Reply to ‘Reviewer comments’ from RC 2 Benjamin Laabs

We thank RC 2 for their thoughtful and constructive review of our manuscript. We have gone through and replied to each individual comment. Below, please find the original comment bolded and italicized and our reply in normal font.

***Line-by-line comments:***

***Line 50: the Uinta Mountains are part of the Middle Rocky Mountains physiographic province and probably do not need to be singled out here (although they are awesome and have a fantastic glacial record).***

Removed the Uinta Mountains as a singled out entity

***Line 53: could probably state “Latest Pleistocene or Early Holocene” here, as Marcott et al. found that some cirque floor moraines were abandoned as early as 15 ka. Additionally, basal $^{14}C$ ages from lake sediments inboard of cirque-floor moraines are Pleistocene in age in some mountains (see records published by J. Munroe for the Uinta Mountains (Munroe and Laabs, 2017) and by J. Munroe and others in the Ruby Mountains).***

Replaced “by the start of the Holocene” with “during the late glacial-to-early Holocene”

***Lines 90-93: should cite some of the earlier, original reports on the glacial record in southern Colorado and northern New Mexico. Jim McCalpin did some work in the region (mostly the Sangres) in the 1980s and Keith Brugger has done more recent mapping in the Sawatch.***

Thank you for bringing these citations to our attention. We felt it would be most appropriate to add the following citation to the list since we are only citing summary papers here:

Laabs, B. J. C., Licciardi, J. M., Leonard, E. M., Munroe, J. S., and Marchetti, D. W.: Updated cosmogenic chronologies of Pleistocene mountain glaciation in the western United States and associated paleoclimate inferences, Quaternary Science Reviews, 242, 106427, https://doi.org/10.1016/j.quascirev.2020.106427, 2020.

Although we did include the following citation to the section specifically discussing the Sawatch Range:

Brugger, K. A., Ruleman, C. A., Caffee, M. W., and Mason, C. C.: Climate during the Last Glacial Maximum in the Northern Sawatch Range, Colorado, USA, Quaternary, 2, 36, 2019a.

***Lines 101-104: the Guido et al. cosmo ages are pre-CRONUS (and also pre-really***

***good AMS measurements) and probably should be recalculated in order to accurately compare with more recently published cosmo ages from southern Colorado. If you've already done this, then it's worth specifying here. If not, the Guido et al. ages are available in ICE-D.***

Thank you for the suggestion. we recalculated the ages and updated some of the text to reflect those changes. In addition, we added the following phrase at the beginning of the section:

"ages discussed below are re-calculated using the promontory point production rate calibration of Lifton et al. (2015) and the LSD*n* scaling model of Lifton et al. (2014)"

***Line 174: prior to this paragraph, consider adding a paragraph about how exposure ages of glacially scoured bedrock are related to ice margin position and some potential limitations of dating these to track ice retreat compared to moraines. As you know, glacially scoured bedrock surfaces that protrude above the valley floor (forming smooth and easy-to-sample surfaces) represent places of minimal scour depth, which can result in an inheritance problem. The Bayesian approach helps to sort this out by accounting for relative age differences, but even so, the potential for inheritance is greater than for most other applications of cosmogenic dating and should be acknowledged. Snow cover is another important consideration along valley floors and should be acknowledged if not assessed.***

Thank you for the suggestion. We added the following sentence to the end of the first paragraph in the section to acknowledge that incomplete erosion is an issue when it comes to exposure ages on glacially sculpted bedrock:

"Bedrock surfaces located in the bottoms of valley floors – where glacial erosion is maximized – were specifically targeted since the potential for incomplete scouring of these surfaces can lead to inherited nuclides and ages that are older than expected."

In addition, we did not choose to make any corrections for snow shielding nor post-depositional bedrock erosion and we acknowledge that with the following sentence at the end of the second paragraph in the section:

"We do not attempt to make any corrections for snow cover or post-depositional bedrock surface erosion."

***Lines 189-211: consider reorganizing the reporting of ages here. The bedrock exposure ages are reported first, then the exposure ages of recessional moraines/young modes of terminal moraines, and then the bedrock ages are described again. Perhaps starting with the moraine ages (or including them in a previous section) and focusing just on the bedrock exposure ages here would improve the flow of this section and a smoother transition to the retreat rates in the subsequent paragraph.***

Thank you for the suggestion. We agree that it is a little awkward reporting the moraine ages here in the results when we did not date them in this study. So we decided to move this paragraph to the previous section (2. Setting).

***Lines 233-242: the statistical reasons for excluding four exposure ages are explained well here, but the most likely reason that some exposure ages fall out of stratigraphic order, inconsistent exposure between sample sites, is not. As noted in a previous comment, the potential limitations of bedrock exposure ages should be acknowledged.***

We added in the following sentences to the end of the paragraph that hopefully convey the potential issues associated with each suspected outlier:

"Two suspected outliers are older than expected, which may have been caused by insufficient glacial erosion leading to inheritance. The two remaining potential outliers are younger than expected, which could have resulted from excessive soil and snow cover, enhanced post-depositional bedrock surface erosion, or erosion and removal of overlying sediments, or a combination of these factors."

***Lines 243-255: I can't see the reason for using NENA-Lm as an example of another production/scaling model for high altitude sites in western NA. The NENA calibration site is far away and much lower in elevation, and I think the reason for using it in some earlier studies in the mountain west was to illustrate the effects of lower SLHL production rate (which started to appear in the literature circa 2010) on exposure ages. Perhaps a better option would be to compare the ages computed with the Promontory Point calibration/LSDn scaling with ages computed with a globally averaged production rate and LSDn scaling, or just show the effects of using different scaling models with the Promontory Point calibration? This would better illustrate the degree to which the choice of production rate affects exposure ages, which I assume is what the authors are doing here.***

The goal of using NENA here was to show it as a sort of an 'end-member' production rate since it is relatively low (especially compared to PPT). In addition, it is the other production rate (or series of rates) that exist from North America. We did originally calculate the ages using the default PR in CRONUS and those ages fall somewhere between PPT and NENA. We wanted to emphasize that even if we used a relatively low PR that produces ages which are 9 – 12% older (e.g. NENA), there is still a significant delay in deglaciation compared to the time of global warming and $CO_2$ rise.

We rephrased the beginning of the final paragraph in section 5.1 to read:

"Although we interpret our results using the Promontory Point production rate calibration site (Lifton et al., 2015) and the LSD*n* scaling scheme (Lifton et al. 2014), we calculate exposure ages with other commonly used calibration sites for North America (e.g.

northeastern North America NENA; Balco et al., 2009 and the 'global' production rate; Borchers et al., 2016) and another commonly used scaling scheme (Lal/Stone–Lm; Lal, 1991; Stone, 2000). Samples used for the NENA production rate calibration range in elevation between ~50 to 400 m asl and are located ~3000 km northeast of the Sawatch Range. This combination produces the oldest ages given the previously mentioned reasonable production rate calibrations and scaling schemes, and are between 9 to 12% older than when using PPT/LSDn (all other combinations fall somewhere in between; Fig. 4; Table 1)."

***Lines 267-269: should probably cite Young et al. (2011) at the end of this sentence.***

Done

***Fig. 1: this is a beautiful map! As you reference some other glaciated mountains in the western U.S. in the introductory paragraphs, consider labeling some of the ones shown on the map along with pluvial lakes.***

We originally chose not to label all of the other glacial centers in the western US since we did not discuss chronologies from any other location outside the southern Rocky Mountains.

***Fig. 2: seems like a good idea to show all the terminal moraine cosmo ages instead of just the young mode at Pine Creek, given that the terminals are the "starting point" for ice retreat? Just a suggestion; I understand that you're emphasizing the onset of ice recession in this paper, not the glacier maxima.***

While we do agree that this might be valuable, we did not originally report individual moraine ages in the text (rather an approximate age range) for the culmination of the LGM in our field area since, as you mention here, we are only focusing on deglaciation rather than the LGM. That said, we will report the LGM moraine ages on the figure and mention in caption that the ages are mean ages from moraine boulders reported in Schweinsberg et al. (2020).

Lake Creek terminal moraine: 20.6 ± 0.6 ka
Clear Creek terminal moraine: 20.0 ± 1.0 ka
Pine Creek terminal moraine: 22.3 ± 1.3 ka

***Fig. 5: may want to consider a more recent and focused assessment of the Bonneville hydrograph in Oviatt (2015) or some of the specific discussions about the duration of the Provo phase of the lake by D. Miller (2016).***

We did not find large enough differences between the Reheis et al. (2014) lake level curves and those from Oviatt et al. (2015) and D. Miller (2016) that would significantly alter our interpretations since the timing of North American ice sheet separation, lake level lowering and Sawatch Range deglaciation are currently only loosely correlated.