**Response to comment from Reviewer 1**

Dear Dr. Newton,

Thank you for your constructive criticism and all your comments on the manuscript. Implementing the changes you requested will certainly improve the quality of the article and make it more accessible to the reader. I agree with all the comments made. Only point 5 of the review regarding the representativeness of climatological stations and the correlation between air temperature and ice cover occurrence (details below) I find debatable and in need of further discussion.

In this response, the reviewer's comments are highlighted in **bold** and the author's response is included under each. Due to the large number of revisions required to the text and figures, I have refrained from detailing in this response all the changes made to the manuscript. These will be added and presented in the final version of the manuscript sent after a period of public discussion (expected end 06 Jan 2024). Instead, the focus is on the substantive justification for these changes. If any of the answers are not clear or the issue raised requires additional clarification, kindly contact me.

**Major issues:**

**1) In the discussion I am not convinced it has explained what the dam has changed in the ice regime itself. Yes, the ice season has reduced, and it does appear that this might be associated with construction of the dams, but that in itself does not do enough in my view, particularly given the scale of impact that you state this paper might have. I think there needs to be a greater investigation into the potential causality of these changes. You outline some theoretical ideas, such as changes in discharge, but provide no evidence from the study site that could help to back this up. I strongly suspect you are correct in your hypothesis about what caused the impact, but it would make the paper significantly stronger if you could demonstrate some of this. Perhaps these data do not exist, but if they do, then I would encourage using these data to help make your point. If you have data such as Figure 9 for other time periods, then this would be a good place to start.**

I agree that the manuscript underrepresents the causes of the transformation of the river ice regime by the reservoirs studied. At this stage of the research, it seems that the main cause is the reservoirs' transformation of river water temperature (warming in winter) below their location. This is due to the presence of winter thermal stratification (catothermia) in the reservoir. The river is warmed by releasing bottom relatively warm (2-4 °C) waters from the reservoir. This results in a situation where the water flowing into the reservoir is close to overcooling (0.1-0.5 °C), while the temperature of the outflowing water reaches several degrees (even 4 °C). As a result, in a long section below the dam, ice phenomena do not form (or form in a limited form and specific conditions) due to the inability of phase transformation and secondary cooling of water.

Other factors may play a role (changes in the volume of river flow due to the operation of the dam reservoir, capture by the reservoir of mobile ice forms from higher parts of the catchment area, capture by the reservoir of suspended material), but the author's field experience suggests that they are of secondary importance. The effect suggested by the reviewer (in the comments on the manuscript attached in pdf) of an increase in the kinetic energy of turbulence below the reservoir on the ice cover may also have some significance. However, the Dunajec River

and the San River are mountainous rivers with complex gravel beds and high longitudinal gradients, in which flow turbulence and turbulence kinetic energy are large even without the influence of the reservoir, so analyzing the impact of this effect requires separate and detailed research.

A number of changes will be added to better present these issues in the article:

1. a broader theoretical description (based on more relevant literature) of the process of reservoirs' transformation of water temperature and river ice regime will be added to the Introduction chapter,
2. based on water temperature data available for the Dunajec River (stations C2 and C3), an analysis of the water temperature before and after the construction of the reservoir, downstream of its location, will be presented to demonstrate the impact of the reservoir in water temperature,
3. in addition, on 05.12.2023, measurements were made of changes in water temperature and the occurrence of ice cover in the longitudinal profile of the Dunajec River. The results of these measurements show a significant impact of the operation of the Czorsztyn-Sromowce reservoir complex on water temperature and will be included in the article. In addition, in the second half of December 2023 it is planned to conduct field research on the second of the rivers studied in the article (San). However, the conduct of the study depends on hydrometeorological conditions. If it is possible to conduct the surveys, the results will also be included in the article,
4. other possible causes due to lack of data relevant data will be indicated on the basis of literature. Research on the prioritization of individual factors determining the formation of ice cover on Carpathian rivers requires further detailed studies.

**2) There is clearly a difference in the ice cover trend before and after the dam construction (Figure 2 shows that), but these trends have been derived from just two time periods – before and after. I would like to see you try some moving averages of that trend, perhaps a moving window of ~10-15 years within the before and after time periods. I suspect this would help to demonstrate that it was not a gradual change, but a rather blunt one. This would help to make your point of the dam influence, though it still does not explain the causal mechanism.**

Figure two will be accompanied by graphs of annual totals of days with ice cover, where moving averages will be used.

**3) I would also like to see some greater statistical analyses of the climate change signal. This need not be complicated – e.g., some work can clearly be done on investigating the climatological trends in the weather station data and how these trends might relate to your ice regime changes. If there is no, or only a small, climate change signal in your weather stations, then this helps to make your point that it must have been the dam construction that impacted the ice regimes. This would be especially useful if data are not available to address point (1) above.**

I agree that the analysis of the climate signal, and the presentation of thermal conditions during the study period can improve the quality of the article, by adding additional context to the results obtained. Therefore, an analysis of trends in air temperature from climate stations analyzed using the Mann-Kendall test and the Theil-Sean

estimator will be added to the article. In addition, air temperature statistics by month for periods before and after the reservoirs were built will be provided.

**4) I would like to see a table that provides the geographical characteristics of the different water and weather stations, such as elevation. It would also be helpful to provide a comment on which weather stations are used to infer the air temperature at which water gauges – this is not currently as clear as it could be. This will provide a stronger link between the datasets.**

A table will be added to the article with information on the longitude, latitude and altitude of the stations used, as well as information on which climatological stations were used to model ice phenomena at each water gauge station.

**5) I would like to see greater discussion in the text about how reflective the author feels the weather stations are of the temperature that is likely to have been observed at the water gauges. For example, the lack of geographical information about the different data sites – e.g., elevation – means that I cannot be sure what elevation difference there might be between them, and this will certainly impact how representative those observations are likely to be. Maybe there is little difference in elevation, but this needs to be explained in more detail, and if there are major differences in elevation between the weather stations and the associated water gauges, then I would think that this needs to be taken into account when drawing up the correlations between temperature and ice presence. The strong correlations do suggest this might not be a significant factor, but I still think this needs to be properly accounted for, or at least discussed.**

A table will be added to the article with information on the longitude, latitude and altitude of the stations used, as well as information on which climatological stations were used to model ice phenomena at each water gauge station. In addition, a comment will be added regarding the representativeness of data from climatological stations in the context of modeling ice cover at hydrological stations.

In this context, I would like to point out that the logistic regression analysis carried out in the article is, to some extent, "robust" to the slight unrepresentativeness of data from climatological stations. This is due to the fact that in the process of building the model there is a learning stage, which involves the model determining how changes in the explanatory variable (average air temperature over 14 days) affect the presence or absence of ice cover. Even if the conditions at the climatological stations do not perfectly reflect the conditions at the water gauge cross sections, in the process of learning the data, the regression model will capture how the variation in air temperature at the climatological stations affects the occurrence of ice cover, inferring from the data itself. Also, the use of a parameter such as the average air temperature over 14 days made it possible to blur the local conditions found at each climatological station and characterize the overall conditions for a longer period.

In addition, I would like to point out that the article does not present a correlation analysis between the occurrence of ice cover and air temperature. While it is true that a correlated explanatory and explained variable is necessary to build a predictive model such as logistic regression, the article does not present a measure of correlation. To evaluate the model, the AUC ROC measure was used, describing not the correlation of variables, but the model's ability to classify binary ice cover occurrence on the basis of air temperature. Therefore, it is not possible to say how correlated these variables are based on the results presented.

**6) The impact of the work needs to be developed further. The start of your concluding point (1) is very important but given the above issues I am not sure you have presented a strong enough case to robustly**

**make this argument. If you can make the above revisions, then you certainly will have. There also needs to be some extra information on what this result would also mean for climate change studies – essentially that the trends of a declining river ice cover are not a de facto proof of climate change, and the wider hydrological setting needs to be taken into account in such settings. If you are able to make the revisions above you will be able to: 1) prove the dam influence is the case, and 2) prove that climate is not the main driver. This is a key selling point of the paper and if that case can be made more strongly, it will provide a good contribution to the literature.**

A broader discussion of what the obtained results mean in the context of research on river ice phenomena and climate variability will be added to the discussion and conclusions chapter.

**Minor issue:**

**1) In a large number of places in the text it is not always clear what is being referred to. The reader can often infer it, but it is better to be specific and unambiguous – e.g., instead of saying "their", "this" etc., state what it is you are referring to.**

The entire text will be corrected in this regard.

**2) I would encourage rewording of the locations from above/below the dam to upstream/downstream of the dam. This is much clearer language. I highlighted some instances, but not all of them.**

I agree that the use of upstream/downstream phrases is definitely clearer. The entire text will be corrected in this regard.

I also agree with all the reviewer's comments sent in pdf form. Appropriate corrections to the text will be made in the revised version of the manuscript.

Yours sincerely,
Maksymilian Fukś