Response to all reviews

Review 1:

Response to Reviewer 1 Benedikt Ritter

We thank Dr. Ritter for their constructive review of our manuscript. Below, please find each review comment in **bold** and our response in *italics*.

General Comments

The manuscript is well written, and the reference list is complete. I have only one point to make regarding the data analysis and subsequent interpretation. First, you should clarify that you have dated 15 rock samples and with the double measurements of the moraine 3 sequence, you have 18 exposure ages. While the exposure ages for moraine 1 and 2 and their averages are fine, the data for moraine 3 need to be checked again, especially the average. Currently you are underestimating exposure age of sample 22LAS-09 and LAS22-10, as all other samples were measured twice and according to the manuscript all exposure ages (8) are used to calculate the average. While checking the data, there is a misunderstanding between the use of all 8 ages and the calculate average of moraine 3.

It looks like that you took the arithmetic mean of the double measured rock samples and then calculated the overall average, as the average over of all 8 exposure ages would be older than stated in the manuscript. Furthermore, I would also recommend that you only plot the averaged exposure ages in the pdf plot. You should end up with a bimodal age distribution, with a peak at around ~14ka and ~19ka, roughly. This bimodal age distribution then needs to be discussed (pre-exposure, reworking etc).

We thank the reviewer for highlighting this apparent discrepancy. As they pointed out, before calculating the age of the moraine, we calculated the average age of each individual boulder that had replicate measurements (3 boulders, 22LAS-08, 22LAS-11, and 22LAS-12) and then used those average boulder ages (along with the calculated ages of the other two boulders with just one measurement on them, 22LAS-09, 22LAS-10) to determine the moraine age. This is essentially what the reviewer is showing on the right-hand side of the Excel table in their review. The way the manuscript is worded and Figure 6 and 7 displayed (all 8 measurements have their own camel plot), it would appear that we simply took all 8 measurements and calculated an average of the 8, even though that's not what we did. We *propose to keep the method the same, but update the wording and Figure 6 and 7 to reflect that we took the first approach outlined above. The reason we want to keep it that way is because we suggest the difference in ages on the replicate boulders (e.g., LAS22-08 has 15.5 ka and 12.2 ka ages that are ~3.3 kyr offset) has more to do with measurement uncertainty and reproducibility during the measurement process than it does any geologic uncertainty. The quartz measured in each replicate comes from the same processed material so geologic uncertainties (e.g., sampling from different locations on the boulder surface) would not impact the measurements. Thus, we suggest it makes more sense to address measurement uncertainty on an individual boulder basis before determining the geologic age of the whole moraine.*

We have updated figure 6 and 7 to include updated camel plots for the LC-3 moraine and included the following description in the results section –

Line 280: "Finally, to determine the average age of the LC-3 moraine 4 km up-valley from LC-1 and LC-2, 8 ages from 5 boulders (with replicate measurements on 22LAS-08, 22LAS-11, and 22LAS-12) from the innermost recessional moraine dated in the Lost Creek Drainage (LC-3) were calculated. First, we calculated the average age and root mean square error – propagating each individual measurement uncertainty and the 1 standard deviation of the two ages – for each individual boulder that had replicate measurements (3 boulders, 22LAS-08 at 13.9 ± 5.2 ka, 22LAS-11 at 20.5 ± 7.7 ka, and 22LAS-12 at 18.5 ± 5.2 ka). Then, we calculated a simple arithmetic mean moraine age for LC-3 using those average boulder ages along with the calculated ages of the other two boulders with just one measurement on them (22LAS-09, 22LAS-10). Thus, the average age of the LC-3 moraine using this approach is 15.3 ± 3.8. We suggest this approach because the difference in ages on the replicate boulders (e.g., LAS22-08 has 15.5 ka and 12.2 ka ages that are ~3.3 kyr offset) is likely more impacted by measurement uncertainty and reproducibility during the mass spectrometry measurement process than from any geologic uncertainty. The quartz measured in each replicate comes from the same processed material so geologic uncertainties (e.g., sampling from different locations on the boulder surface, grain size, etc.) would not impact the measurements."

Please add, that you have extracted the LSDn calculated exposure ages from ice-d.org.

Added at line 328 along with more description of the workflow for extracting exposure ages from ICE-D (see below).

Line 9: Rather write 21Ne in quartz. Similar to Balco et al. 2019.

Changed.

Line 11: Without hyphen

Changed.

Line 23: Maybe this needs one more sentences for explanation for the non-cosmo reader

We updated the preceding sentence and added one more sentence to clarify why glacial erosion and transport are important mechanisms (see below).

Line 24: "… remove and thereby reduce exposed or partly exposed rock mass from previous exposure and…"

We reworded the sentence to say: "Glacial erosion and transport are important processes that act to remove exposed or partially exposed rock mass containing inherited cosmogenic nuclides from the landscape and thereby expose previously shielded rocks and sediments to the cosmic ray flux at Earth's surface (Balco, 2011)."

And added the following sentence immediately preceding: "Thus, the inventory of cosmogenic nuclides on the landscape is reset and the accumulation of new nuclides tracks with the timing of glacier recession and exposure."

Line 25: Just Pleistocene

Removed "-age"

Line 27: Add in brackets half-life

Added in the following: (1.386 x 10^6 yr; Chmeleff et al., 2010) and added the reference to the reference list.

Line 29: production rates of 10Be allow to date also young Pleistocene samples with AMS

Noted, but the reference PR for 10Be is the lowest of the commonly measured cosmogenic nuclides so this would technically not be a strength for 10Be compared to other nuclides.

Line 40: without hyphen

Removed.

Line 52: Maybe add the dating method and mineral in brackets

Added in the following: "40Ar/39Ar age; Turrin et al., 1998)

Line 72: 18 new cosmogenic

Added.

Line 72: see comment above. I would write it without hyphen

Removed hyphens.

Line 87: use scientific writing x10^x. Maybe this needs a reference.

Changed to scientific notation and added relevant references (Niedermann, 1993, 2002).

Line 142: your analysis or from any reference mentioned before? If so please add here.

From previous work. Added the relevant citation (Clynne and Muffler, 2010).

Table 1: Total. Different font size. Round to significant digits. I assume 1 sigma? 1s or 2s, please indicate in the top or somewhere else.

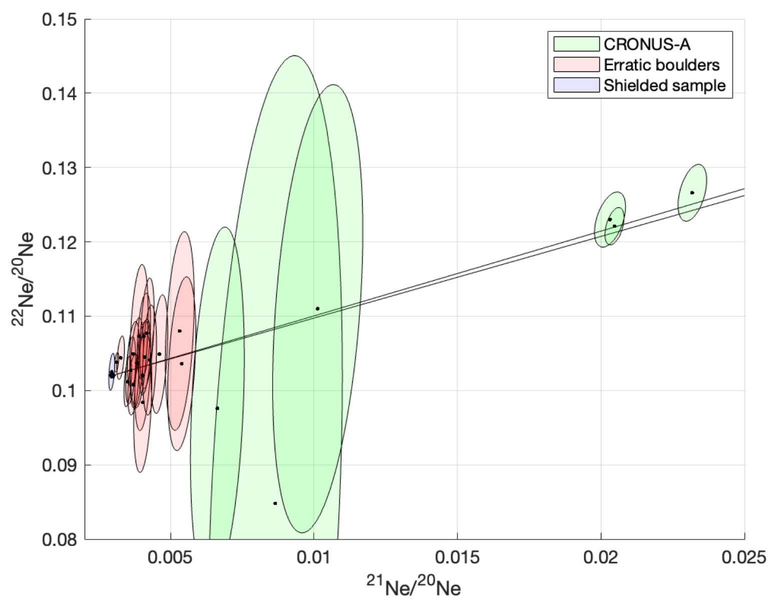
Table 1 has been updated to reflect the changes suggested here.

Line 218: check wording

Wording is correct, we did some steps at BGC and then some steps at UB, we now note where each took place in the preceding sentences in parentheses.

Line 243: It is possible to add this standard measurement to the triple isotope plot?

The X/20Ne ratios are much higher for CRONUS-A than for the Lassen samples, so if we expand the axes to include both, the Lassen data become narrow at one end of the figure and difficult to discern:



Note that two heating steps are shown for each of the CRONUS-A samples measured with the Lassen samples – in contrast to the Lassen samples, the second heating step of CRONUS-A generally contains a small, but nonzero, amount of excess Ne-21. In addition, the neon isotope ratios of CRONUS-A are already very well documented, and our measurements lie on the atmosphere-cosmogenic mixing line as expected, so minimal new information would be added. Thus, we have focused the figure on the Lassen data to clearly highlight the contrast between shielded and unshielded samples. We note that all *the isotope ratio data for the simultaneously measured CRONUS-A samples are reported in the supplement, so a reader can verify that standard measurements lie on the expected mixing line.*

Line 244: I suggest using scientific writing x10^x

Changed.

Line 250: reference for this value needed.

Cited and added the following reference to the reference list:

Dunai, T. J.: Cosmogenic nuclides: principles, concepts and applications in the earth surface sciences, Cambridge University Press, 2010.

Line 262: mark in Figure 5 Air for non-specialists.

We agree that this would increase clarity and have added an annotation in the figure to mark neon ratios of air.

Figure 5: If possible add also the Cronus A measurement data points in this plot. Check x and y axis number of digits. Mark it better (atmospheric neon) to be visible.

See response to previous comments above

Line 278: average to 22.1

Changed.

Line 280: average to

Changed.

Line 281: You use 8 ages from five boulders to calculate the average. However, with this approach you put more weight on the doubled measured samples, and thereby reducing the impact of, for instance 22LAS-10 with only 9.9ka. It would be better if you discuss the age distribution and explain the difference in some of the doubled measured samples. You can plot all pdf of moraine 3, however then mark which data you used. I would only use one age per sampled boulder. Maybe this also makes the entire pdf function more clear.

We hope that the suggested fixes we made to the first comment in the review address this comment as well.

Line 328: So you just exported the published exposure ages? So no general re-calculation that the entire datset uses the same scaling scheme for instance? This would be recommendable for a better comparison. Additionally some of the ages indicate pre-exposure being similar to dated boulders from the other moraines. This should be discussed more in detail.

We thank the reviewer for highlighting an opportunity to be clearer with our wording. We add the following sentences starting at line 329:

"ICE-D is capable of dynamically re-calculating cosmogenic nuclide exposure ages using internally consistent scaling schemes and the default production rate calibration from version 3 of the online exposure age calculator (https://hess.ess.washington.edu/). All ages extracted from this study were automatically re-calculated using the LSDn scaling scheme (Lifton et al., 2014) and default production rate from the online exposure age calculator (Borchers et al., 2016) to provide a consistent comparison between our samples and those measured in previous studies."

We also add the following statement in regard to a few minor instances of potential pre-exposure from previous studies starting at line 379:

"Some previously dated moraines contain a few ages older than the rest of the population of samples (e.g., WHITEBR2D and WHITEBR2B) which may indicate some instances of nuclide inheritance and insufficient erosion to reset the cosmogenic nuclide inventory in the rock surfaces, but we consider those instances to be minimal."

Review 2

Response to Reviewer 2 Anonymous

We thank anonymous reviewer 2 for their constructive review of our manuscript. Below, please find each review comment in **bold** and our response in *italics*.

1. In addition to cosmogenic, nucleogenic, and atmospheric neon, quartz may also contain a "crustal" neon component, in particular in fluid inclusions (see e.g., Niedermann, 2002). Although the presence of crustal neon can be ruled out in young volcanic rocks (such as those used in this study), its potential presence should at least be briefly mentioned in the introductory paragraph (where the different neon components are described).

The term "Crustal neon" in this context was introduced by Kennedy and others (1990, EPSL), and is essentially just a special case of nucleogenic neon in which the nucleogenic production did not happen in situ, but somewhere else. Also, as the reviewer notes, if 'crustal neon' was present in a magma, it would be called 'magmatic neon' instead. To avoid confusion and make the paper easier to understand, therefore, we have not mentioned it.

2. Please give the original grain size of the quartz phenocrysts present in your samples (near line 139). Is there any relation between grain size and atmospheric neon content in the studied samples?

We add the following information regarding grain sizes of quartz phenocrysts in the dacite of Lassen Peak:

Line 139: "… with 30% phenocrysts of quartz with grain sizes ranging 1-2 mm (up to 10%)…"

Regarding grain size and atmospheric neon content, we prepared all samples for measurement in the same fashion by isolating the 300-600 micron fraction of the crushed material and measuring undifferentiated aliquots of grains within that grain size range. Thus, we are not able to determine any relation between grain size and atmospheric neon in the measured samples.

3. Please provide the length, width and height of each sampled boulder in Table 1. Studies like those of Heyman et al. (2016, http://dx.doi.org/10.1016/j.quageo.2016.03.002) are highly relevant and would be impossible without this information.

This was not an original objective of the field sampling campaign, but field photos provided for each sample are available online on the ICE-D webpage and readers could use them to estimate boulder dimensions if desired.

Line 1 (Title): Please consider removing "Cosmogenic" from the title (exposure dating is per definition using cosmogenic nuclides).

We would argue this is not technically true. 'Exposure dating,' meaning the determination of how long a rock surface has been exposed to the Earth surface environment, can refer to multiple dating techniques including lichenometry, dendrochronology, weathering rind thickness, Schmidt hammer hardness measurements, obsidian hydration dating, and other creative methods. We prefer to keep the term in the title.

Abstract: I suggest to mention the three exposure ages (and uncertainties) obtained for the three moraines in the abstract (instead of giving only the ~22-15 ka range).

Updated.

Line 21: I suggest to write "... can record the extent and timing of ..."

Updated.

Line 85-88: This sentence needs 1-2 refs at the end.

Cited Niederman, 1993 and 2002 here.

Line 99: The three cited references mainly report 21Ne exposure ages of millions of years. Please add 1-2 refs with 21Ne exposure ages in the range hundreds of thousands of years.

While these studies report 21Ne exposure ages in the millions of years range, we note that the Ne-21 exposure ages reported in the Balter-Kennedy paper range from 440 ka to > 10 Ma. The compilation in Spector et al. also includes numerous Ne-21 exposure ages < 1 Ma.

Line 120: delete "complex", please.

Removed.

Line 126: I suggest to write: "The Eagle Peak sequence consists of geologic..."

Updated.

Line 146: reference needed for the second part of sentence.

Citation applies to the entire sentence so we will move the Klemetti and Clynne, 2014 reference to the end.

Line 147: I suggest to write "abundance" instead of "abundant availability".

Updated.

Line 151: "surrounding landscape" of what?

Updated the sentence to clarify the landscape surrounding Lassen Volcanic National Park:

"…it is likely that large ice caps smothered the landscape surrounding Lassen Volcanic National Park (Fig. 2; Turrin et al., 1998)…"

Line 190: I could not find the field photographs of the boulders on the cited webpage.

The link will be updated in the final version of the paper to direct to the publication page in ICE-D where links to individual sample pages can be found.

Line 224: Please write "... through decay of U and Th in adjacent...." (the ratio doesn´t matter).

Updated.

Section 3.2: Please add the information that the measured 20Ne and 22Ne concentrations are given in the supplement table, because you do not give them in Tab. 1.

Updated the first sentence of 3.2:

"We measured all three neon isotopes (20Ne, 21Ne, 22Ne) in purified quartz on the "Ohio" noble gas mass spectrometer (NGMS) system at the Berkeley Geochronology Center (see supplement for complete data report)."

Line 244: Please add uncertainty to the value of 320 (i.e., ±11).

The uncertainty is not included here because it was not used for normalization. As CRONUS-A is not a primary or certified standard (i.e., its absolute Ne-21 concentration is not known from first principles), the interlaboratory scatter quoted by Vermeesch is not a meaningful quantification of how accurate the 'accepted' value is. All the laboratories contributing to the Vermeesch study could have agreed on a value which is totally incorrect, and the interlaboratory scatter would not give any information about the likelihood of this outcome. The purpose of normalization to the CRONUS-A standard is simply to normalize all data to a specific assumed common value, not to represent that the normalized value is more accurate in an absolute sense. To do this normalization, it is appropriate to use the uncertainty in the CRONUS-A measurements from our lab (the 336.1 +/- 8.1 quoted in the previous sentence), because that implies an uncertainty in the lab-specific normalization factor. However, it is not appropriate to apply an uncertainty in the common value used for normalization, because this would have a 'double-counting' effect resulting in a spuriously large uncertainty in comparing normalized data from two different labs. The common value for normalization isn't a measurement, it's an assumption, so it is considered to have a fixed value.

Line 267: please add references to support your statement.

Unfortunately, we are not aware of any paper that compiles concentrations of atmospheric neon determined as a byproduct of cosmogenic Ne-21 measurements. Furthermore, these values are rarely explicitly calculated and/or discussed in papers. In nearly all cases it is necessary to make separate calculations of this quantity from data tables, making these *papers unsuitable as references in this context. Thus, overall, there is not really any suitable reference for this point.*

There is a compilation of atmospheric Ne-21 concentrations in quartz measured in the BGC lab prior to 2010, with some other published data, here:

https://cosmognosis.wordpress.com/2010/03/12/is-ne-21-worth-bothering-with-for-exposure-dating-part-i/

which is quite interesting (and supports our point in this paper), but is not well documented and not peer reviewed. However, we could cite this if necessary.

Line 271: Please add a reference for the mixing line in the caption.

Added the following reference: Niedermann, 2002

Line 312: I suggest to explain in more detail what Phillips et al. (1998) did and to describe other applications to give the reader a better idea what can be done with cosmogenic 21Ne in volcanic rocks.

Added the following phrase here at line 312:

“for example, see work on depth-profile dating of alluvial fans composed of the Bandelier Tuff in New Mexico, USA by Phillips et al., 1998”

Also added the following sentence at the end of the section starting at line 320:

“Topics of study could include further glacial history reconstructions as presented here, long-term fault slip rates by measuring offset surficial features (e.g., Rood et al., 2011), and volcanic histories (e.g., Valentine et al., 2019) in such places where young, quartz-bearing volcanic rocks exist.”

And the following citation to the citation list:

Valentine, G. A., Briner, J. P., van Wyk de Vries, B., Macorps, É., and Gump, D.: 10Be exposure ages for the Late Pleistocene Gour de Tazenat maar (Chaîne des Puys volcanic field, Auvergne, France), Quaternary Geochronology, 50, 8–13, https://doi.org/10.1016/j.quageo.2018.11.002, 2019.

Line 315-316: If you would analyze 2-3 aliquots from each sample and then take the weighted mean of the 21Ne concentrations, the uncertainty could be reduced. This possibility could be briefly discussed here.

We added the following sentence to the paragraph starting at line 320 (before the sentence added above):

"Moreover, measurement uncertainties can be slightly reduced, for example, by measuring multiple aliquots on individual samples and taking the weighted mean (similar to what was done for samples 22LAS-08, 11 and 12), which could be a worthwhile endeavor in future work if time and resources are available."

Line 376: range FROM ~23 to 18 ka

Updated.

Line 393: please specify "the region" that is meant.

Updated to say "Cascade Range and Sierra Nevada"

Table 1: The amount of quartz analyzed from each sample is quite variable (from 0.27 to 0.53 g). Wouldn´t it be better to use as much quartz as possible to decrease the analytical uncertainty?

In principle, yes. In practice, there is a tradeoff between sample size and measurement efficiency; the most time-consuming part of the analysis process is pumping the sample chamber to high vacuum, and using larger samples requires more time spent changing samples and pumping out, and less time spent measuring. In this trial study, the sample sizes were largely determined by these efficiency considerations as well as variations in sample size and petrology and the efficiency of quartz extraction. If more resources were available for a future study, more or larger samples could be analyzed.

You give a density of 2.8 g/cm3, which I think is too high for a dacite. Did you measure the density, or did you estimate it from the density of the individual minerals in the rock. The latter approach probably neglects that the groundmass has a rather low density.

The density is estimated from the mineral composition but as noted here, it is possibly an overestimate and we will update to a value of 2.6 g/cm3, a likely more accurate value. However, it should be noted that changing the density even to 1 g/mL changes the calculated age by ~<1%, well within the measurement uncertainty.

References: Please use capital letters in journal names on lines 431, 450, 482. There is a typo in line 473 (i.e., USAu).

Updated and will work with the copy editors to make sure other minor mistakes throughout are corrected.

Review 3

Response to Reviewer 3 Mark Kurz

We thank Dr. Kurz for their constructive review of our manuscript. Below, please find each review comment in **bold** and our response in *italics*.

-Since the paper is method oriented, and quite valuable for that reason, additional information should be added to "Analytical measurements", on page 11. The text here, or supplementary data table, should provide the size of the air standards used as primary standards, reproducibility of the air standards, and the size of the blanks and hot blanks. The paper mentions corrections for doubly charged contributions to 21Ne and 20Ne but does not give any indication of the magnitude or difficulty of the corrections for this sample suite.

To note, the air standards are in the range 5×10^{-16} - 2×10^{-14} mol Ne, their reproducibility is about 2% (which is included in the uncertainty propagation, although in this case minor compared to the uncertainty produced by the atmosphere subtraction), the cold blanks are close to negligible and << 1% of sample signals, and the hot blanks are significant but have isotope composition indistinguishable from atmosphere, so are not corrected separately but included in the atmosphere correction. We can add this information to the text.

The isobar correction procedure is described in the Balco and Shuster (2009) paper and is not any different in this study, so we have not discussed it in detail but can add clarification that readers be directed to this paper for more information. On this mass spectrometer and for these samples, the 40Ar++ correction is a significant (sometimes > 20%) fraction of the mass 20 signal. However, the uncertainty in this correction is fully propagated into the calculation of excess Ne-21.

We disagree somewhat with the reviewer that this paper is method-oriented in terms of the isotope measurements: neon isotope measurements in quartz have been conducted since the 1980's, the method we are using is not very different from that used 30 years ago and is well documented in many publications, and this study does not make any significant changes or contributions to the method. In addition, detection limits and measurement precision are also not that different from 30 years ago, although various automation advances make it possible to collect the data faster. Perhaps it is method-oriented in that Ne-21 exposure dating is an unusual method for LGM-age samples, but all aspects of the mass spectrometry are routine and no different from previous publications. For this reason, we did not include details about the mass spectrometry that are already fairly well described elsewhere, but, as noted above, we can add a few sentences with some of this information/clarity on further reading.

- The paper mentions etching in "low concentration HF" (page 10). What concentration, for how long, and at what temperature?

We added the following phrasing to clarify the etching process:

"…before etching the quartz-rich fractions in low-concentration (~2-3%) HF at room temperature for several weeks to…"

-The paper should give the Sea Level High Latitude 21Ne production rate that is used for the age calculations. The paper references Fenton et al. (2019), and I assume that the value is given there, but this number is a fundamental assumption for all the ages presented, and should be stated explicitly.

We would phrase this slightly differently. The fundamental measurements that underpin the exposure ages are the nuclide concentrations in the calibration samples, not the SLHL production rate. The SLHL production rate is the outcome of a model calculation, not a direct measurement, and has a different numerical value for every different scaling method, numerical algorithm, or code used for the exposure age/production rate *calculations. In particular, there are so many parameter choices (magnetic field models , etc.) that go into the LSDn scaling method that it is highly unlikely that a reference production rate scaling parameter computed using the code that we used (the online exposure age calculator at hess.ess.washington.edu) would be consistent with another LSDn scaling implementation (e.g., CRONUSCalc or CREp). Thus, we agree completely with the reviewer that it is important to clearly reference the fundamental measurements that underpin the production rate calculations, but we argue that we have done this by referencing the calibration data, not a specific, model-dependent, value of the reference production rate. A reader seeking to reproduce our calculations would be able to do this more accurately by beginning with the directly measured calibration data, not with a derived reference production rate. We note that it would be beneficial for readers to have easy access to the calibration dataset, which is available here: https://www.ice-d.org/production%20rate%20calibration%20data/cal_data_set/13 . We will add this link to the manuscript where we cite the calibration dataset.*

-There is some description of the moraines in the text and in the figures, and reference to mapping of Clynne and Muffler (2010). Have there been previous attempts to date these moraines? I assume that these are the first ever dates for these particular moraines, but this is not actually stated in the paper (maybe in the abstract and conclusions).

The only previous attempts to date these moraines are through relative dating approaches and some indirect radiocarbon constraints. We updated the text starting at line 160:

"The only current absolute dates that help constrain the ages of moraines in the LVC containing dacite of Lassen Peak debris come from indirect radiocarbon constraints that bracket the radiometric 40Ar/39Ar date of the dacite of Lassen Peak from Turrin et al. (1998); there currently are no direct dates on moraines in the LVC."

-The text states that the samples are "sourced from the dacite of Lassen peak" (line 188) and the dacite is described briefly on page 5. However, I did not see any listing of the lithologies of the samples. Is the lithology homogeneous throughout the sample suite, or is there some variability? This could be useful, for example in applying the shielded sample history to the others.

The dacite of Lassen Peak is lithologically homogenous and is distinguished from other dacites of the Lassen Domefield by its greater abundance of quartz and by the presence of clinopyroxene. We thank the reviewer for the opportunity to clarify the text to reflect that all *boulders sampled in this study come from the dacite of Lassen Peak and that the dacite is lithologically homogeneous, suggesting the non-cosmogenic neon composition of the shielded sample should be representative of all samples collected.*

At lines 138 and 188, we specified the dacite is 'lithologically homogeneous'

At line 209 we also added the following sentence: "Since all samples were collected on boulders sourced from the lithologically homogeneous dacite of Lassen Peak, it is likely that any observed amount of nucleogenic 21Ne measured in the 'shielded' sample would likewise be measured in the 15 sampled boulders."

-Is there some reason that the authors did not attempt to measure helium in these samples? The methods section mentions that helium was discarded. There are conflicting opinions on helium diffusion rates in quartz and it is generally assumed that helium in quartz cannot be used for dating purposes, but I doubt that this has ever been attempted in volcanic quartz. Did the authors try?

We did not attempt to measure helium-3 in these samples. Although some noble gas labs routinely measure helium and neon sequentially for each heating extraction, we don't do this at BGC because the MAP-215 source tuning is quite different for helium and neon, but the 1990's electronics don't support computer control of the source voltages, so it is not possible to change tuning during a sequence of unattended automated analyses. Typically we would measure helium on a different aliquot in a completely separate analysis series. Anyway, as the reviewer indicates, helium-3 is likely not fully retentive in quartz at earth surface temperatures and is generally not useful for dating purposes. Moreover, there are some measurements of helium diffusion kinetics in volcanic quartz in Tremblay et al. (2018, GCA), which don't indicate that volcanic quartz is likely to be systematically more retentive than anything else. Regardless, we agree that this is potentially interesting, but measuring helium-3 in these samples was not in the scope of this work.

Additional comments:

Line 31-32. It might be worth noting here that 3He has the highest production rate of any cosmogenic nuclide, so not necessarily just time saving and economical.

rephrased the line to:

"...particularly noble gas cosmogenic nuclides like 21Ne and 3He that have higher production rates than 10Be and do not require AMS measurements..."

Line 37: reference is made to data pulled from www.ice-d.org. I went to this address and got the following message: "You are trying to connect to part of version 1 of the ICE-D infrastructure that has been replaced by an updated version at version2.ice-d.org." Update and give additional information on how to find the data that is mentioned.

The link does need to be updated and will be in the final version of the manuscript: Updated the line in parentheses here to:

"(data pulled from ICE-D, https://www.ice-d.org; last access 09/20/2023; query used to pull data available on request)."

Line 87. It is stated that atmospheric 21Ne in quartz is "commonly in the range 50-100 Matoms/g." This is useful information, but a reference should be given. There are many types of quartz, and I suspect there is a huge range.

As we have also discussed in the response to review 2, we are unfortunately not aware of any comprehensive discussion or review of this, and in most cases this parameter isn't even mentioned explicitly in papers. The only attempt to systematically compile this that we know about is a blog entry from 2010:

https://cosmognosis.wordpress.com/2010/03/12/is-ne-21-worth-bothering-with-for-exposure-dating-part-i/

We agree that systematically compiling these data would be valuable and interesting, but it is beyond the scope of the present paper.

Line 105-110 (page 4). The discussion here makes some assumptions about quartz neon diffusion rates. Some references or justification should be given.

We would phrase this slightly differently – really the speculative assumption here is not the diffusion rates (there do exist measured diffusion kinetics for quartz that indicate very high neon mobility at magmatic temperatures), but the partition coefficients. We are implicitly assuming here that (i) when quartz is in hot magma, any neon present is partitioned into the magma and not the quartz, and then (ii) if cooling to surface temperatures after eruption is *rapid, there is not enough time for back-diffusion of any atmosphere-derived neon into the mineral. We're not aware of any attempts to measure neon partition coefficients or partial pressures in magmas, so this is, in fact, totally speculative. However, the empirical observation that noncosmogenic Ne concentrations are low in volcanic quartz would tend to indicate that the neon does tend to be in the magma and not in the quartz at high temperatures. We can clarify this in the revised text.*

Line 266-267. It is stated here that atmospheric 21Ne abundances are "substantially lower than in most other measured rocks to date". As above, some reference or justification should be given for this statement.

Unfortunately, as discussed above and in the other response, we are not aware of any suitable reference that compiles this. A complete compilation of all these data would be really interesting, but is beyond the scope of the present paper.

Figure 7. I recommend larger fonts, if possible, to make this easier to read.

We agree that larger fonts would make the figure easier to read and will update.