We would like to thank the referee for their careful examination of our work and the insightful suggestions they have provided. To address these comments, we have carried out several supplementary analyses, as described in the detailed response below. We believe the revised manuscript will satisfactorily address their concerns.

Note : During the revision process we identified and corrected two minor issues:

1. A coding error in the conversion from NDSI to FSC which only affected NDSI values below 0.1 in resampled NASA products.
2. Three corrupted products in our VNP10A1 dataset, corresponding to tile h18v04 from December 3–5, 2023. These products correspond to clear-sky acquisitions following a major snowfall event affecting a large number of relevant pixel-to-pixel correspondences

As a result, the updated metrics show minor differences, such as a slightly higher overall RMSE and a slightly more positive bias. We confirm that this update does not affect any of the conclusions of the work. Hereby, we compare Table 3 of the original manuscript to its updated version.

Tab. 3 of the original manuscript

Product	N pixels	% Snow Cover	F1-score	Commission Error	Omission Error	Bias [%]	RMSE [%]
VNP10A1	4.33+06	17.7	0.93	0.01	0.07	0.07	9.82
VJ110A1	4.45+06	17.9	0.93	0.02	0.07	0.35	10.81

Tab. 3 after corrections

Product	N pixels	% Snow Cover	F1-score	Commission Error	Omission Error	Bias [%]	RMSE [%]
VNP10A1	4.48+06	18.7	0.93	0.02	0.06	0.42	10.09
VJ110A1	4.45+06	17.9	0.92	0.02	0.06	0.81	10.88

We propose to update all tables and figures of the manuscript to integrate these two corrections. The numerical results reported in this reply will include these corrections.

Hereafter, the point-by-point response. In italic, we report the comments of the referee.

Section 5.1: Table 3 suggests that the choice of cloud mask has an effect on product performance that is comparable to that of the retrieval algorithm itself. The influence of the cloud mask on snow retrieval is therefore an important consideration, and it is closely tied to the well-known problem of cloud/snow discrimination. Since the VIIRS products evaluated here originate from different sources and apply different cloud masks, and since the present study does not explicitly isolate the effect of the cloud mask, this represents a limitation of the analysis. Where feasible, I would encourage the authors to compare and evaluate the accuracy of the cloud masks across the *different product sources and to assess their influence on the computed snow cover area. At a minimum, this issue should be discussed.*

We agree with the referee that cloud masking is of paramount importance in a snow cover retrieval algorithm Here, we used the union of the cloud masks of reference and target datasets to limit the influence of cloud masking on the performance assessment. Cloud masking was considered during this study. For example, we tested some configurations of Météo-France cloud masking algorithm without finding relevant differences. However, a comprehensive investigation of cloud masking and how it affects snow detection accuracy require careful design and additional analysis that is well beyond the scope of this study. In particular, such a study would require a reference cloud mask that is not available. We can revise the manuscript to emphasize this limitation of our study.

Section 5.3: The multi-platform compositing clearly improves data availability, and the reported reductions in cloud cover are substantial. However, given that the three platforms share the same orbital plane with overpasses only about 50 minutes apart, it would be helpful to disentangle the mechanisms responsible for this improvement. Specifically, how much of the gain arises from cloud advection over the roughly 50-minute window, and how much from differing viewing geometries or other factors?

We thank the referee for this insightful comment. In the manuscript, we attributed the improvement to cloud advection but a dedicated analysis would be needed to disentangle the contributions of cloud advection, differing viewing geometries, and other potential mechanisms. For cloud advection, this would involve tracking cloud feature displacement between consecutive overpasses (50 minutes apart) using wind speed data to estimate expected movement. As shown by Li et al., 2025, wind speeds are typically on the order of 10 m/s, corresponding to a displacement of ~30 km between passes. For viewing geometry, this would require comparing cloud cover statistics across platforms while accounting for differences in solar and viewing angles. The VIIRS cloud shadow detection algorithm is based on the propagation of the line of sight (Huchinson et al., 2009); the shadow projected by a cloud is proportional to its height. We can suppose this to be of the same order of magnitude as cloud height (5–10 km), which is confirmed by Fig. 3 of (Hutchinson et al., 2009), where cloud shadows are on the order of 10 km. Based on this preliminary estimate, we hypothesize that cloud advection is the dominant mechanism. However, under certain conditions, the contribution due to viewing and solar geometry may be more significant. To conclude, a detailed attribution analysis would require additional data (e.g., high-temporal-resolution atmospheric fields) and modeling (e.g. radiative transfer) that falls beyond the scope of our current study. We can revise the manuscript to explicitly acknowledge this limitation and discuss how both mechanisms likely contribute to the observed improvements.

A more important concern relates to Figure 5 and the comparability of the products shown. Section 3.1 states that NASA does not currently distribute a JPSS-2 product, yet Figure 5 presents single-platform series labelled MF-FSC-VNP-L3, MF-FSC-VJ1-L3, and MF-FSC-VJ2-L3. As I understand it, these single-platform series are all derived from the Meteo-France pipeline using one platform at a time, and are therefore not the same products (VNP10A1, VJ110A1) evaluated in Section 5.1. I suggest that the authors clarify explicitly how comparability is ensured between the single-platform and multi-platform composites, and between these and the NASA products evaluated elsewhere.

We thank the referee for his comment and acknowledge that, given the multiple processing steps involved, the manuscript can be hard to follow when it comes to understand which products are compared and why. We propose to add a table in the revised manuscript to provide a synthetic overview of the assessed products and how they compare in terms of satellite platform and processing pipelines. We provide an example of such a table for one product (to be filled with each of the evaluated products).

Product ID	Sensor	Platform	Pipeline	Reference	Detailed analysis
VNP10A1	VIIRS	SNPP	NASA	(Riggs et Hall, 2023)	Yes

Section 6.1: The statement that Landsat-based products "are only available in the United States" is inaccurate, as Landsat provides global coverage. I suspect the authors intend to say that the specific high-resolution reference datasets or studies they cite are concentrated in the United States. The sentence should be reworded accordingly. In addition, since Landsat data are in fact globally available, the authors should explain why Landsat-based reference data were not used in this study

In Section 6.1, we state that using Sentinel-2 FSC operational products is a scalable and efficient method for generating reference data for moderate-resolution sensor assessment. We stress the fact that, unlike most studies, we use directly operational FSC products as reference rather than deriving snow cover from reflectance. This allows us to avoid an additional pipeline and quality evaluation for ground truth data processing. We apologize for any misunderstanding caused by the lack of clarity and propose to reformulate Section 6.1 to explain our position.

Section 6.2: I have a substantive concern with the MOD10A1 versus VNP10A1 comparison. Terra (MOD10A1) is a morning-orbit platform (descending node around 10:30), whereas VIIRS daytime overpasses occur in the early afternoon (around 13:30), as does Aqua (MYD10A1). On the grounds of overpass-time matching, VNP10A1 is more appropriately compared with MYD10A1. The comparison with MOD10A1 remains informative but should be framed accordingly. This bears directly on the interpretation in the manuscript. The authors attribute the finding that MOD10A1 yields more snow than VNP10A1 to a less restrictive cloud mask. This attribution is likely incomplete. Because the morning overpass precedes much of the intraday accumulation of positive degree-hours, the morning platform may simply observe more snow than the early-afternoon platform owing to within-day melt, independent of cloud masking. Notably, the authors themselves observe that studies reporting the opposite result used MYD10A1, which indicates they are aware ofthe Terra/Aqua distinction. The same overpass-time effect is discussed in the China-based VIIRS evaluations they cite. I recommend that the authors add a comparison with MYD10A1.

The referee's comment made us further investigate this point. We modified Figure 6 to show the evolution of the cloud-covered area, and we added the Aqua product MYD10A1 to every panel to allow a comparison with Terra’s MOD10A1. We attach the result of such a comparison, adding Aqua to Fig. 6 of the original manuscript (filename fig06_aqua.png).

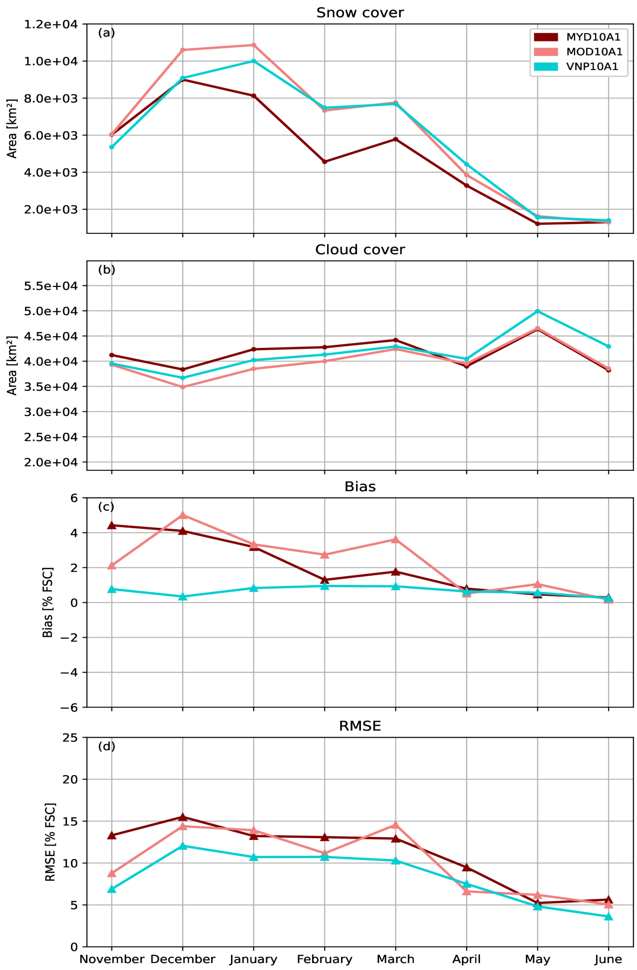

Figure 1: Per-month distribution of: (a) average daily snow cover area, (b) average daily cloud cover area, (c) bias with respect to Sentinel-2 and (d) RMSE with respect to Sentinel-2

We observe (panel b) that more clouds are detected in VNP10A1, except for the month of December where there is a notable difference in snow-covered area. This means that our interpretation that the larger snow cover area of MODIS (Terra) is due to a less restrictive cloud mask is indeed insufficient and will be removed from the manuscript. Furthermore, MYD10A1 (Aqua, 13:30) yields less snow and more cloud cover than both VNP10A1 (VIIRS SNPP, 13:30) and MOD10A1 (Terra, 10:30) from December to March (panels a and b). This comparison confirms the results of Thapa et al., 2019 and Liu et al., 2022 in North America and China, respectively. It also suggests that the overpass time is not the main driver of the differences in snow-covered area. Finally, in terms of bias and RMSE, the behavior of Aqua is more similar to Terra than to SNPP. This finding strengthens our discussion, confirming the need to examine systematic differences if MODIS and VIIRS products are to be merged into a long-term record, and showing that NASA's VIIRS products slightly outperforms their MODIS counterparts in terms of RMSE. We propose to follow the referee's suggestion, i.e., to add MYD10A1 in the comparison, and adjust the rest of the manuscript accordingly.

Section 6.4: As the authors themselves demonstrate in Figure 7, the NDSI-FSC relationship differs markedly between forested and open areas. Yet the methodology (Section 4.1) applies a single linear relationship to convert NDSI to FSC everywhere. Applying one relationship across land *cover types necessarily introduces a systematic error in forested pixels. I encourage the authors to strengthen the associated uncertainty analysis, for example by fitting separate NDSI-FSC relationships for forested and open terrain (or finer land-cover classes) and reporting the resulting change in accuracy. This would substantiate the discussion in Section 6.4.*

We thank the referee for this insightful remark and have performed a test to assess the proposition. We compared VNP10A1 FSC derived using the (Salomonson and Appel, 2006) fit (as used elsewhere in the manuscript) with a version obtained using the fit—different for open areas and forests—developed in this work (Fig. 7). We illustrate the differences by reporting Table 3 (for VNP10A1) and 4 of the manuscript in the two cases. Please note that the results have minor differences from the original manuscript for the reasons stated at the beginning of this document.

Tab. 3 (VNP10A1) and 4 of the manuscript, NDSI to FSC conversion with (Salomonson and Appel, 2006)

Product	**N pixels**	**% Snow Cover**	**F1-score**	**Commission Error**	**Omission Error**	**Bias [%]**	**RMSE [%]**
VNP10A1	4.48+06	18,7	0.93	0.02	0.06	0.42	10.09

Reference FSC	**Landcover**	**Aspect**	**N pixels**	**% Snow Cover**	**F1-score**	**Commission Error**	**Omission Error**	**Bias [%]**	**RMSE [%]**
1-99 %	Forest	N	4.99e+04	39.58	0.71	0.20	0.28	-1.38	29.72
1-99 %	Forest	S	1.28e+04	43.83	0.80	0.15	0.20	-1.86	22.97
1-99 %	open	N	6.58e+04	68.07	0.90	0.17	0.12	-1.74	23.06
1-99 %	open	S	5.76e+04	65.63	0.93	0.17	0.05	3.31	18.01
0-100 %	Forest	N	3.18e+05	8.41	0.76	0.02	0.24	0.42	13.63
0-100 %	Forest	S	2.02e+05	4.32	0.87	0.01	0.14	-0.13	6.42
0-100 %	open	N	2.44e+05	36.10	0.95	0.02	0.07	-0.64	13.00
0-100 %	open	S	3.69e+05	18.27	0.96	0.01	0.03	0.55	7.43

Tab. 3 (VNP10A1) and 4 of the manuscript, NDSI to FSC conversion with the fit found in this work:

Open areas : FSC = 1,43 NDSI – 0,04
Forest areas : FSC = 2,17 NDSI – 0,13

Product	N pixels	% Snow Cover	F1-score	Commission Error	Omission Error	Bias [%]	RMSE [%]
VNP10A1	4.48+06	18,7	0.93	0.02	0.06	0.51	10.81

Reference FSC	Landcover	Aspect	N pixels	% Snow Cover	F1-score	Commission Error	Omission Error	Bias [%]	RMSE [%]
1-99 \%	Forest	N	4.99e+04	39.58	0.71	0.27	0.23	4.89	33.93
1-99 \%	Forest	S	1.28e+04	43.83	0.81	0.20	0.14	4.88	26.14
1-99 \%	open	N	6.58e+04	68.07	0.90	0.15	0.13	-3.96	23.32
1-99 \%	open	S	5.76e+04	65.63	0.93	0.16	0.05	1.27	17.69
0-100 \%	Forest	N	3.18e+05	8.41	0.75	0.03	0.20	1.43	15.23
0-100 \%	Forest	S	2.02e+05	4.32	0.87	0.01	0.09	0.38	7.02
0-100 \%	open	N	2.44e+05	36.10	0.94	0.02	0.07	-1.41	13.19
0-100 \%	open	S	3.69e+05	18.27	0.96	0.01	0.03	0.16	7.32

These results indicate that our linear fit slightly degrades overall performance with respect to the universal linear fit. The RMSE obtained with the two formulas is comparable in open areas, while it degrades in the forest when using the linear model estimated in this work. The bias also increases in forested areas when applying our fit. This highlights the difficulty in developing a predictive model for NDSI-to-FSC conversion, which performs well over large areas and in different seasons. We propose to add these considerations to the manuscript.

Section 6.5: Topography exerts a strong influence on moderate-resolution snow retrievals, particularly in high mountains, and the discussion in this section is somewhat brief relative to the importance of the effect. The manuscript's own results, such as omissions on northern slopes and declining performance with increasing slope, are consistent with prior scale-effect studies of moderate-resolution snow retrieval (e.g., work over the Tibetan Plateau) that demonstrate a strong dependence of retrieval accuracy on slope and aspect. Importantly, such studies also show that the locally optimal NDSI threshold varies with slope and aspect because of illumination conditions, which directly challenges the use of a single, fixed NDSI-FSC relationship and threshold in this study. This connection, and its implications for the fixed-threshold approach, should at least be discussed

We agree with the referee on the importance of topography and the suboptimality of threshold approaches for snow cover retrieval over large and heterogenous areas. We propose to extend Section 6.5 to discuss this aspect.

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