Our responses to the referees are listed below in bold, blue text.

Referee 2 – Anonymous

Review of Menounos et al.

I have reviewed the submitted manuscript and believe that this is a nice complementary study that expands on the current understanding of mass change in Western Canada. The text would largely benefit from some further clarification and correction of some inconsistencies and after some minor revisions, suggested below, I would endorse this for publication.

We thank Referee 2 for taking time to review our brief communication and for providing helpful feedback that we feel strengthens our paper.

L15: Alaska --> would it be better to change this to Yukon/Alaska (or Alaska/Yukon)? Many global assessments of glacier mass balance term the region Alaska but the actual areas tend to include regions that fall within the Yukon as well (and perhaps portions of Northern British Columbia).

We see the issue that the referee raises here, but given the precedent on the usage of the term 'Alaskan Glaciers' we prefer to use Alaska. To our knowledge, Arendt et al., 2002 (Science, 297(5580):382-6. doi: 10.1126/science.1072497) was one of the first to use this term to include Alaskan glaciers and those on the eastern Alaskan border. The glaciological community has largely adopted the naming convention of the Randolf Glacier Inventory (RGI) in which region 01 (Alaska) represents Alaskan glaciers and those that straddle the Alaska-BC, Alaska-Yukon borders. Since we use glacier extents from the RGI, we wish to maintain terminology that is consistent with this global mapping exercise.

L23: Same comment as above.

Please refer to our previous comment.

L31-32: Regarding Terra's orbit, is there a study or technical document that you can cite here for this statement?

We now provide the Terra website (https://terra.nasa.gov/) that describes the orbital degradation and attempts to extent Terra as long as possible.

L33-34: Could you had some contextual information as to why glaciers in western North America have been excluded in these global studies? Also, on L33, the authors note recent studies that leverage laser altimetry and then cite Jakob and Gourmelen (2023). However, this study utilizes CryoSat-2 data, which is a radar altimetry, so this sentence should be revised.

A fair point and now one that we addressed under Referee 1's comments. A good catch on the term 'laser altimetry'! We now omit 'laser' from this statement.

L47-53: The reference Copernicus DEM is derived from TanDEM-X SAR data, but due to the penetration of the SAR signal into snow/firn/ice, it is unlikely that the surface elevations over glacier in this dataset represent the true surface glacier height – particularly in accumulation areas (probably a negligible problem in ablation areas). This is likely to be unavoidable, but can the authors provide some comment on this and how the penetration of the SAR signal is likely to impact the DEM generation. Are there any optically derived DEM sources that can be used?

A fair point and one that we acknowledge in the paper here and in the discussion (lines 156-161) section of the paper. Based on the area-altitude distribution, we expect the total area impacted by penetration bias to be low (1-2 percent). The close correspondence between our estimates of COP30 - ICESat-2 mass change estimates with those derived from optical imagery (Hugonnet et al., 2021) also suggests that the potential bias of the penetration to be negligible. We did observe a significant seasonal bias in our analysis when we include all ICESat-2 data which we believe is due to the presence of seasonal snow.

L02-16: Results – Can the authors comment at all on how the in situ mass balance data within the region compares with these results (mass balance records form Peyto, Place and Helm Glaciers)?

This is an interesting question but one where the sparse number of laser shots for these glaciers does not allow us to directly compare our results from either approach (i.e. COP30/ICESat-2 or ICESat-2 /GEDI) since the altitude distribution of elevation change for a given glacier is low. The regional average mass change for the Southern Coast Mountains from both approaches is comparable (about 1.0 m w.e. loss per year) to mass change from these three glaciers, however.

Page 4, L6: (Fig. 1 is missing a closing bracket.

Corrected.

P5: L24-27 – Here is the mention of the penetration bias in the SAR derived DEM. I suggest that this be moved into the description of that dataset (as identified above) so that I comes earlier in the text.

This point is now brought up in the data description and discussion section of the paper.

L25: There is some inconsistency here, earlier in the text it is TanDEM-X while here it is Tandem-X, check for consistency throughout.

A good catch. We corrected this and now use 'TanDEM-X' throughout the paper.

L17-29: Discussion and Conclusion: Here I would suggest that the authors be a bit more detailed in their descriptions about why this work is important. Glaciers in Western North America are often overlooked in the global assessments, but are key sources of water for communities and for agriculture in these regions. So, these dedicated and more detailed assessments that investigate these glacier changes in more precise detail within these regions are fundamentally important. I would suggest that the authors make this more apparent in the text.

A valid point; we now add information that acknowledges this point both in the introduction and at the end of the discussion of our paper.