The manuscript by Bair et al. is focused on the erroneous representation of snow reflectance spectra in airborne and satellite data. While I find this topic very interesting and current, the content of the manuscript is quite redundant with another manuscript currently under evaluation in this same journal (https://egusphere.copernicus.org/preprints/2024/egusphere-2024-1020/) from the same group. I have already reviewed that manuscript, so I address the authors to my comments (RC1).

In general, I would highly suggest to merge these two submissions during the revision of Bohn et al. 2024. I leave to the Editor the decision on this point. If the authors prefer to keep this manuscript as a separate submission, a major review is needed before publication.

We thank Referee #1 for their careful review. We are aware of the issue with redundant information in both manuscripts, which sometimes happens while manuscripts are under simultaneous review. We have discussed this issue with the co-authors of both manuscripts and the Handling Editor for this manuscript and will remove the overlapping section and verbiage from Bohn et al. (2024) during its revision. These will be replaced with a citation to this discussion paper.

Hereafter, I will detail my major concerns.
The authors build their argument on one (n=1) spectrum from PRISMA showed in Figure 1.

That is not correct. The hooking is observable across many surface reflectance products.

From lines 38-39 “Surface reflectance products show suspicious hooking in AVIRIS-NG (Green et al., 2023), PRISMA (Townsend et al., 2023), EMIT (Green, 2022), and Collection 2 Landsat 8/9 (Crawford et al., 2023).”

The Brief Communication format limits the number of figures to 3. We already have 2 multi-part figures, so we will add one additional multi-part figure showing erroneous hooking in clean snow on flat terrain. We will show erroneous hooking in: (a) AVIRIS-NG; (b) EMIT; and (c) Landsat 8/9.

Furthermore, this spectrum is derived from a atmospheric-topographic correction that itself can introduce erroneous hook in snow reflectance.

Yes, that is the point of this manuscript.

At least, I would ask the authors to provide a comparison with standard L2(C-D) products from PRISMA.

The hooking is present there as well. See our response below to the question about which PRISMA processor was used for Figure 1.

A recent paper (Di Mauro et al. 2024) provided an evaluation of PRISMA reflectance and radiance products for different snow conditions. Same holds for Ravasio et al. (2024). In that cases, no clear hook is displayed in snow reflectance spectra. Which PRISMA processor has been used for generating the plot in Figure 1? When data have been downloaded from the ASI portal? In fact, several improvements have been made in the latest PRISMA processor (v_4_1_0_02_05). For example, Kokhanovsky et al. (2022) is based on an earlier version of the processor, and a downward hook is sometimes displayed in an area with expected clean snow (i.e. upper portion of the Nansen Ice Shelf, Antarctica).

The data are PRISMA L1 TOA, downloaded September 2022. In Kokhanovsky et al. (2022), the PRISMA L2D "processor" was used. Instead, we used the SISTER terrain-naïve "processor", so it should not depend on any updates to PRISMA processing scheme.

If the authors want to show that the hook is widespread, they should provide more evidence (e.g. different snow types, different latitudes, different sensors, etc.).

Agree, we will show the hooking for other surface reflectance products above as an additional figure.

Furthermore, they should provide evidence that the snow was clean (low concentration of impurities) at the ground.

For the Idaho site in Figure 1, snow spectra were collected at this same site on 2 Feb, 9 Feb, 23 Feb, and 10 Mar. There was no signal of LAP, which is clear from Figure 1.

Here are details for the figure we will add.

For AVIRIS-NG, we have an example from 19 Mar 2021 over Grand Mesa CO. We do not have in situ spectroscopic measurements, but they aren't needed. Instead, a snowpit was dug the day before the flight as part of the SNOWEX campaign and we have careful pit measurements and snowpack surface photos showing a clean snowpack with no evidence of dust or other impurities, which would have been noted in the pit. In contrast, the AVIRIS-NG spectra from that date show dramatic hooking indicative of several hundred ppm dust.

For EMIT, we have a scene near Mammoth Mountain from 20 Feb 2023 on a flat lake showing the hooking and a terrain-corrected broadband albedo measurement of 0.80 at CUES indicating clean new snow, as detectable dust-covered snow for this region at this time would be highly unusual.

For Landsat 8, we have co-located and co-incident in situ spectroscopic measurements from a flat lake on Mammoth Mountain from 8 Apr 2021 showing erroneous hooking.

Further information on the properties of snow at the surface is needed. I see that they reference to Townsend et al. (2023) dataset, and I learnt about the SISTER initiative. This

should be described in detail in this manuscript as well. How many pixels have been averaged?

4 pixels over Idaho City Football Field. Location can be found here: 43.8387, -115.8293

Was snow flat in that area?

1.3 - 2.6 degrees. Yes.

During which period field data have been collected?

Most of the winter. But for this one you are showing, PRISMA is Feb 10 and ASD is Feb 9.

Which spectrometer and protocol have been used for field spectroscopy measurements?

- ASD FieldSpec4
- Held bubble level 1 m above the surface
- We used the bare fiber with no fore-optic attachment for this measurement.
- +/- 1 hour around solar noon
- Measurements for Feb 9 were completely cloud free.

We are happy to provide this information to the Reviewer, and as these reviews are open will be accessible to any interested person, but will omit from the manuscript given it is a Brief Communication.

In the title, I read that the manuscript is about satellite and airborne sensor. Throughout the manuscript those airborne sensors are not detailed. Can you provide evidence of hooking from airborne sensors (e.g. AVIRIS, APEX etc.)?

Yes, we will provide evidence of hooking from AVIRIS-NG, as mentioned above

Further still on the title: If the hook is located below 500nm, likely snow will not "look" dirtier, at least from a correct RGB representation.

Unclear on this comment. Blue light < 500 nm is where most of the issue is. If the snow in polluted with LAPs, it will appear dirtier to the naked eye.

In line 31, I read: "Standard surface reflectance products are rife with hooking errors", but no references either evidence of this hooking errors is detailed. I strongly encourage the authors to go more in detail on this error. Please, see my comments to Bohn et al. 2024 on this topic.

See above

Line 94-95: these conclusions strongly overlaps with Section 5.1 ("the blue hook") in Bohn et al. 2024.

See above

References:

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Di Mauro, B., Cogliati, S., Bohn, N., Traversa, G., Garzonio, R., Tagliabue, G., et al. (2024). Evaluation of PRISMA products over snow in the Alps and Antarctica. Earth and Space Science, 11, e2023EA003482. https://doi.org/10.1029/2023EA003482

Kokhanovsky A, Di Mauro B and Colombo R (2022) Snow surface properties derived from PRISMA satellite data over the Nansen Ice Shelf (East Antarctica). Front. Environ. Sci. 10:904585. doi: 10.3389/fenvs.2022.904585

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