The authors well updated the manuscript. The main subject of this study is to show that water vapor transport is an important process shaping the vertical snow density profile in both tundra and forest-dominated areas and to test the performance of the snow model Crocus and explore the adjustments, that is clearly stated in the main text. I understood that the scientific originality is to present the snowpack structure and thermal properties in **both tundra and boreal forest** which no previous study addressed yet. Following these updates, I tried to check the manuscript focusing on the points strongly related to the main subject. However, some concerns that were pointed out in the previous round of review were not significantly solved.

First, the logical flow to conclude "predominance of water vapor fluxes for vertical density/thermal conductivity profile" is still insignificant. According to L422–425, we guess the authors reached the conclusion based on two results: (1) a significant fraction of depth hoar which is a remarkable characteristic indicating active water vapor flux, and (2) the snow density profile increasing towards the snow surface which should be decreasing towards the surface based on the compression process only. However, the strong wind densifies the snow surface, which coincides with high thermal conductivity (*wind packing*). The density of precipitation particles under the strong wind itself is also high (e.g. ρ_n=418 kg m-2 with T_a=263.15 K and W_s=4 m/s using Eq. 2 above the vegetation height, which is comparable to the observation). These natural processes and the stabilizing effect of shrubs can also cause the vertical density profile which is different from the expected one based on the compaction process only (the result by the default Crocus). The "predominance" should be argued after the order of importance in things is clarified. The authors should address each dependency of the process (compression, water vapor transport, stabilizing effect, and blowing snow effect) on the density profile quantitatively. Without this kind of quantitative discussion, it is difficult for me to understand that the water vapor transport **overcomes** the effect of the compression process.

Second, the robustness of density/thermal conductivity profiles observed is not enough demonstrated. The problem that the thermal conductivity observation using TP02/TP08 heated needle probes gave a large error was well discussed in the revised manuscript (though a small mistake was found; see a specific comment). I understand that the error would not be a problem in terms of average. However, the profiles were largely variated (Figs. 6 and 7). Whereas it seems that one profile increased towards the surface while another decreased, it is very difficult to see the true variability from a spaghetti plot. The authors should show the confidence interval (or probability density function) or perform a statistic test for the vertical tendency of the profile. Then, the robustness of the vertical tendency of the profile should be discussed. Even though the authors showed comparable profiles between the mean and median in "tc-2022-19-author_response-version2.pdf", that is only a piece of evidence that the probability density function might be assumed as the normal distribution.

Third, much evidence for coefficients of the adjustments (Eq. 1–4) is not still disclosed even though exploring adjustments is one of the study subjects. This is an important point in terms of scientific reproducibility, too. The authors should appropriately show the methods and results of preliminary simulations (L137–138, L152–153, L167–168) and discuss the improvements from the original adjustments by Barrere et al. (2017), Gouttevin et al. (2018), and Royer et al. (2021b). Although the authors were concerned as "Including an analysis would give the impression that we recommend the used parameters for other sites and/or models", that is not a problem since the authors appropriately made remarks about it at L413–420. Moreover, I guess that the coefficients a_ρ, b_ρ, c_ρ, a, b, and c are intrinsically obtained based on the multivariable model with a very large degree of freedom: the density and snow height are outcome variables and a_ρ, b_ρ, c_ρ, a, b, and c are explanatory variables. Eq. (1) also affects the selection of coefficients. Since this seems very complex model to obtain the coefficients, the authors need to describe the procedure carefully.

Specific comments:

1. L46–48: This sentence should be revised as "These models have been developed for alpine applications, where the dominant process that controls the density profile in the models is the compaction that results from overburden."
2. L48–50: This parameterization is ad-hoc to fit the calculation result with the observation as the authors say in "tc-2022-19-author_response-version2.pdf". The water vapor transport has not been directly implemented. Please emphasize this point here and revise this sentence appropriately.
3. Fig. 1: Please describe what a dotted line of the figure indicates in the caption.
4. L108–116: First, you indicate the blowing snow effects consist of snow erosion and accumulation (L109). However, after that, you say sublimation and wind packing effect are implemented in Crocus to simulate blowing snow (L110, L115). This is very confusing because the sublimation and wind packing effects are strictly different from erosion and accumulation, respectively. Please revise the wordings appropriately.
5. L128–143: The terminology of *Snowfall* is very confusing. According to L141–143, *Snowfall* includes densification effects of the wind, which associate with *Blowing snow*. Please replace it with *Snowfall density* or something appropriate word. Moreover, the densification effects of the wind have been already considered in Eq. (1) as a wind packing effect. Is this an appropriate procedure? Please clarify it in the main text.
6. L137–138: It is very hard to understand how the authors obtain a_ρ by varying c_ρ to be comparable density between the simulation and observation. Please describe the procedure

more!

7. L156: The terminology of *Blowing snow* is confusing because this includes snow erosion, accumulation, and wind packing until this sentence. Please replace it with *Snow redistribution* or something appropriate words.
8. L163: Please delete “or remove” from this sentence. This is confusing because this study does not remove snow using Eq. (4).
9. Eq. (4): You can replace “W_s” with “min(W_s, 10)”.
10. L173–174: Please delete this sentence. This is confusing because this study does not remove snow using Eq. (4).
11. L182: Please replace “ERA5” with “the closest grid point data of ERA5 to the study site”.
12. L185: Please replace “rain and snow” with “liquid and solid”.
13. L188: Typo.
14. L236: Why do you need to simplify stratigraphy? Fig. 5 and Supplementary figure 2 are very similar.
15. L244: However, melt-freeze forms are depicted at the bottom in Supplementary figure2. Please make sure if there is no mistake.
16. L246: However, no precipitation particles are depicted in Supplementary figure 2. Please make sure if there is no mistake.
17. L252: Please revise as “…, every measurement height was divided by the height of the respective snow cover.”
18. L254: However, the variability among the measurements is very large. Is the profile really different between TUNDRA and FOREST significantly?
19. L327: I feel like there is something a little bit off with this sentence because the adjustments were ad-hoc procedures to fit the calculation result with the observation. Is it appropriate to say “the adjusted version **does reproduce** the density profile …”? Please reconsider the terminology.
20. L351: Snow amount at TUNDRA would be expected to be reduced by a strong wind. However, Crocus simulation, which ignores the snow erosion, relatively simulates snow height (Fig. 9) and snow water equivalent (Supplementary figure 1). How do you interpret this result? Please describe it in the main text.
21. L354–357: Do you have some cases when the depth hoar was not or weakly developed with much snow amount? In such cases, if this hypothesis was true, the difference between the default Crocus simulation and the observation should be smaller.
22. L380: Does “the uncertainty” indicate the standard error? If so, “n-1” should be correctly “n-1/2”. Please make sure if there is no mistake.
23. L403–404: So, did your implementation, not taking the whole vegetation height as a zone

where compaction is reduced, effectively improve the simulation score? This can easily demonstrate quantitively through the preliminary sensitivity analysis (L153). Even though you responded to my comment as “we did not focus on a quantitative assessment…” in “tc-2022-19-author_response-version2.pdf”, the quantitative assessment is necessary to demonstrate the **predominance** of water vapor transport.

24. Supplementary figure1: This should move to the main text.
25. Supplementary figure2: No legend for “/”.