General comments:

I appreciate the opportunity to review the manuscript entitled "Environmental Conditions for Snow Cornice Formation tested in a Wind Tunnel." In this study, the authors seek to improve the process understanding of snow cornice formation by conducting wind tunnel experiments in a cold laboratory. Specifically, the authors simulate cornice development in the wind tunnel by forcing snow particles made by a snowmaker over a small "ridge" of compacted snow at various wind speeds. Cross-sectional photographs of the model ridge and associated cornices taken with high temporal resolution help illustrate cornice development under different wind speeds. The manuscript in its current form is generally well-written, with few grammatical errors and clear language. The authors describe their methodology such that future work can easily repeat, and thus build upon, the present experiments. Figures are relevant, clear, and appropriately described. I found the combination of the repeatable methodology and associated results to be a relevant basis for future field studies and would like to complement the authors on their work.

Relatively few, to my knowledge, studies in the last couple decades have addressed cornice formation in laboratory settings. I think such laboratory studies offer a compelling avenue to improve our understanding of cornice processes and refine conclusions derived from field data. The methods employed by the authors in the current study therefore have the potential to augment recent field investigations by better constraining the environmental conditions influencing various processes of cornice dynamics (e.g. wind speeds leading to cornice accretion). Such work falls within the scope of *The Cryosphere* and will be of interest to an audience of snow researchers and practitioners working with cornice-related avalanche problems.

In this context, although the manuscript provides a decent overview of some previous work and general cornice-related concepts, the current introduction does not, in my opinion, adequately address the scientific framework for the current study. Specifically, the introduction fails to effectively link the referenced field studies to the "macroscopic view" mentioned in the abstract and laboratory methods presented in the current work. The authors should, in my opinion, considerably expand the introduction to better introduce and justify the laboratory methods employed in this work as pathway to improve the understanding of cornice dynamics in the field. In such an expanded introduction, the authors would have an opportunity to cite the Naitou and Kobayashi paper referenced by reviewer #1 (which, to be fair, I also had not read previously) in addition to other laboratory experiments serving as a basis for the presented work. Additionally, the authors could help guide the reader by more specifically stating which aspects or processes of cornice formation they sought to investigate with their wind tunnel experiments – e.g. explicitly state what processes currently unresolved by field studies you hope to address in the laboratory. See also the specific comments related to content in the introduction.

My other major concern with the manuscript in its current form stems from the results and discussion in Section 3.2. In general, I think splitting the combined results and discussion section here could help with clarity (e.g. split the calculations and numerical results into a results section and the associated discussion into its own section). However, the main issue stems from the selection of the appropriate wind speed range for cornice growth in the field. The authors cite our 2020 paper as stating the wind speed range for cornice growth in the field is 12-30 m s^{-1}.

Unfortunately, this is a mischaracterization of the results from that paper. Vogel et al. (2012) determined cornice accretion occurred during periods with hourly maximum wind speeds of 12 m s^{-1} and observed cornice scouring when maximum hourly wind speeds were as low as 15 m s^{-1}. In our 2020 work, the temporal constraints on our TLS measurements of cornice accretion were relatively poor and did not allow us to effectively determine a lower threshold wind speed for which cornice accretion begins to occur. Instead – and admittedly this is a weakness in that study – we simply used 5 m s^{-1} as a conservative lower threshold for snow transport (and therefore, we assumed, cornice accretion) derived from the literature. Accordingly, although the comparison between the authors' experiments and field studies from lines 172 – 202 is interesting and relevant for this work, I think the authors should redo their calculations with a more appropriate $U_{t2.8}$ value.

My suggestion here would be to consider that field studies often struggle to determine the threshold wind speeds for cornice accretion and/or scouring due to temporal or spatial constraints on data acquisition. Laboratory experiments such as the current study can help better determine these thresholds, and therefore one option would be to extrapolate a "field" $U_{t2.8}$ based on your measured $U_{t0.4}$ and a logarithmic wind profile. This would then allow the authors to discuss how their results can help address gaps in field studies (e.g. more specifically constrain the wind speeds at which cornice accretion happens, with the wind speeds expressed for the height at which standard meteorological observations occur). In the conclusions, however, I would appreciate a link between your measurements (e.g. cornice growth occurs between 3.5 and 6 m s^{-1}) and the corresponding "field" wind speeds which will be more relevant, especially, for practitioners interested in your work.

Specific comments:

Title – I wonder if the title could be more specific than "environmental conditions" – would wind conditions be more appropriate?

Line 16 – please clarify what the percentages refer to, or consider omitting the percentages altogether

Lines 27-31 – consider splitting this long sentence for clarity and readability

Lines 36-38 – please revise this sentence, I don't understand what widely accepted hypothesis is referred to here

Lines 39-40 – which assumptions have no supporting evidence?

Lines 41-42 – this sentence needs to be revised in lieu of the existence of the Naitou and Kobayashi work. I am unable to read Japanese so cannot specifically comment on the methodological and content overlap between this work and the Naitou and Kobayashi study. I would suggest attempting to determine how your work differs from this previous work and adjusting the intro/results as needed.

Line 53 – is 7 m s^{-1} the maximum wind speed this device can generate?

Lines 108-113 – Super cool! Thanks for this.

Figure 3 caption – I think the cornice length growth rate and cornice thickness growth rate are represented with Xs, not triangles.

Line 144 – by crackdown do you mean cornice collapse or failure?

Line 145-146 – Is this because higher wind speeds form a cornice with a smaller angle?

Lines 175-185 – I am struggling to follow these calculations here, which may partially be due to my inexperience with such work. Is it possible to more explicitly define your terms somewhere (e.g. in a table) in the manuscript to help along readers such as myself? Also, to what are you referencing the Leonard et al. (2012) study here?

Line 204 – please revise this sentence in lieu of Naitou and Kobayashi.

Lines 205 – 215 – specifically here I think this work would really benefit from explicitly linking your results to field meteorological measurements (e.g. $U_{t2.8}$) for increased utility of your results and work.

Technical corrections:

Line 28 – that snow cornices only grow

Line 135 – there are no more chances for slabs to form on the model edge because of…

Line 137 – gets smaller

Line 202 – the newly formed cornice