This preprint presents an analysis of snow cover trends in the Greek mountains, a region where knowledge of the cryosphere remains limited. The manuscript reads very well. The methods and analyses are well illustrated, and the conclusions are clearly outlined. The code developed in this study should be readily transferable to other regions. Overall, I believe this is an original and important contribution.

Main comments

The paper has a double objective: (1) to introduce a new algorithm for snow cover area reconstruction, and (2) to present an original analysis of snow cover in the Greek mountains. The introduction is not focused specifically on snow cover in Greece, which is a good choice since it broadens the potential applications of the SnowMapper algorithm. However, it lacks a review of previous studies on snow cover in Greece, which is important for contextualizing the second part of the paper dedicated to snow cover trends in this region. In particular, the authors could better position their analysis with respect to the previous work of Masloumidis et al. (2025). I am also somewhat surprised that there appear to be no other relevant studies on this topic.

We will discuss further the work done by Masloumidis et al. (2025), as well as any other relevant regional/global-scale studies, in order to better contextualize the climatology part of the manuscript. Unfortunately, the available literature on this topic in Greece is extremely limited (Fayad et al., 2017), highlighting the extent of the current knowledge gap, the relevance of the present study, and the need for more studies like it.

Fayad, A., Gascoin, S., Faour, G., López-Moreno, J. I., Drapeau, L., Page, M. L., and Escadafal, R.: Snow hydrology in Mediterranean mountain regions: A review, Journal of Hydrology, 551, 374–396, https://doi.org/10.1016/j.jhydrol.2017.05.063, 2017.

As acknowledged by the authors, a key challenge is the lack of satellite data prior to the 2000s. I believe the results would be strengthened if the authors tested the robustness of their trend analysis with respect to the increasing availability of satellite observations (Bayle et al., 2024). For example, the trend analysis could be repeated without the assimilation of satellite data. Another option (potentially better, but more difficult to implement) would be to limit the annual number of assimilated satellite observations to a constant value. At least, this issue should be discussed.

We acknowledge the need for further deliberation on this issue, which we will do in our discussion section. While testing the sensitivity of trend analysis to the availability of imagery is important when trends are relying purely on imagery, we do not think it would be particularly informative in our case. The reason is that our trend analysis does not depend on the irregularly captured and irregularly spaced satellite imagery, but rather on monthly aggregates derived from reconstructed daily snow cover maps. The main purpose of satellite observations in our model is to 'course-correct' the simulated snow cover and limit the propagation of gap-filling errors, in time. Furthermore, based on our evaluation of snowMapper's performance, it appears mainly to underestimate snow cover, rather than overestimate it, and this has remained fairly consistent over the four decades. In fact, this underestimation is more evident in the first two decades of the reconstruction, which, if anything, would then tend to underestimate our negative snowcover trends over the full period.

As noted by Reviewer 1, the calculation of the monthly SCA should be clarified: "Daily binary snow cover is converted to monthly fractional snow cover (FSC).". This step involves both temporal and spatial aggregation. I assume that the authors computed the monthly average of the daily snow cover fraction within each mountain polygon. In that case, it is possible to obtain identical FSC values from very different snow conditions. For example, a constant 10% snow cover throughout an entire month would yield the same FSC as a short-lived 100% snow cover lasting only three days (assuming a 30-day month).
10% SCA × 30 days / 30 days per month = 10% / month
100% SCA × 3 days / 30 days per month = 10% / month
If my understanding is correct, this limitation should be acknowledged and discussed.

Your understanding and examples are indeed correct. As you point out, this simplification of a spatiotemporally distributed dataset is a limitation, necessary to obtain a monthly spatiotemporal FSC. However, we believe that the relevance of this with respect to the climatological analysis is limited. While several different combinations of (a) daily snow cover extent and (b) its associated duration within a month, can result in the same monthly spatiotemporal FSC, this metric encapsulates that information. Ultimately, as described in more hydrologically-focused studies, the value of the snowpack is a relationship of "how much" water is stored as snow, and "how long" it stays stored in that frozen form (Aragon & Hill, 2024). The spatiotemporal FSC, in a way, does exactly that, informing on both the extent of the snow cover, and its duration.

Aragon, C. M. and Hill, D. F.: Changing snow water storage in natural snow reservoirs, Hydrology and Earth System Sciences, 28, 781–800, https://doi.org/10.5194/hess-28-781-2024, 2024.

Minor comments
In their literature review, the authors may also consider the study by Zakeri and Mariethoz (2024).

Thank you for bringing this study to our attention; we will reference it.

L169: Is this the full MicroMet algorithm, including the Barnes objective analysis scheme?

The quick answer is no. Apart from this being inherently complicated and computationally expensive within the GEE environment, it is our understanding that the Barnes objective analysis scheme is most useful when the MicroMet algorithm is applied to interpolate station data 'randomly' located in an area, rather than a uniform grid of points extracted from a reanalysis product. In our case, we perform a nearest neighbor resampling of the original grid to the desired spatial resolution, and we then correct the meteorological values using the respective MicroMet equations and lapse rates/correction factors for temperature and precipitation (Liston & Elder, 2006). We will include this information in the manuscript.

Liston, G. E. and Elder, K.: A Meteorological Distribution System for High-Resolution Terrestrial Modeling (MicroMet), Journal of Hydrometeorology, 7, 217–234, https://doi.org/10.1175/JHM486.1, 2006.

L366: typo (parentheses)

Thank you – in those brackets we will add the missing legend item "standard deviations (shading)".

L399: I wonder if the significance in the "increase in the frequency of extremely low snow cover instances" is influenced by the temporal autocorrelation in the negative anomalies timeseries (i.e. a negative anomaly in April is more likely if there was a negative anomaly in March). I am not an expert in trend analysis but I suspect that this could inflate the trend significance?

We will explore this further and provide a more complete answer in the revised manuscript.

References

Bayle, A., Gascoin, S., Berner, L. T., and Choler, P.: Landsat-based greening trends in alpine ecosystems are inflated by multidecadal increases in summer observations, Ecography, e07394, https://doi.org/10.1111/ecog.07394, 2024.
Masloumidis, I., Dafis, S., Kyros, G., Lagouvardos, K., and Kotroni, V.: Snow Cover and Depth Climatology and Trends in Greece, Climate, 13, https://doi.org/10.3390/cli13020034, 2025.
Zakeri, F. and Mariethoz, G.: Synthesizing long-term satellite imagery consistent with climate data: Application to daily snow cover, Remote Sens. Environ., 300, 113877, https://doi.org/10.1016/j.rse.2023.113877, 2024.

Thank you very much for supporting the improvement of our paper! All your comments are very valuable.