Response to RC 1:
(The reviewer comments appear in back, the responses are in blue and the proposed changes to manuscript are in ***bold italics***.)

Liang and others report the filling and drainage of a collapse basin at the Flade Isblink ice cap in northeast Greenland. The study builds on the investigations by Willis et al. (2015), who show that the basin was formed in 2011 by sudden subglacial lake drainages. Liang and others extend the time series of the observations made by Willis et al. (2015) with surface elevation estimates on the basis of digital elevation models of ArcticDEM and ALOS data as well as ICESat-2 laser altimetry. They show that the ice surface of the collapsed basin rose by 55 m after the drainage event in 2011. The authors link the ice surface uplift with a refilling of the subglacial lake and ice flowing into the basin. Furthermore, they correlate the amount of water needed for the refilling with the amount of surface meltwater produced at the ice surface.
In summary, I believe that this manuscript is an important contribution to the literature on subglacial water activity of the Greenland Ice Sheet and Greenlands smaller ice caps and fits into the scope of The Cryosphere. The manuscript is overall clearly written, and the results are well presented, however, some of the statements made need some clarification. Overall, I believe that the manuscripts, with some modifications and clarifications, merits publishing.
We thank the reviewer for a positive assessment.

1. General comments
The scientific quality of the work presented is generally strong, with good associated analysis and discussion. The methodology is almost completely inspired by the Willis et al. (2015) paper, which is probably not very innovative but has the advantage that it is an appropriate continuation of the observations made at this ice cap. Furthermore, the methods are in most parts clearly described, which enable others to reproduce the results. The figures are of good quality and appropriate. The paper structure is generally easy to follow and is mostly clearly written and clear in its conclusions.
Thank you for this review. We agree with all the suggestions and believe that these will improve the quality of the paper.

Main points:
1) Introduction
The introduction provides useful information about the study region and subglacial lakes in Greenland. However, I think a paragraph describing the versatile methods, which have been used to detect subglacial lake activity (even though most of them are located in Antarctica) should also be introduced here to give the reader an impression of what is available and what you are using in your study. In addition to laser altimetry and DEMs it would be useful to complement these two methods with:
- SAR tracking e.g., Joghin et al. (2016); https://doi.org/10.1002/2016GL070259
- InSAR: Neckel et al. (2021); https://doi.org/10.1029/2021GL094472 and Gray et al. (2005); https://doi.org/10.1029/2004GL021387

Thank you for the suggestion. In the revised version, we will add a paragraph describing the methods of detecting subglacial lake activity.
Several new sentences added to the Introduction section will be:
"Satellite remote sensing techniques have recently been used to monitor the subglacial lakes and detect their activities. As an indirect observation of subglacial lake activity, long-term ice surface elevation changes are usually derived from satellite altimetry (e.g., Fricker et al., 2007; Siegfried and Fricker, 2018, 2021). More recently, time-stamped digital elevation models (DEMs) have been utilized to reveal the detailed patterns of surface deformation (e.g., Livingstone et al., 2019; Willis et al., 2015). A few studies also use the Synthetic Aperture Radar (SAR) speckle tracking (Joughin et al., 2016; Hoffman et al., 2020) and Interferometry SAR (InSAR) (Gray et al., 2005; Neckel et al., 2021) to detect ice surface displacements."

2) Figure 2
This is a great figure and shows that the authors have put a lot of thought into how they present their results. However, there is a lot of information and a lot of lines with all colours of the colour spectrum. I have a suggestion on how the results you describe maybe a little bit easier to visualize:

- What about if subfigures (b-e) are split into two columns, where you show in the left column the same style of figure, but only showing the ice surface rise until 2019. In the second column, you can show then the ice surface subsidence and the following uplift in 2020.
- For the plots in the right column, you could still show the lines from the uplift before 2019 in slight grey or so in the background. This might be a way to disentangle the two processes you describe: the steady uplift and the sudden drainage in 2019 and continuous filling thereafter.

Thank you for the suggestion. We will improve this figure accordingly.

3) Throughout the manuscript, not enough attention is paid to the fact that the Flade Isblink ice cap is not directly connected to the rest of the Greenland Ice Sheet. Here it should be ensured that this is generally separated and argued in such a way that the results and interpretations of this study, however, can of course be applied to the Greenland Ice Sheet and are thus definitely very helpful.
Agreed. In the revised version, we will add several sentences to clarify the disconnection between the subglacial lake and the GrIS. We will also give more discussion on the difference/similarity between the ice sheet subglacial lake and the ice cap subglacial lake in the discussion section.

4) Ice inflow into the basin
One thing in the manuscript that I found difficult to understand was the idea that "volume change is mainly caused by ice inflow into the basin" (L109). I finally understood this, when I read the paper of Whillis et al. (2015) where it is explained a little bit better. So that means that the ice inflow that you talk about in your paper is related to the subsidence that occurs around the basin outline in your 1.5m buffer as

indicated in Figure 2a. And the most likely interpretation is that this negative volume flows into the basin (because where else should it go). If this is what you mean, I have the following recommendations:

- Please explain a little bit better what you mean by "inflow into the basin" and that this concept is taken from the Whillis et al. (2015) manuscript.
- I think it would also be helpful to (for example) plot the 1.5 km buffer in Figure 2a and also visualize with an arrow that all the blue area (the subsidence) is the volume that flows into the basin.

Thank you for pointing this out. We will add/change some text to explain more about the "inflow into the basin". And the 1.5 km buffer area will be shown in Figure 2a.
The text added/changed here will be:
"***Previous studies show that a reduction in the depth of the depression would result from the infow of the ice around the basin (Aðalgeirsdóttir et al., 2000; Willis et al., 2015). Therefore, we expect that the basin volume change here is mainly caused by ice inflow and subglacial lake filling. Assuming the subsidence that occurs around the basin outline in a 1500-m buffer region correspond to ice flowing into the basin, we calculate the inflow volume by integrating the surface elevation changes over the buffer area (Willis et al., 2015).***"

5) Treatment of ice surface flow velocity analysis after the lake drainage (e.g. L127-129 and Figure 6c,d): I think the analysis of ice velocity variations after the drainage event should be done along a flow line instead of a single point location. At the moment we can just see that "somewhere" downstream of the lake the velocity increased, which makes me very curious what happened up- and downstream of this location (and more importantly when!).

Thank you for pointing this out. We do understand that more velocity variations analysis is desirable, and we would very much like to show the velocity change map or velocity change profile. But unfortunately, the coverage of the velocity product is poor in July and August 2019, due to few successful matching pixels in the summer when intense surface melting usually happened. Therefore, we are limited in where we can sample the velocity and choose to calculate the mean velocity from a region (800m*800m) located downstream of the collapse basin.
We will modify the text to clarify why this specific small area was chosen to evaluate changes in ice surface velocity.
"***Due to the limited coverage of the ice velocity product in the summer, a 800m*800m region that is located downstream of the collapse basin is chosen to evaluate changes in ice surface velocity (Figure 6c). We calculate the mean velocity within this region to estimate the velocity time series from 2018 to 2020.***"

6) There is a slight inconsistent usage of the term "Greenland Ice Sheet" and its abbreviation "GrIS". Please make this consistent:

- "Greenland ice sheet" vs. "Greenland Ice Sheet"
- "GrIS" vs. "the GrIS"

Thank you for catching this. We will use "Greenland Ice Sheet" and "the GrIS" throughout the text for consistency.

7) Overall the language of the manuscript is clear, but I have the impression that the grammar is not always correct. This should be checked by a native English speaker.
We regret there were problems with the English. In the revised version, the manuscript will be carefully checked by a native English speaker to make sure the grammar is correct.

2. Specific comments
L38-49: Please update the paragraphs information of the current knowledge and database of subglacial lakes in Greenland by the findings of the recent review of Livingstone et al. (2022):

- Livingstone, S. J., Li, Y., Rutishauser, A., Sanderson, R. J., Winter, K., Mikucki, J. A., et al. (2022). Subglacial lakes and their changing role in a warming climate. Nature Reviews Earth & Environment, 1–19. https://doi.org/10.1038/s43017-021-00246-9

Thank you for the suggestion. We will update this.

L56: Reference "(Willis et al., 2015)" without brackets.
Thank you for catching this. We will correct this.

L57: "DEM" is not defined anywhere. Although most of the readers might know what it stands for, it would be good to define it here once.
Thank you for catching this. We will define it when it first appears.

L59: "[...] this subglacial lake is under the ice cap [...]" should be "[...] this subglacial lake is located under the ice cap [...]"
Thank you for catching this. We will correct this.

L234: You state that "The repeat filling and drainage of the subglacial lake is on the scale of ~8 years". However, then you state later that the lake was probably not completely full in the 2019 drainage event. Also when I have a look at Figure 3 I do not fully have the impression that the small drainage event in 2019 would allow speaking of a real cyclicity of filling and drainage of the lake. I agree that "something happened" in 2019, but when I compare this to the original collapse basin surface in Figure 2, I have more the feeling that the 2019 event was a small outburst, but still in the "filling process" (what you also state later in the conclusions as "partial drainage". You do later argue that "the repeat filling and drainage is not only decided by the volume of water stored in the subglacial lake but also may be controlled by meltwater input … and bedrock relief …". However, all this is still very speculative, hence, I recommend removing this statement about the "repeat filling drainage cycle of 8 years".
We agree. We will avoid using the statement of "repeat filling drainage cycle of 8 years" throughout the paper.

L245-248: Here you discuss why it is most likely that "surface meltwater is likely the only supply for this subglacial lake." I think at this point it should again be made clear that the Flade Isblink ice cap is isolated from the Greenland Ice Sheet and therefore not connected to its subglacial hydrology network, which further supports the idea that supraglacial meltwater that finds its way to the ice-cap bottom is the most likely source for the lake filling.

Thank you for the suggestion. We will add a sentence here to emphasize the separation. "***Moreover, the Flade Isblink ice cap is isolated from the GrIS, hence the subglacial lake is not connected to the subglacial hydrology network beneath the GrIS.***"

L261: In the conclusions, you state that: "The long-term measurements show that the subglacial lake was recharged by surface meltwater produced in the melt season". I think this should be stated as the most likely scenario instead of as a fact. Your observations are good and convincing but are based on remote sensing data only and the hypothesis is mainly based on observations made in other regions in Greenland where supraglacial water reached the bed. Hence, you cannot prove that the water at this ice cap makes its way down to the bed, although you have strong arguments for it. I would recommend stating that this is "the most likely scenario" instead of that the measurements "show" it.

Agreed. We will modify the text to:

"***The long-term measurements imply that the subglacial lake was most likely recharged by surface meltwater produced in the melt season***"

L263-265: Furthermore you state: "Our work demonstrates the potential for subglacial lake to store multi-year meltwater in GrIS, which may affect the ice flow by preventing the transfer of meltwater to the ice sheet margin." Here, again it should be made clear that this ice cap is not connected to the rest of the GrIS. I think it would be better to state something like that your findings on the ice cap that the subglacial lake can store multiyear meltwater [...] are useful to understand the hydrological processes on the GrIS.

Thank you for the suggestion. We will modify the text to:

"***Our findings on the Flade Isblink ice cap that the subglacial lake can store multiyear meltwater and affect the ice flow by preventing the transfer of meltwater to the ice sheet margin are helpful for better understanding the hydrological processes on the GrIS.***"