Response to comment by Anonymous Referee #2

Thank you for the constructive review. Hereby, we would like to respond to your comments. In the following, the comments from the reviewer (AR#2) are in plain text, and our responses are in bold text. As the comments were embedded in an annotated version of the manuscript, we have extracted the comments and, where relevant, added line number and context in brackets.

General comment

AR#2: Please see my comments embedded in the attached annotated manuscript. This is a very nice paper. My suggestions are minor in nature and are meant to provide some small improvements to the paper to improve clarity for the reader and place the work in a broader context of the existing literature.

Response: Thank you for the positive comment.

Abstract

AR#2: [Line 6, re. controls on evaporation] Perhaps state temporal scale (i.e., seasonal).

Response: We will update the sentence to: "We found that ecosystem evaporation was indeed mainly controlled by atmospheric evaporative demand, both in the snow-free and the snow-covered season, and that spring snow-cover duration impacted total annual evaporation."

AR#2: [Line 7, re. soil moisture level] Be specific. "Never decreased to a level where it would restrict evaporation."

Response: We will change the sentence to: "Soil moisture remained high during the measurement period, and our results suggest it never decreased to a level where it would restrict evaporation".

Introduction

AR#2: This is a good well written paper. It could benefit from the results being put into the context of additional literature, notably from Canadian landscapes where several of these kinds of studies have been conducted. The authors already cite Nicholls, but could also do so from the perspective of environmental controls. Other authors/papers to consider would be Rouse, Leljedahl, Sonnentag, Lafleur and Spence. Here are a couple of examples to draw from: Rouse et al. 2000. Phys Geog. 21: 345-367, Liljedahl et al. 2011. Biogeosciences 8: 3375-3389, Warren et al. 2018. Ecohydrology 11: e1975, Lafleur and Rouse, 1988. Boundary Layer Meteorology 44: 327-347., Spence and Rouse, 2002. J. Hydromet. 3: 208-2018.

Response: Thank you for the suggested literature. We found the suggested papers of Warren et al. (2018), Spence and Rouse (2002) particularly relevant and will discuss our results in the context of these studies. The paper of Liljedahl et al. (2011) was already included in the preprint (sections 1 and 4.1).

AR#2: [Line 19] Not necessarily so in some continental locations (NW Canada).

Response: We will rewrite the sentence to include exceptions, citing references such as Wang et al. 2013. Hydrol. Earth Syst. Sci., 17: 3561–3575. In addition, we will search for literature showing exceptions in other northern latitude areas.

Methods

AR#2: [Line 96] was

Response: Thank you. We will change the tense, from *is* to *was*.

AR#2: [Line 119] Maybe introduce the term Eobs in this section somewhere so that the reader knows what is being compared to Epm when that is introduced below.

Response: We agree, however, since the term E_{obs} is used to describe observed evaporation only in the context of calculating the relative error, and *observed evaporation* is used elsewhere in the text and figures, we feel it could confuse more than it clarifies.

AR#2: [Line 158, re. soil heat flux] This is an important term as it is used to derive available energy. But its description is a bit lacking. From Equation 2, it seems as if SHF = G. More detail is needed of the estimate of SHF.

Response: Thank you for pointing this out. We suggest adding the following to section 2.2.2 Measured ancillary local data. "To estimate the soil heat flux (SHF) in the surface energy balance (equation 2), we used measurements from soil heat flux plates (Huskeflux), available at each site. Further details about measured ancillary local data can be found in Pirk et al. 2023 (Finse), Pirk et al. 2024 (Iskoras), Pirk et al. 2017 (Adventdalen) and Bekken et al. Carbon dynamics of a controlled peatland rewetting experiment in the Norwegian boreal zone *Scientific Reports* (in press) (Hisåsen).

Discussion

AR#2: [Line 380, regarding level of soil moisture] See comment above.

Response: We will change the sentence to: "Therefore, it is likely that the soil moisture content did not decrease to a level where it would restrict evaporation in the measurement periods."

AR#2: [Line 398] Including a table of energy balance closure early on in the Results would be a good idea.

Response: We agree and will include it.

AR#2: [Line 425-428] While these are good points; I might argue that since available energy (driven much by incoming solar radiation) is a major control and the sites were not moisture limited, the seasonal ET rates are energy limited - which makes sense given that the authors found available energy to be very important. These sites evapotranspire at their maximum rates bounded by energy, and there was not much variation among the sites - or at least three of them. So precipitation controls the ET/P ratio. I would not expect the ET/P ratio to be the same among the sites. Maybe it would be best to say this more explicitly.

Response: We agree and suggest adding the following sentence in line 425: "The evaporation ratio is thus mainly controlled by precipitation."