Review 1

Thank you for the review of our manuscript. We appreciate your constructive comments and suggestions. Please find our point-by-point response below (in blue). The line numbering in our response refers to the revised manuscript.
In general, we have identified the following main issues, which we have addressed below:

1) We found the method section related to a description of the HBV model (structure, routines, parameters) too brief and we have described it in more detail.
2) We have been more explicit about the data used for model calibration and simulation. In particular, we have clarified how the discharge, glacier mass balance and snow data have been used to model calibration and evaluation
3) We have better explained how the runoff components were calculated in the model and how to interpret the results related to the inter-annual variability of the components and explain the contradiction between the snowmelt which contributes most to the total runoff and glacier-melt which controls the inter-annual variability.
4) More emphasis has been placed on a literature review and discussion related to understanding the runoff process in polar areas and its impact on terrestrial ecosystems.

Overall, this manuscript is within the scope of the journal and adds some significant contributions to our understanding of the water budget and runoff processes on the Antarctic Peninsula. As I understand it, this paper is about quantifying the various contributions from snow, glacial melt, and rain to streamflow on the Antarctic Peninsula, and the relationships of total runoff and runoff contributions to climate variations. However, the title only mentions snow and glacial contributions but the paper also addresses rain contributions as well. There is also some confusion with the various study periods for the different model components as well as the field validations and measurements. The methods section needs more detail on the model: how the model uses the input data and how it estimates many of the simulated values, and also the field validation methods and what exactly was measured and how it was used. The discussion section also needs to be expanded in order to elaborate on the importance of the results and the implications for the ecosystems in this region and the effects of climate on runoff processes.

Specific comments

The title only mentions snow and glacial contributions but the paper also addresses rain contributions as well.

When formulating the research questions, we were interested mostly in snow and glacier contributions since the rainfall is rather minor. However, we agree that the original title might be confusing, so we have decided to change the title accordingly to “The role of snowmelt, glacier melt and rainfall in streamflow dynamics on James Ross Island, Antarctic Peninsula”

1. ABSTRACT AND INTRODUCTION:

Lines 13-14: It would be helpful to actually state what the total study period is (i.e. June 2010 – May 2021).

We agree that this change might improve readability. We reformulated the respective text to "We used the hydrological model HBV to simulate the runoff process from June 2010 to May 2021 at a daily resolution." on lines 14-15.

Lines 17: What does "strong glacier and snow melt" mean?

We found the word "strong" to be too vague and redundant in the sentence. We removed it from the text on line 18.

Lines 25: What are "high air temperatures in recent years"? Can this be quantified above average or above previous maximum temperatures?

We agree that the text is too vague. We rephrased it to "Although the warming of this region was interrupted in the early part of the 21st century (Turner et al., 2016; Oliva et al., 2017), the cooling period ended in the mid-2010s and (Carrasco et al., 2021) since then the region has experienced several warm events with record high temperatures (González-Herrero et al., 2022)." (lines 26-28).

Line 40: It may be helpful to also note that all of the precipitation in the dry valleys is in the form of snow (whereas the peninsula also receives some rain, analyzed in this paper).

We added the note to the text on lines 42-44: "In addition, this region receives a certain amount of precipitation in the form of rain, unlike the Dry Valleys where only snowfall was observed (Fountain et al., 2010).".

Line 55 and Line 62: It would be helpful to include some description of the terrestrial and marine environments in these regions in relation to streamflow and how they are affected by these runoff processes. What is the importance of understanding runoff processes for these ecosystems and environments?

The description of the impact of changes in terrestrial ecosystems on streamflow is stated in L30-35. In the revised version, we additionally rephrased the text along lines 57-59 to be more specific.

Lines 66: Again the study period of 2010/2011 – 2020/2021 is confusing. It may be helpful to state that streamflow only occurs during XX months, so the study covers the time period of June 2010 – May 2021 or something along those lines…

We agree and we reformulated the text on line 69.

1. METHODS:

Lines 86-87: How much of the precipitation is estimated to come from rainfall and from snow each year? Has this changed over time?

As previously stated on L106, numerical models represent the only available source of information about precipitation from a long-term perspective in the study area. However, data from three stations located west of the Antarctic Peninsula Carrasco and Cordero, (2020) indicate an increase in precipitation from 1970 to the early 1990s, followed by a decrease from 1991 to 1999. Additionally, an increase in snowfall and a decrease in rainfall were observed from the mid-1990s to the mid-2010s, resulting from the cooling period. We added this information to the text on lines 90-93.

Lines 70 and on… Some further discussion of the streams where discharge is being measured would be helpful. How much does the discharge vary from year to year? What is the average flow season? Is runoff only estimated at one location/stream?

Indeed, in such remote areas, the water level is not possible to measure continuously over longer periods. Further uncertainties arise from discharge calculation using rating curves which need to be created separately for each year due to the riverbed changes. Usually, the discharge data is only available from a few locations around the polar station and only during its operation (usually one to two months each year). For our study area of the Triangular catchment, the only available measurements cover the time period from February to March 2018 which was used as one of the variables for the HBV model calibration (see the respective lines 177-178). Therefore, neither the inter-annual variability nor the length of the discharge season are known. However, based on your comment, we decided to extend the discussion on the above topic on lines 347-355.

Line 99: Why was 1 June estimated as the start of the water year?

After a careful literature search, we are not aware of any systematically used definition of the hydrological year in this area. Therefore, we determined the beginning of the hydrological year based on our data. We have chosen June as the beginning because (based on our dataset) from that month onwards the number of days with precipitation and air temperature above 0 °C is negligible.

Line 99: Were air temp and total precip the only inputs for the model? How does it use those to estimate runoff and glacier mass balance?

The model inputs are formed by air temperature (T), precipitation (P) and potential evapotranspiration (PET; calculated using air temperature). Available data of glacier mass balance and runoff was used for model calibration. The whole procedure is described in section 2.3. However, we realized that the model description was not fully clear in the original manuscript and some important details were missing (e.g. the better description of snowmelt and glacier routines), although references to detailed model descriptions were provided in the original text. We significantly extended the methods section to better explain how the model deals with input data and how the individual water balance components are calculated. Therefore, we modified the whole section 2.3.

Lines 106: Was there any field validation for the simulated precipitation?

Yes, there was a field validation of the simulated precipitation. We added the lines 111-113 and 121-122.

Lines 116: Why was the runoff only calculated from Feb – Mar 2018? Was there no other data outside this period? How did this period relate to the rest of the study period?

Please also see our response to one of your similar comments above. There was no other measured discharge data in the study catchment outside this period. We are aware that this period represents only a small part of the entire study period and this was one of the reasons why we used glacier mass balance measurements for model calibration. As noted above we extended the discussion section on lines 347-355 related to the topic with a new paragraph (which also relates to your comment on lines 304-305 of the original manuscript).

Lines 120: What is the “glaciological method”?

We believe this is a well-established method and it is beyond the scope of this paper to describe it. We have therefore included a reference to a paper describing this method (line 127).

Lines 120-21: Why was surface mass balance only estimated for 2014-2020? These study periods don’t match up across the entire study and needs to be addressed.

The mass balance measurements were only carried out in 2014-2020. In combination with the volume measurements in 2006 and 2015, we were able to simulate the mass balance for the whole study period (L127-132). Please see also our answer to your comment about Lines 304 – 305.

Lines 125- 127: The section on snow depth needs more detail -- was snow depth only measured at one location? How was it extrapolated across the catchment? Is the snow on the glacier the only thing assumed to contribute to runoff? What about snow in the rest of the catchment? Was SWE/snow density measured as well?

Snow depth was only measured at one site. This data was not used to calibrate the model. It was only used as an additional check that the model simulated a realistic snow cover. No SWE measurements are available from this area. We modified the lines 133-137 accordingly. Additionally, we replaced “snow density” with “estimated SWE” throughout the manuscript.

Lines 132: model routines need to be defined. Were all of these routines used? Or was one chosen from these 5?

All of these routines were used since only together they can calculate the water propagation through the entire system and simulate the runoff in the outlet. As we noted in the response to one of your comments above, we realized that the model description, despite the referenced literature, was not fully clear in the original manuscript and some important details were missing. We have decided to extend the methods section 2.3 significantly to better explain how the model deals with input data and how the individual water balance components are calculated.

Lines 146: Why was this section not considered in the model?

Unfortunately, no measurements of the mass balance from this area are available. However, the small part of the ice cap is located in the highest part of the catchment. Therefore, we can assume that this part does not contribute with glacier-melt water to runoff and thus can be technically considered as “glacier-free” in the model (which contributes only by occasional snowmelt to the total runoff as explained on lines 385-390). This assumption was later supported by model results where no glacier melt was simulated above approximately 350 m a.s.l. Besides reformulating the discussion section in this respect, we also better specified it in the methods section on lines 166-167.

Lines 155-165: The runoff is reported in mm, but I assume it was measured in some rate (i.e. l/s). How was this rate converted to mm and why? Was it summed up over the daily, monthly, yearly period?

Yes, the streamflow (in $m^3 s^{-1}$) was calculated from direct water level measurements using a rating curve. However, it is a common (and good) praxis in hydrology and hydrological modelling to report streamflow as runoff depth (the term "runoff" is frequently used), which is a water volume per unit area (usually catchment) for a defined period (daily, monthly, annual) or a long-term mean (therefore the units are mm per defined period). Among others, this approach enables the direct comparison of all measured and simulated water balance components (precipitation, PET, snow, glacier, soil and groundwater storage) available in the same units. We went throughout the whole text to ensure that all values and their sums (daily to annual or a mean) are always clearly reported and named by correct terminology.

1. RESULTS:

Lines 168 -171: These values could be put into a table to show the overall model fit.

As stated in the methods, we performed 100 calibration runs to address equifinality and increase the model robustness. The range of the objective function values was shown in the supplement as Fig. A1 in the original manuscript. However, we decided to show the figures in the main text as Figure 2 and thus bring them closer to readers to avoid jumping between the supplement and the main text. Therefore, we think the additional table showing the same is not needed.

Lines 176-177: Glacier mass balance simulation and observed should be included in Figure 2.

Similar to our response above, when writing the manuscript, we decided to move some of the plots showing the model performance to the supplement. However, we newly have merged Fig. A2 and Fig. 3, so the comparison of simulated and observed values is now in the results section.

Lines 179-180: There was never a mention of SWE or snow density measurements in the methods. How was simulated SWE compared to snow depth? How did the model estimate SWE and snow depth? This needs a lot more detail in the methods and results.

We refer to our response to your comment on Lines 125- 127 above. Additionally, we added the following text on lines 181-182: "During validation, the mean simulated SWE in the elevation zone, in which the weather station is located was compared to estimated SWE."

Figure 3: The cumulative line in (a) should be a color that is easier to see (or does this line really need to be there?). The axes also need more values, especially at 0 in (a) to see mass loss or gain.

We agree, we modified the image to make it easier to read.

Lines 197: the glacier did not melt at all? Might be more appropriate to say "did not lose mass" as there was likely still some melt generated on the glaciers even if they did not lose mass.

We rephrased the sentence as suggested (line 232).

Lines 198: Are these simulated SWE values? SWE measurements were never addressed in the methods section so I'm unsure if these values are from the model or measurements. If they are from the model, this needs to be added to the methods section of how the model calculates and estimates SWE.

These are simulated SWE values. We modified the method section on lines 136-137 to clearly state that measured SWE was only used for model evaluation. Please also see our previous response to your related comments above.

Lines 202: What does it mean "these months accounted for 32% and 25% of the total SWE loss"? This needs to be explained more and likely shown in a figure.

We agree that this specific result was not sufficiently described and might be confusing. We have modified the lines L236-237 and added table 2.

Lines 204: The simulated mean runoff was 415 mm? Should this be 415 mm per year?

As stated in the sentence, we refer to "mean annual runoff".

Lines 204-205: What were the instances when there was runoff outside of the main summer season? When did this winter runoff occur? This is brought up in the abstract and conclusion and needs much more attention in the results and discussion.

We agree that this particular result is not well addressed in the discussion section. We have added the following text to the discussion (lines 405-412) to clarify this issue: "According to Kavan et al. (2023) seven studies have been devoted to measuring runoff in the Antarctic Peninsula area between 1991 and 2020 (Inbar, 1995; Kavan et al., 2017; Sziło and Bialik, 2017; Falk et al., 2018; Stott and Convey, 2021; Kavan, 2021; Kavan et al., 2023). For most of the studies, the duration of the measurements did not exceed two months. Only one study (Stott and Convey, 2021) measured for 78 days. The measurements were mostly made during January and February, sometimes also in March. Only one study (Stott and Convey, 2021) covered most of December. However, no study took measurements during September, October, November or May. Therefore, our results, which include 11 years of simulated daily runoff, provide valuable information, although they are subject to the uncertainties of modelling based on limited amounts of data."

Lines 205: How was evapotranspiration estimated? This was never mentioned before in the methods.

The method of calculation of daily potential evapotranspiration is mentioned on lines 143-145. The actual evapotranspiration was calculated by the model as a function of simulated soil moisture and air temperature. We have specified it better on line 149.

Lines 206: It was never stated how much of the precipitation was estimated to come from snow vs. rain, or how the model partitions this. So how was the different contributions estimated between rain and snow in the model?

The HBV model is partitioning snowfall and rainfall using the concept of a single threshold temperature (T_T). The T_T is one of the model parameters calibrated by the model. Since the catchment is divided into elevation zones, the precipitation phase is then calculated separately for the specific elevation zone using a calibrated temperature lapse rate. Further, the model keeps track of whether the water comes from rain or snowfall (or glacier melt) throughout the other model components until leaving the catchment, assuming the complete mixing of water in individual components (so-called effect tracking; Weiler et al., 2018). Similar to your comments and our responses above, we included a better explanation of precipitation partitioning along with a more detailed description of the model structure and parameters in section 2.3, mainly on lines 153-173.

Figure 4: The axes should be colored for each of the values they are representing, it is confusing as is. Also, these should be split into two panels so that the various y axes don't overlap. Precip and temp should be panel A with only one y-axis on each side. Panel B below should be Q and cumulative Q.

We have modified the image to make it easier to read.

Figure 5: can these bars be stacked to add up to total Q instead of being side by side? That will make it easier to see how the allocation of various Q contributions changes.

Thank you for the suggestion. We tried to redesign the figure following the reviewer's suggestion, but after careful consideration, we came back to the original version which we believe is clearer and allows easier comparison between runoff components in individual months.

Figure 6: Similar to Fig. 5 these figures are confusing and have too many axis values. It would be beneficial to split into panels so that the axes labels do not overlap.

We have modified the image to make it easier to read.

Line 247: is this referring to snow cover on the glacier that melted out earlier?

It refers to the mean snow cover in the catchment including the snow cover on the glacier.

Line 276: I am confused how Qglacier can be the only one significantly correlated with Q total, and how it can have the dominant control over total runoff, but Qsnow dominates the proportion of total runoff (over 75%)? This needs more in depth analysis and thought into what this means in both the results and discussion section.

Of the three runoff components, annual $Q_{glacier}$ has the highest inter-annual variability (see newly added Table 4). This is because it is more dependent on annual temperature than Q_{snow} and Q_{rain}. For snow, as an example, it means that there is a similarly equal amount of snow in individual years, which results in low inter-annual variability of snow runoff contribution even though it is a dominant component of the total runoff. Therefore, the inter-annual variations of the total runoff are dominantly caused by inter-annual variations in glacier-melt runoff rather than snowmelt runoff.

We agree that the text in the original manuscript was not fully clear in this respect, therefore we have reformulated it on lines 319-321 and added new Table 4.

Figure 9: It is hard to see the solid vs dotted circles, may want to make the outlines bigger or different colors.

Thank you for the suggestion, we modified the image to make it easier to read.

1. DISCUSSION:

Lines 304 – 305, should address the discrepancies in study periods for all of the data and how this may affect the interpretation of results.

Thank you for the comment. Besides better clarification of the input data and time periods in the methods section (as mentioned above), we have added lines 347-355.

Lines 316: Where did these percip measurements come from? From the methods, I thought that there were no precip observations so this needs to be addressed earlier. Are there measurements of both rain and snow?

We agree that the respective section might be confusing. Therefore, we added the following text to the method section on lines 111-113: "Short-term precipitation measurements were taken in January and February 2022 at the Johann Gregor Mendel Station using a Thies laser precipitation monitor (disdrometer) and a manual rain gauge."

Lines 326: So SWE was measured? Needs to be brought up in methods.

Please see our answers to your comments on Lines 125-127 and Lines 179-180.

Lines 331: I thought this part was not considered in the model at all, but now it is saying that snow from this area was considered? Why is this? This needs to be explained more.

We rephrased the text in methods (lines 165-167) so it is clear that the highest part of the catchment is considered as glacier-free area in the model. Please, see also our response related to your comment on Line 146.

Lines 333 – 334: The statement "the glacier is covered with snow most of the year preventing any potential glacier melt" needs to be backed up. How does snow prevent glacier melt? Is there data to show that the glacier is actually covered with new snow most of the year? If there is no data or references to back this up than this statement needs to be removed.

Thank you for the comment. After reviewing our text we have decided to remove the respective sentence since it is not necessary.

Lines 349 – 350: What are the different definitions of the start year? Why was June 1 chosen for this study?

Thank you for the comment. After reviewing our text we decided to remove the respective sentence since it is not necessary.

Lines 351-355: Why is this statement worth mentioning? If the authors feel that this is important to mention then this needs to be explained and elaborated on further here.

We agree that this sentence is not necessary. We removed it.

Lines 364-365: It is also important to note that the flow season for streams in the McMurdo Dry Valleys is much shorter than on the Peninsula, and most of the streamflow comes from glacial melt.

Thank you for the suggestion, we reformulated the text (lines 423-425).

Lines 370: AP?

We changed it to “Antarctic Peninsula”.

Lines 371 -372: The statement “this in turn may cause water shortages and affect local freshwater ecosystems” can be explained much more. How does water availability in these areas affect the ecosystems? Why is it important to study the runoff regimes of these areas for the environment and ecosystems? This needs references and elaboration to back it up.

Thank you for the comment. We reformulated the whole section in lines 434-444.

Lines 373-374: How does rising temperatures influence suspended sediment transport? And what does this mean for this study? This section of the discussion needs to be expanded on quite a bit for the broader environmental implications.

Thank you for the comment. We reformulated the whole section in lines 434-444.

Lines 376: Again, needs more expansion on the idea that higher amounts of rainfall can affect melting of snow and glaciers. How? And why is this important? Need references to back this idea up.

Thank you for the comment. We reformulated the whole section in lines 434-444.

CONCLUSIONS:

Line 391 – 392: the statement: “Together with mean annual evapotranspiration (7 mm) and mean annual precipitation (369 mm).” is not a complete sentence, maybe combine it with the previous sentence.

Thank you for pointing out that. We corrected the sentence on Line 461.

Lines 396: Again, what does “strong glacier and snow melt” mean?

We agree that the word “strong” is too vague and redundant in the sentence. We have removed it from the text (line 466).

Lines 398-399: as stated before, I am confused how Qglacier can be the only control on total annual Q, but Qsnow is the main contributor to Qtotal. This needs to be clarified in the discussion and if stated in the conclusion then it needs to be explained more.

Please see our response to your comment on Line 276. We reformulated the sentence for clarity (lines 468-469).

Lines 404-405: The presence of runoff-generating events outside of the summer season was not addressed in detail in the discussion section, and if it is going to be included in the conclusion and abstract it needs much more attention and analysis outside of the brief mention in results.

Please see our response to your comment on Lines 204-205. We have reformulated the sentence on line 475.

References

Carrasco, J. F. and Cordero, R.: Analyzing Precipitation Changes in the Northern Tip of the Antarctic Peninsula during the, 2020.

Weiler, M., Seibert, J., and Stahl, K.: Magic components—why quantifying rain, snowmelt, and icemelt in river discharge is not easy, Hydrol. Process., 32, 160–166, https://doi.org/10.1002/hyp.11361, 2018.

Review 2

Thank you for the review of our manuscript. We appreciate your constructive comments and suggestions. Please find our point-by-point response below (in blue). The line numbering in our response refers to the revised manuscript.

The manuscript is well written and good to publish in the journal. My main concern is about the degree-day factors used in the study. Therefore, my specific comments are as follows:

Line 132-133: It is good that the degree-day-based snow melt and glacier melt modules were used in the HBV model. It is necessary to mention whether the degree-day factors are model-calibrated or assigned values derived from the field measurement in the past. Since the degree-day factors play a significant role in the ablation estimation, it should be mentioned degree-day factors in this study. Moreover, sometimes model-generated degree-day factors may be unrealistic. Therefore, I suggest mentioning degree-day factors used in this study.

Based on this comment and also several others from the second reviewer we realized that the method section related to the model structure and parameters needs to be extended since a lot of information was missing in the original manuscript (despite references to other literature). This also applies to a better explanation of the snow routine of the model which is based on the degree-day method.

The degree-day factors both for snow and glaciers are one of the model parameters and thus they were calibrated. Before the calibration, the upper and lower limits have been applied to ensure that parameters are physically relevant. In the case of our study, the lower limit was set to 2 mm $°C^{-1}$ d^{-1} and the upper limit to 8 mm $°C^{-1}$ d^{-1}. However, after leaving the same range for non-glaciated and glaciated parts of the catchment, the model simulated high snow accumulations in the highest elevation zones of the catchment which did not completely melt in the season and thus created an unrealistic increase in snow storage over the study period (in hydrological modelling literature often referred to as “snow towers”). Therefore, we needed to do a fine-tuning of the degree-day factors separately for non-glaciated (median value resulting from 100 calibration runs was 6.01 mm $°C^{-1}$ d^{-1}) and glaciated parts of the catchment (median value equal to 2.23 mm $°C^{-1}$ d^{-1}) to achieve realistic snow storage simulations.

Additionally, we provided a simple evaluation of the simulated snow water equivalent and snow depth (measured automatically during a single summer season) as described in L208-211 and Figure 3 (previously placed as Fig. A1 in the original version). Despite the lack of data, the modelled SWE values correlated well with measured ones.

To better address all these issues described above, we reformulated the methods section related to the HBV model description (L 153-162) and respective discussion (L 378-382). Among others, we have included the calibrated degree-day factor values.

Line 347: It should be …. was also slightly negative in 2014/15, ……………..

Thank you, we have corrected the sentence on line 401.

Line 435: Missing the reference of Seibert and Vis (2012).

We are not sure whether we interpret this comment correctly since the referenced line (L435 of the original manuscript) represents the beginning of the references section. The mentioned reference is included in the reference list (L694).