Response to referee #2's interactive comment

General comments:

1. 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.

Response: We thank the reviewer for the positive feedback on the general aspects of our work. We really appreciate your efforts in reviewing this manuscript. In the following, we respond point by point to your comments.

2. 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.

Response: Thank you for this valuable suggestion. We have expanded the introduction by adding the following paragraph:

"Cornice accretion and scouring have only been measured in few field and laboratory environments: A notable exception is Naito and Kobayashi (1986), who proposed that the suitable wind speed for cornice growing process both in the wind tunnel and the field is between 4 to 8 m s^{-1}. In a moist Arctic environment, Vogel et al. (2012) determined that cornice accretion occurred during periods with hourly maximum wind speeds of 12 m s^{-1} and observed cornice scouring when the maximum hourly wind speeds were as low as 15 m s^{-1}. Hancock et al. (2020) used 5 m s^{-1} as a conservative lower threshold for snow transport for cornice accretion which is the threshold wind speed for particles for entrainment. To explain the huge gaps in the threshold value in field observation results and the inner growth mechanism from a physical point of view, wind tunnel experiments in a cold laboratory at WSL/SLF are presented to quantitatively investigate the cornice formation process."

In the discussion, we added:

"The gaps found between different observations are mainly caused by the different snow surface conditions. In Gruvefjellet, the snow surface is relatively hard, which means the threshold wind speed for snow particle entrainment is high. No matter what kind of snow surface there is, the threshold wind speed for cornice accretion can be roughly estimated by the threshold wind speed for particle entrainment. The threshold wind speed for cornice scouring can be roughly estimated at 2.75 times of threshold wind speed."

3. 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.

Response: Sorry for our misleading expressions using $U_{t0.4}$ and $U_{t2.8}$. Here, it is not suitable for us to use the logarithmic wind profile to link the laboratory experiments and field observations for the following reasons:

1) A well-developed boundary layer cannot be generated in the ring wind tunnel.

2) The wind speed sensor is installed at the height of 0.4 m in the inlet of section S2, 0.5 m upstream of the ridge edge, which is not the real wind speed over the cornice (0.125 m in height, as shown in Figure 1), while the field observation station is located 400 m upstream of the cornice study site that contains a 220 m plateau ridgeline, which is also not the real wind speed over the cornice, moreover there is a height difference between the field observation station (Gruvefjellet met station, 464 m a.s.l.) and the cornice study site (about 460 m a.s.l., Figure 2 in the paper of Vogel et al. 2012).

Thus, we used the **nondimensionalization method** to unify the wind speed range between field studies and our laboratory works, using the threshold speed. In our experiments, the cornice accretion starts after the wind speed exceeds the threshold wind speed (3.2 m s^{-1}) with enough snow supply, and no cornice formation when the wind speed is below it (e.g., 3.0 m s^{-1}). In field observation, Vogel et al. (2012) surmised that cornice accretion proceeded during both entire snow seasons (46h in 2008/2009, 54h in 2009/2010), when wind speeds averaged 12 m s^{-1}, with a minimum of at least **10 m s^{-1}**, marking the lower limit of the cornice accretion; Eckerstorfer et al. (2012) measured that the initial cornice accretion started along the plateau edge during the first snowfall (10-12, Oct. 2010) with maximum wind speeds of **11 m s^{-1}**, which is lower than the observation result of Vogel et al. (2012). Thus, by analyzing the time series of wind speed data from Gruvefjellet meteo station (http://158.39.149.183/Gruvefjellet/index.html), we can conclude that the corresponding averaged wind speed is about 7.37 $\pm$ 0.97 m s^{-1} (and mean friction velocity is about 0.288 m s^{-1} using $z_0 = 10^{-4}$ m) when the maximum wind speed is in the range of 10.5~11.5 m s^{-1}. Considering the harder snow surface in Gruvefjellet (Eckerstorfer et al., 2012), this wind speed value is comparable to the threshold wind speed in previous literature. For the wind in the field is more gusty and turbulent compared to the wind tunnel, the actual threshold wind speed value for cornice growth in the non-dimensional calculation should equal the maximum value of the threshold wind speed measured in the field, according to the study of Li et al. (2020, DOI: 10.1029/2019GL086574). Thus, we chose the maximum wind speed value 11 m s^{-1} as the comparison value with the threshold wind speed in our laboratory experiment where the wind condition is stable and steady. The nondimensional velocity is then defined as: $\tilde{u} = \frac{u_{ref}}{u_{t\_ref}} = \frac{u_*}{u_{*t}}$, in which u_{ref}, u_* are the wind speed at the measurement height and friction velocity, respectively, and u_{t_ref}, u_{*t} are the corresponding threshold wind speed at the measurement height and threshold friction velocity, respectively.

In summary, we used the **nondimensionalization method** to avoid all the problems of the uncertainty to choose the roughness length and reference height of the measured wind speed, and only need to figure out the threshold wind speed (when cornice accretion starts), which is a good way for extending our works to the field investigations.

To avoid misunderstanding, we revised the description of lines 172 to 178 as:

"Estimates of the threshold wind speeds for cornice accretion (Vogel et al., 2012; Hancock et al., 2020) are compromised by temporal or spatial constraints on data acquisition. Based on this, we made an estimation of the appropriate wind speed using a non-dimensional method to unify the wind speeds in the wind tunnel and fields. The non-dimensional mass concentration of snow particles can be estimated in the following steps:

1) Dimensionless wind speed $\tilde{u}$ can be calculated as the ratio of wind speed at the measured site relative to the threshold wind speed measured at the site when the cornice starts to grow: $\tilde{u}=\frac{U_{0.4}}{U_{0.4t}}=\frac{U_{2.8}}{U_{2.8t}}=\frac{u_*}{u_{*t}}$. Here 0.4 m represents the sensor height in our experiment, 2.8 m represents the weather station wind speed measurement setup height in Gruvefjellet, and * represents the friction velocity over the cornice. In the field observation, Vogel et al. (2012) surmised that cornice accretion proceeded during both entire snow seasons (46 h in 2008/2009, 54 h in 2009/2010), when wind speeds averaged 12 m s^{-1}, with a minimum of at least 10 m s^{-1}, marking the lower limit of the cornice accretion; Eckerstorfer et al. (2012) measured that the initial cornice accretion started along the plateau edge during the first snowfall (10-12, Oct. 2010) with maximum wind speeds of 11 m s^{-1}. Considering more gusty and turbulent winds in the field, the maximum value for threshold wind speed is more comparable to our laboratory data (wind flow is more stable), we chose 11 m s^{-1} as the threshold wind speed in the field. Thus, dimensionless snow transport rate on the flat surface $\tilde{Q} = \frac{gQ}{\rho_a u_{*t}^3}$ can be calculated as a function of the dimensionless wind velocity $\tilde{u}$. Several common formulas of the function are shown in Table 4."

We also added content to discussions:

"Our wind tunnel experiment results can resolve inconsistencies in these observations. From our wind tunnel experiment, we can conclude that the threshold wind speed for cornice accretion is very close to the threshold wind speed for particles entrained from the surface. The inconsistency in the threshold wind speed for cornice accretion is due to the different snow surface conditions. We can conclude from our experiment that: "Drifting snow is necessary for cornice formation. Only when the wind speed is over the threshold wind speed for particle entrainment, there exists an opportunity for particles to impact and stick on the edge surface, where accumulation is the basis for the cornice formation. When the non-dimensional wind speed $\tilde{u}$ is over 2.7, the scouring effect is much stronger than accretion so that no cornice forms, which is consistent with the Eckerstorfer et al. (2012) and Vogel et al. (2012) field observations."

Specific comments:

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

Response: Thank you for the suggestion. We will revise the title to "Wind Conditions for Snow Cornice Formation tested in a Wind Tunnel."

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

Response: The percentages refer to the contribution of different types that causes secondary snow avalanche in the snow seasons 2006-2009 in Longyearbyen. For clarity, we have omitted the percentages in the revised manuscript.

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

Response: Thank you for pointing this out, we will revise it as:

"Indirect evidence was presented by van Herwijnen and Fierz (2014) that snow cornice only grows under moderate to high strength wind during or soon after the snowfall. The cornice width from observation is in remarkable agreement with the wind drift index calculated by the snow cover model SNOWPACK (Lehning and Fierz, 2008), which indicates that snow transport plays an important role in cornice formation."

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

Response: We have deleted this sentence which might lead to misunderstanding: "Particles, which follow the changing wind direction locally as the flow passes the ridge will stick to the growing cornice front at the mountain ridge is a widely accepted hypothesis."

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

Response: In here, we mean these assumptions of the vortex and the electric fields lack supporting experimental evidence. To make it clearer, we revised the sentence to "However, there is no direct experimental evidence to support neither vortex nor electric field influences on the cornice formation."

6. 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.

Response: Thank you very much for the suggestion. We have answered the differences between our work and Naitou and Kobayashi's study in the reply to the first reviewer (*Q*3) and will adjust the intro/results in the revised manuscript according to the context of the discussion of the reply.

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

Response: The maximum wind speed can reach 8 m s^{-1}. However, in our experiment, we only use the range of 3 - 7 m s^{-1} for there is no cornice when $U \leq 3$ m s^{-1} and $U \geq$

6.5 m s^{-1}.

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

Response: Thanks for your field observation data.

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

Response: Thank you for pointing this out, we will correct the caption to "cornice length growth rate (light blue Xs), cornice thickness growth rate (grey Xs)".

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

Response: Thank you for pointing this out, we will change the word "crackdown" to "collapse" for precise expression in the revision.

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

Response: Yes, cornices form at a higher wind speed with a smaller angle.

12. 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)s in the manuscript to help along readers such as myself? Also, to what are you referencing the Leonard et al. (2012) study here?

Response: Thanks for your comment, we will add a notation with physical definitions and units of all the symbols in the manuscript.

For Leonard et al. (2012) study, we were referencing the threshold friction velocity. However, we deleted the sentence "where u_{*t} = 0.25 m s^{-1} is the threshold friction velocity Leonard et al. (2012)" to avoid misunderstanding.

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

Response: We have deleted the word "first" in this sentence.

14. Lines 205 – 215 – specifically here I think this work would really benefit from explicitly linking your results to field meteorological measurements (e.g. Ut2.8) for increased utility of your results and work.

Response: Thanks for this valuable suggestion. We will reorganize it by splitting the linking wind tunnel results to field meteorological measurements as a separate section of the Discussion.

15. 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

Response: Thank you for pointing these out. We will revise them in the revised manuscript.