions are below.

1. *Lines 249-253. About the starting point of the model for all scenarios, authors mentioned "…… a two-year spin up coupled to CLM using climate forcings from the 2011 WY was performed. ……ran for 55 years with ParFlow only. Then, for an additional 18 years coupled to CLM using the 2001 WY and 2002 WY climate forcings." Why the two-year spin up coupled to CLM using climate forcings from the 2011 WY? Moreover, why choose "55 years" and "18 years" to ran the model rather than other number of years, such as 50 years and 20 years? How many years did the model run from the starting point for all scenarios? Did the model only run 11 years, i.e., from 2001 WY to 2011WY for all scenarios?*

It was our intention to provide as much transparency as possible about the spin-up process, however in this case we think the details we provided are obscuring the main point which is only that we successively initialized our model to achieve a stable equilibrium condition before running simulations (as is common practice in watershed modeling). The simulation lengths for the spin-up steps are determined entirely by the performance of the model and cannot be predicted which is why the numbers seem random. We have decided in response to this comment to simplify the main text to remove the unnecessary detail that was provided in the original manuscript. In the revised text, we now focus on how we determined when spin-up was complete rather than describing all individual year lengths since that is the more important factor.

However, all values will be clarified in detail here. First, all of our year lengths correspond to the point at which we achieved steady state when we spun-up the model at various stages and could not be pre-determined. In every stage of spin-up we evaluated the equilibrium conditions of the model by comparing changes in storage to other water balance components. It took 115 years to bring the ParFlow model into steady state. We chose 2011 as that was our calibration year to ensure we would not see large fluxes in the pressure field when we began calibration coupled to the climate model. It is not in this version of the text, but we also did an abbreviated spin-up of the model after each parameter change because it takes time for the groundwater system to move and to see the real impact of the parameter changes made.

After we got our best set of parameters from calibration, another spin-up was necessary to make sure that the scenarios with the new parameter set would start from a steady state. 55 years is how long it took to achieve steady state with the long-term recharge forcing, and 18 years running with the coupled climate model to bring it as close as possible to what the groundwater system would look like at the start of the scenarios (2001WY). In fact, we surpassed our target of storage change as a percent of recharge of 1 % in both these cases out of an abundance of caution. It cannot harm the results to spin-up for longer, the only cost is increased compute time. After we had this new pressure state, that is what we used as a starting point for all scenarios. All running before this point was only to get a properly calibrated and initialized model to run the scenarios from. Each scenario was then run from this starting point for 11 years each for the reasons given from Lines 282-286.

2. *I suggest a summary of the effects of cryosphere changes on seasonal hydrological characteristics to consistent with the objective of this paper stated in Lines 88-91.*

We appreciate the suggestion and have updated the text in both the seasonal discussion (Section 4.3) and the conclusion to more closely reflect the objectives stated on Lines 88-91. This includes specific statements on the impacts to surface and groundwater storage depending on the season for each of the scenarios.

Minor comments:

1. *The number of section headings from section 3.2 to section 3.5 is incorrectly marked.*

This has been corrected

2. *Missing units in right ordinate in figure 8 and figure 11.*

This has been corrected

3. *Does "15.0-3.9%" refers to "15-39%" in Line 539?*

It is supposed to be 3.9–15.0 % and has been corrected.

In addition to the specific minor comments above, some grammatical changes were made to the text none of which changed the meaning. Some of these changes were in figures, but only to assure that mathematical units were in the form expected from HESS and nothing in the actual scientific content of the figures.