Dear Editor,

I sincerely appreciate your kind and encouraging comments and suggestions regarding our manuscript. We have tried to revise the manuscript as thoroughly as possible.

For the comments from Reviewer 1, we corrected the typo as follows:

Lines 137 and 305: from “Dynin” to “Dyunin.”

However, we are somewhat confused by the comments from the second reviewer. As I mentioned in my previous reply, this manuscript aims to present a concise overview of our unique observations and the obtained results. The dataset from our extensive observations is substantial, providing various avenues for concrete conclusions about the spatiotemporal structures of blowing snow. More comprehensive analyses will be presented in subsequent manuscripts currently in progress.

I would feel reassured by your comments, as an editor, that you do not agree with the reviewer’s statement that preliminary results do not belong in scientific results. We are considering whether we should mention that this is the first issue instead of calling it preliminary.

Additionally, I do not understand why the reviewer mistrusts the SPC. It is a well-established blowing snow sensor that outputs both particle size and numbers, representing transport flux every second. Many researchers in the field of blowing snow have used this system as the most reliable sensor, not only in wind tunnels but also under harsh conditions like the alpine regions of the Alps and even in Antarctica (e.g., Naaim et al. 2010 and 2014, Wever et al. 2023). It has also been applied in sand transport research as a "Sand Particle Counter." The reviewer suggests that the signal is not a genuine feature of snow flux. However, snow flux over the snow surface is not uniform, so it is natural that the intensity shown in the figure changes with time and space, not due to sensor sensitivity. As is also explained in the previous reply, all the SPC sensors are properly calibrated with specific procedures before the observations; thus, the sensitivity and accuracy of all sensors are consistent. Over the snow surface, we can generally avoid the effect of dust and fine soil contamination over the optical parts, which differs from sand and soil surfaces. Numerous researchers overseas have attested to this and have kept measurements for long periods in the Alps and Antarctica. Generally, we can leave the system for an entire winter without cleaning or wiping the optical parts. This campaign was carried out for less than two months, so we believe the contamination over the optics can be reasonably neglected.

The following description was added to the manuscript:

L69-77: Incidentally, it has also been applied in sand transport research as a "Sand Particle Counter" (Yamada et al., 2002 and Mikami et al., 2005). Accurate calibrations of all sensors are carried out beforehand, allowing us to reasonably exclude the effect of sensitivity differences. Over the snow surface, we can generally avoid the effect of dust and fine soil contamination over the optical parts, which differs from sand and soil surfaces. Numerous observations overseas have attested to this and have kept measurements for long periods in the Alps (e.g., Naaim-Bouvet et al., 2010, 2014, and Gilbert, 2019), the Arctic (e.g., Lenaerts et al., 2014 and Frey et al., 2020), and Antarctica (e.g., Sigmond et al., 2021 and Wever et al., 2023). Generally, we can leave the system for an entire winter without cleaning or wiping the optical parts. This campaign was carried out for less than two months, so we believe the contamination over the optics can be reasonably neglected.

As pointed out by the reviewer two, the 1.5-meter spacing in our measurements may not be fine enough. However, increasing the number of sensors narrows the gap between them, which can substantially affect both the air and blowing snow particle flow. This presents a dilemma and requires a compromise. Smaller sensors, such as FlowCapt for snow (Trouvilliez et al., 2015) and Sensit for sand (Stockton and Gillette, 1990), could be alternatives. However, it is well known that neither can obtain particle flux precisely (e.g., Lehning et al. 2002). Although the resolution may not always be fine enough to discern precise structures, our data analysis succeeded in drawing the approximate outline and structures of the streamer in Fig. 7. The 2D-autocorrelation of horizontal mass flux in Figure 8 indicates a lateral spacing of about 5 meters, which is more than three times larger than the sensor spacing of 1.5 meters. These explanations, including the limitations, are briefly described as follows:

L144-148:

Despite the 1.5-meter spacing between SPCs, which did not offer sufficient resolution for clarity in the figure, the analysis suggests that the structures at 1 cm above the surface had widths with the peak of flux around 30 cm wide and a lateral spacing of about 5 meters. The strong correlations are evident within plus/minus ΔY= 5 m in Fig. 8.

Although the reviewer two claims that Table 1 only reports quadrant percentages, not links with snow flux, it shows the percentage during the four blowing snow events. Furthermore, the correlation between the horizontal snow flux and the absolute value of the kinetic shear stress u'w' in each quadrant, and the relations between the snow flux, the fluctuating component of u' and w', and the product of u' and w' are shown in Figs 11 and 12 and discussed.

As mentioned above, hopefully the comments by both reviewers are reasonably satisfied. I hope you understand that the sheer volume of data obtained from our extensive observations presents a multitude of avenues for a more in-depth exploration of the spatiotemporal structures of blowing snow. Specific aspects, including the particle speed and wind speed in the proximity of these structures, as previously pointed out by the editor, are topics we are actively addressing in subsequent manuscripts currently in progress.

Lastly, following the suggestions from the editorial support team, we have checked the color schemes used in the figures with the Coblis Color Blindness Simulator. We have confirmed that readers with color vision deficiencies (Anomalous Trichromacy) will be able to recognize them without any difficulties.

Best regards,

Kouichi

Dear Reviewer1,

Thank you very much again for your careful review of our manuscript and for providing positive evaluations.

As is pointed out, we corrected the typo as follows:

Lines 137 and 305: from "Dynin" to "Dyunin."

Dear Reviewer2,

Thank you very much again for your careful review of our manuscript and for providing insightful comments, especially regarding the standpoint of sand particle transport. Although all of them are rather harsh review, I believe they are informative and educational to improve our manuscript.

First, the authors stress many times that this manuscript only presents "preliminary results", "a brief overview", and that "analysis is still quite limited". This is a very serious problem because scientific research publications should fundamentally not be preliminary, brief, or limited. Preliminary outputs may be presented at a conference, but they cannot be expected to become part of the established scientific literature, since any such preliminary work is by definition potentially still subject to change and modification. On this basis alone I believe this manuscript must be rejected because it is explicitly unable to contribute definitive scientific findings.

As I mentioned in my previous reply, this manuscript aims to present a concise overview of our unique observations and the obtained results, as a first issue. The dataset from our extensive observations is substantial, providing various avenues for concrete conclusions about the spatiotemporal structures of blowing snow. More comprehensive analyses will be presented in subsequent manuscripts currently in progress.

I would feel reassured by the editor, that he does not agree with the reviewer's statement that preliminary results do not belong in scientific results. We are considering whether we should mention that this is the first issue instead of calling it preliminary.

Second, my central concerns about the SPC data artefacts have been ignored. The authors stress that their SPC sensors are well calibrated and they give additional technical details, but they entirely fail to engage with my comments on the clearly artificial nature of the unrealistically elevated/depressed signals for specific sensors over long periods of time, as visually evident still in the revised snow flux 'maps' of figures 4c, 7, 10a, and 13a. In figure 13a, for example, it is abundantly clear that the elevated signal at Y=3 over a period of 8 seconds is not a genuine feature of variable snow flux, and further scrutiny of the snow flux maps shows that most individual sensors produce signals that are consistently at different relative intensities. The authors have not addressed this issue, nor have they recognised the likelihood that sensor optics are fouled by the challenging field conditions (i.e. independent of calibrations) and the artefacts continue to affect their interpretations.

SPC is a well-established blowing snow sensor that outputs both particle size and numbers, representing transport flux every second. Many researchers in the field of blowing snow have used this system as the most reliable sensor, not only in wind tunnels but also under harsh conditions like the alpine regions of the Alps and even in Antarctica (e.g., Naaim et al. 2010 and 2014, Wever et al. 2023). It has also been applied in sand transport research as a "Sand Particle Counter." The reviewer suggests that the signal is not a genuine feature of snow flux. However, snow flux over the snow surface is not uniform, so it is natural that the intensity shown in the figure changes with time and space, not due to sensor sensitivity. As is also explained in the previous reply, all the SPC sensors are properly calibrated with specific procedures before the observations; thus, the sensitivity and accuracy of all sensors are consistent. Over the snow surface, we can generally avoid the effect of dust and fine soil contamination over the optical parts, which differs from sand and soil surfaces. Numerous researchers overseas have attested to this and have kept measurements for long periods in the Alps and Antarctica. Generally, we can leave the system for an entire winter without cleaning or wiping the optical parts. This campaign was carried out for less than two months, so we believe the contamination over the optics can be reasonably neglected.

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Third, the author-response and the revisions in the manuscript still lack any recognition of the very significant limitations imposed by the 1.5 m lateral spacing of the instruments, i.e. that the spatial resolution is fundamentally limited to this scale and the data cannot, by definition, reveal any structures or trends that are smaller than this. The author-response claims that "…by adjusting the range of the color bar in the figures, the width of the streamers became reasonably thinner,…" but this is simply a visualization gimmick (further exacerbated by the relative sensor distortions); streamers are generally on the order of 10-20 cm wide, and the instrumentation array is fundamentally incapable of resolving the sort of spatial patterning (streamers) visible in figure 6b. Text in the conclusion (L214-216) claiming to have observed streamer families and nested streamers is simply not sustainable. It also means that the (new) 2D auto-correlation map of Figure 8 has a fundamental lateral (spatial) resolution of 1.5 metres and hence nearly all of the autocorrelation structure is simply an interpolation feature.

In actual, the 1.5-meter spacing in our measurements may not be fine enough. However, increasing the number of sensors narrows the gap between them, which can substantially affect both the air and blowing snow particle flow. This presents a dilemma and requires a compromise. Smaller sensors, such as FlowCapt for snow (Trouvilliez et al., 2015) and Sensit for sand (Stockton and Gillette, 1990), could be alternatives. However, it is well known that neither can obtain particle flux precisely (e.g., Lehning et al. 2002). Although the resolution may not always be fine enough to discern precise structures, our data analysis succeeded in drawing the approximate outline and structures of the streamer in Fig. 7. The 2D-autocorrelation of horizontal mass flux in Figure 8 indicates a lateral spacing of about 5 meters, which is more than three times larger than the sensor spacing of 1.5 meters. These explanations, including the limitations, are briefly described as follows:

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Despite the 1.5-meter spacing between SPCs, which did not offer sufficient resolution for clarity in the figure, the analysis suggests that the structures at 1 cm above the surface had widths with the peak of flux around 30 cm wide and a lateral spacing of about 5 meters. The strong correlations are evident within plus/minus ΔY= 5 m in Fig. 8.

Fourth, although the author response notes that “analysis was conducted with all the sensor data,” this is only true for the spatio-temporal maps of snow flux and wind forcing, but instead these are severely limited because only very short snippets of time (on the order of 10s of seconds) are shown and discussed. Other results, such as the auto-correlation, power spectra, and quadrant analysis, are severely limited because they are derived only from single instruments or from just one specific measurement run. There is insufficient consistent analysis of all available data as well as its internal variability to produce any reliable findings.

We believe it is not always necessary to present all data, as this manuscript is not a data report. For example, in this study, we introduced typical two-minute data segments from three different days. Similar strategies are also found in sand transport studies, such as Baas and Sherman (2006). While the analysis presented here is limited to specific cases, the amount of data obtained in this study is enormous. Analyzing all the available data is not practical and not always meaningful.

We believe that analyzing several typical cases, selected methodically rather than arbitrarily, and using these analyses to start discussions is a standard approach for reaching conclusions. In fact, we analyzed five cases, not just one, to discuss the relationship between wind speeds and mass fluxes with the parametric curves in Fig. 14.

Partly due to the above problems, the conclusions are still essentially trivial; the only finding that can be somewhat substantiated is the notion that snow flux correlates better with $u'>0$, but this is also already well-known from the sand transport literature (as is recognised here too). Even this is still quite limited though because the relevant results were only obtained from one measurement run. Authors claim that Table 1 also supports this finding, but this is not correct as that table only reports quadrant percentages, not links with snow flux.

Although the reviewer claims that Table 1 only reports quadrant percentages, not links with snow flux, it shows the percentage during the four blowing snow events. Furthermore, the correlation between the horizontal snow flux and the absolute value of the kinetic shear stress $u'w'$ in each quadrant, and the relations between the snow flux, the fluctuating component of u' and w', and the product of u' and w' are shown in Figs 11 and 12 and discussed.