Response to Reviewer 2

RC = Reviewer Comment

AR = Author Response

RC 64	Dear Editor and Authors: I appreciate the opportunity to read this Discussion paper that provides new cosmogenic nuclide ages on post-LGM moraines in the Sierra Nevada and attempts to place the glacial history of this area in a broader regional and global context. The new glacial chronologies will be of great interest to those studying the history of this region, and are an exciting addition to the multi-archive, multi-proxy body of work on western North American paleoclimate. I find the new ages to be interesting and well-presented. My primary comments on this manuscript are centered on the treatment of regional proxy information and the interpretations about the broader expression of Heinrich Stadial 1, so this is what I will discuss for the remainder of this comment. I find that the discussion of the temporal evolution of events in the North Atlantic region and the western US is a bit muddled in the discussion. The previously published proxy information that the authors present here could be treated more carefully and clearly.
AR 64	Dear Reviewer #2: Thank you for investing your time in this paper and for commenting upon its strengths and opportunities for improvement. We are grateful to receive these comments.
RC 65	The authors emphasize a very narrow window for “Heinrich Event 1” which they define from a Spanish stalagmite record from Ostolo Cave. This is shown in the graphical abstract and in Figure 8 with yellow shading and labeled “HE1”. The Ostolo Cave record suggests a change in the $d^{18}O$ of the moisture source as well as temperature in an excursion contemporaneous with IRD in the Bay of Biscay (Eynaud et al., 2012) and the change in seawater isotopic composition (Voelker et al., 2009). The Ostolo Cave paper by Bernall-Wormull which you cite, interprets this negative excursion in $d^{18}O$ as follows: “An exceptional light $\delta^{18}O_{speleo}$ excursion centered at 16.2–16.0 kyr B.P. is interpreted to reflect the major phase of HE1 iceberg melting reaching the Iberian Peninsula, which drastically changed the $\delta^{18}O$ composition of regional precipitation.” While this is a precisely dated record, it is a regional expression of the Heinrich Event in Spain and should be taken in context with other North Atlantic records of the event – which Bernall-Wormhull also show in their figure 4. I suggest you also

	compare to the records of IRD and foraminifera d^{18}O which suggest a broader *peak* for meltwater release between ~16.5 and 16 ka – rather than shading this sharp speleothem δ^{18}O excursion as the full expression of the Heinrich Event in your Figure 8 and associated discussion in the text. The record of 231Pa/230Th in North Atlantic sediments suggests that AMOC slowdown began closer to 18 ka, again reaching a minimum close to 16 ka (McManus et al., 2004). Similar timing of freshwater release to the North Atlantic is used in the TRACE and iTRACE transient climate model simulations (He et al., 2011; He et al., 2021). A summary of Heinrich Stadial 1 model results and proxy records for the western US is provided in Oster et al., 2023. This supports that the window that you have chosen for Heinrich Event 1 is too narrow.
AR 65	For clarity, we differentiate between Heinrich Events (such as Heinrich Event 1; HE1) and Heinrich Stadials (such as Heinrich Stadial 1; HS1). As described by Andrews and Voelker (2018) and Heath et al. (2018), Heinrich Stadials are time periods of relatively cold sea-surface temperatures in the North Atlantic while Heinrich Events are the disintegration of the Laurentide Ice Sheet's Hudson Strait ice shelf and the drainage of the land-based ice behind it into the North Atlantic (e.g., Álvarez-Solas et al., 2011; Marcott et al., 2011). Heinrich Events are recorded by layers of ice-rafted detritus (IRD) in the North Atlantic ("Heinrich Layers") with diagnostic characteristics that link those particular IRD layers with the Hudson Strait ice stream (Andrews and Voelker, 2018). Heinrich Stadials typically last a few thousand years while Heinrich Events durations are typically <1 kyr (Hemming, 2004; Andrews and Voelker, 2018) and potentially only ~200-300 years (Francois and Bacon, 1994; Dowdeswell et al., 1995; Thomson et al., 1995; Pérez-Mejías et al., 2021). Heinrich Stadial 1 began at 19.3 ka and ended at 15.3 ka (Heath et al., 2018). The causal link between Heinrich Stadials and Heinrich Events has been challenged throughout the recent literature (e.g., Álvarez-Solas et al., 2011; Marcott et al., 2011; Barker et al., 2015; Bassis et al., 2017), so for the purposes of this manuscript we make our differentiation and treat each separately. The beginning and end of Heinrich Event 1 is less well defined than the beginning and end of Heinrich Stadial 1, but we note that Bernall-Wormull et al. (2021) describe it as lasting from ca. 16.5 ka to ca. 16.0 ka and that Pérez-Mejías et al. (2021) identify it as beginning at 16.17 ka and ending at 15.89 ka. Here, in a revised version of this manuscript, we will adopt a timing for HE1 of from ca. 16.5 ka to ca. 15.9 ka, to provide a more conservative timing of this event (compared with our original interpretation of 16.22 ± 0.04 ka to 16.04 ± 0.04 ka), as suggested by the reviewer.
RC 66	Additionally, there is a long-standing discussion of Heinrich Stadial 1 in the western US and whether it contained 2 phases - one that was overall drier- particularly in the southwest, and one that was much wetter centered on 16 ka. This discussion is

	ongoing, but much evidence has come from the timing of lake high stands and how they have varied across the region (Broecker and Putnam, 2012; McGee et al., 2018; Hudson et al., 2019; Oster et al., 2020). At any rate, the development of Heinrich Stadial 1 in this region has been well-explored in the literature, and that discussion should be reflected here. Following this idea, the discussion on Great Basin Lakes (Section 5.5.3) is oversimplified and under-cited. There have been very nice compilations of Great Basin Lake high stands that include analysis of the timing and geographic patterns and include modeling and other regional syntheses (McGee et al., 2018; Hudson et al., 2019). The presentation of the lake data in Figure 8d is also oversimplified which carries through to the graphical abstract. Only some of the lakes are labeled. It is unclear if only one age is provided per lake or if there are more. This needs to be clarified in the figure and caption.
AR 66	We agree that the discussion regarding a two-phase Heinrich Stadial 1 in the western U.S. is poorly reviewed in the manuscript and that the manuscript should be strengthened in that regard. Likewise, we agree that the manuscript's section on Great Basin lakes is oversimplified and under-cited. We will strengthen this section of the manuscript by highlighting more of the literature on this topic and interpretations therewithin. Again, however, our manuscript is focused primarily on the relationship between Sierra Nevadan deglaciation and Heinrich *Event* 1, not Heinrich Stadial 1. With regards to the labeling of the lakes in Figure 8d and in the graphical abstract, and with regards to the number of dates per lake, we will more explicitly refer readers to Munroe and Laabs (2013) and Ibarra et al. (2014) for this information (or, alternatively, refer readers to our supplement for this information). We feel that including this level of detail in the graphical abstract and Figure 8d would complicate the figures and make them harder to interpret for the reader – yet we fully agree that readers should either have this information or know where to find it.
RC 67	The 6-degree jump (lines 949-950 and other places) appears from Figure 8 to be defined by the high stands of Lake Cochise and Lake Russell or Surprise. However, there are numerous other high stands on your figure prior to 16.4 ka that are further north than Cochise. Cochise and Estancia are also outside of the Great Basin. It is not consistent with the uncertainty on the ages of lake high stands to pinpoint 6 degree jump within a 200 year interval.
AR 67	First, the reviewer is correct that Lakes Cochise and Estancia do not lie within the Great Basin. We will correct this oversight in a revised version of the manuscript by renaming section 5.5.3 "Western U.S. paleolakes" (or equivalent), renaming panel (d) of Figure 8 "Western U.S. lake-level highstands" (or equivalent), and by replacing references to "Great Basin lakes" with references to "Western U.S. lakes".

	Second, while there were numerous lake-level highstands in what is now the western United States between 20.0 ka and 16.4 ka that were further north than 38° N (the latitude of McLean's Cave), the 6° latitudinal jump described in the text and shown on Fig. 8d is in the *southern limit* of lake-level highstands (as stated on line 949 of the preprint). Older highstands at more northerly latitudes do not change the observation that there are no lake-level highstands in the dataset at latitudes less than 38° N after the ca. 15.7 ka highstand of Lake Russell. Finally, with regards to the 6° northward jump in the southern limit of lake-level highstands, it is in the preprint defined by three data points: the highstands of (1) Lake Cochise (at 32° N) and (2) Lake Estancia (at 35° N) and by (3) the ca. 16.2 ka $\delta^{13}C$ minimum in the McLean's Cave speleothem. However, while falling lake levels and rising $\delta^{13}C$ could justifiable be used to argue for a 6° northward shift in the *winter-storm track* or a similar meteorological phenomenon, using the $\delta^{13}C$ minimum as evidence for a 6° northward shift in the southern limit of *lake-level highstands* as we have done was over simplified. In a revised version of the manuscript, we will redefine the 6° northward shift in the southern limit of lake-level highstands to be based on the highstands of (1) Lake Cochise (at 32° N), (2) Lake Estancia (at 35° N), (3) Owens Lake (at 36° N), and (4) Lake Russell (at 38° N). This revised definition will also result in a more conservative ca. 700-year duration for the 6° northward shift in southern limit of lake-level highstands, rather than the ca. 200-year duration we interpreted in the preprint.
RC 68	Provide a citation for the hypothesis on the delay of Owens and Russell high stands due to meltwater (Lines 956-958). It is stated as a fact here but with a question mark in your figure – is this a hypothesis put forward by this paper or elsewhere?
AR 68	We are aware of two relevant citations. In the first, Zimmerman et al. (2011, p. 270) says, "*millennial-scale IRD variability during lake highstands may be the effect of glacial melting*." We argue, by extension, that millennial-scale IRD variability would be associated with millennial-scale variability in meltwater input and thus millennial-scale variability in lake levels. In the second, Munroe and Laabs (2013, p. 56) discuss why some lakes in the southern and southwestern Great Basin obtained lake-level highstands at the same time as the larger lakes in the northern Great Basin – and note "*some southwestern lakes might also have been influenced by… additions of glacial meltwater (Owens, and by downstream connection, Searles)*." We argue that Lake Russell should also be listed here, as it was also fed by glacial meltwater (e.g., Russell, 1889; Wahrhaftig et al., 2019) and its ca. 15.7 ka highstand was also included in the Munroe and Laabs (2013) compilation.

	In revising this section of the manuscript, our primary interpretation is that the southern limit of lake-level highstands shifted 6° northward over ca. 700 years (as mentioned in AR 67) – while noting that the atmospheric reorganization responsible for this migration in lake-level highstands may have potentially occurred in as little as 200 years, using the onset of rising $\delta^{13}C$ in the McLean's Cave speleothem at ca. 16.2 ka as reflecting the start of drier conditions. We will cite Zimmerman et al. (2011) and Munroe and Laabs (2013) when mentioning that the highstands of Lake Russell and Owens Lake were potentially delayed by meltwater input. With regards to the statement on Fig. 8d that says "Highstands delayed by meltwater release from SN glaciers?", we will (1) retain the question mark and (2) change the statement's font color to gray, to visually deemphasize it.
RC 69	Regarding your McLean's Cave age model, there is a paper currently in review that includes new U-series dates for McLean's Cave and a new age model run using the COPRA algorithm (Breitenbach et al., 2012), that I believe shifts the minimum in $d^{13}C$ of this record slightly older than what your Bchron age model put it at and closer to 16.25 ka. I realize this information is not yet accessible, but caution against putting too much stock in the exact timing of the shift in McLean's Cave $\delta^{13}C$ computed from the Bchron model here. This is consistent with the comment to broaden the constraints of the timing on Heinrich Stadial 1 used in this paper, which can be done with the records available in the literature.
AR 69	We agree with the reviewer and, as noted above, will change the duration Heinrich Event 1 from 16.22–16.04 ka (as it is in the preprint) to being between ca. 16.5 ka and ca. 15.9 ka.
RC 70	I am also curious about the emphasis on the ice sheet thinning rather than changes in freshwater flux and AMOC that are frequently modeled and evaluated in discussions of Heinrich Event impacts. This comes through in the discussion of the TrACE results (Example Lines 1005 to 1017) which misses the influx of meltwater to the North Atlantic and the subsequent impacts to AMOC. Other modeled scenarios of Heinrich 1 (McGee et al., 2018; Oster et al., 2023) investigate the cascading influences of this freshwater flux to the North Atlantic.
AR 70	McGee et al. (2018) and Oster et al. (2023) were focused on the Heinrich *Stadials*, not Heinrich *Events*, which we differentiate (based on prior literature) and will better highlight in the revised version of this manuscript. We agree that Heinrich Stadials are closely linked with meltwater flux into the North Atlantic and its impacts upon AMOC, and that our manuscript should mention that fact. Proxy evidence focusing on HE1 (and not on HS1) documents this event as happening between ca. 16.5 ka and ca. 15.9 ka (e.g., Bernal-Wormull et al., 2021; Pérez-Mejías et al., 2021) – just when the $\delta^{13}C$ trend in the McLean's Cave

	speleothem reverses, and not at the beginning of Heinrich Stadial 1, which was at ca. 19–18 ka (e.g., McManus et al., 2004; Heath et al., 2018; Martin et al., 2023).
RC 71	It was surprising to me to equate the changes noted around 16 ka in the proxy records from western North America with the shift in the ice sheet parameterized with ICE-5G between 15 ka and 14 ka in the model simulations. There are proxy records from western North America that cover this interval, including the records in your Figure 8. It is more appropriate to compare the atmospheric changes and drivers in TrACE at 16 ka to the proxy records from 16 ka.
AR 71	Our intention with this passage was to simply highlight the sensitivity of atmospheric circulation to changes in ice sheet geometry in a state-of-the-art climate model. In our revised manuscript, we will address the reviewer's comment by removing the discussion of timing and focus simply on the response of the atmosphere to the LIS's evolving geometry.
RC 72	I think the authors should consider the influences of meltwater flux, SST changes and AMOC shifts and those teleconnections which do align in timing with the shifts seen in western North American proxy records (McGee et al., 2018; Oster al., 2023).
AR 72	In revising the manuscript, we will expand the discussion of Heinrich Stadial 1 in the western United States, as described in AR 66. In doing so, we will describe the evidence for a two-phase HS1 in what is now the western United States, with first phase (HS1a) lasting from 18.0 ka to 16.1 ka and the second phase (HS1b) lasting from 16.1 ka to 14.6 ka (e.g., Smolen et al., 2025). Although, as mentioned above (Ars 65 and 70), we differentiate between Heinrich Stadials and the Heinrich Events themselves (e.g., Martin et al., 2023).
RC 73	Below are more targeted comments by line: Abstract: Line 19 – Does the "60% LGM length" refer to the length of glaciers? Not clear.
AR 73	Yes. We will make this clearer. Also, as it happens, Reviewer #1 has suggested that we replace most of these % LGM-glacier length estimates with ELA estimates and we plan on making that change, as discussed in our responses to their comments.
RC 74	Overall abstract is too specific with place names and is difficult to follow.
AR 74	We (1) removed the place names "Swamp Lake" and "McLean's Cave" from the abstract and (2) revised it for clarity. Please see AR 10 (in our response to Reviewer #1) for the revised abstract.

RC 75	Graphical Abstract – For the great basin highstands – which lakes are you talking about here – what are the ages from? The citations you have are for papers that include multiple lakes, and the high stands vary across the region. Are the ages for individual lake highstands or are they curves for different lakes (multiple points per lake). (Similar comments on Figure 8).
AR 75	With regards to the graphical abstract, we think it should remain uncluttered and easy to read. With regards to Fig. 8d, we propose clarifying these points in the caption and in the supplement. We think labeling all the lakes by name in Fig. 8d (and especially in the graphical abstract) would clutter the figure and make it harder to read – but we agree that specialists should be able to find this information in our manuscript, or find a redirection in our manuscript to where this information can be found.
RC 76	Introduction: The discussion of timing in the introduction needs to be more concrete and include the available age information of the time periods discussed leading into this study. In part this is presented in the setting section, but the information should be included in the introduction to set up your central questions/objectives.
AR 76	Reviewer #1 raised similar points (RCs 14, 15, 16, 24, and 34) – and it indicates that the glacial history of the region (the Tioga 1-4 and Recess Peak terms) needs to be moved up from the "Regional setting" section and into the Introduction. We agree with the reviewers and will make this change.
RC 77	-Line 63 – provide a time range to go with "broadly in-phase with the global Last Glacial Maximum".
AR 77	Reviewer #1 also raised this issue (RC 15) and we will make this change.
RC 78	Lines 66-71 – please also provide timing estimates for these advances and retreats based on the references included here. It is important to situate your discussion of Heinrich 1 in time.
AR 78	We agree and will include the estimates and references in the revised manuscript.
RC 79	-Line 82 – first mention of the Tioga 4 glaciation – need to define what the timing of this glaciation is understood to be for non-specialists.
AR 79	We agree.

RC 80	-Line224 – do not think you need to include the phrase "is especially relevant to this manuscript". You can explain the proximity to your sites and make the relevance apparent.
AR 80	We will revise the manuscript as suggested.
RC 81	Line 617 – it is more typical to report 2 sigma uncertainties for U-series ages.
AR 81	Thank you for this comment. 1-sigma uncertainties are more common for cosmo dates. Because this paper touches upon both disciplines, we'll be explicit about our level of certainty though the manuscript, clearly identifying 1 sigma vs. 2 sigma uncertainties.
RC 82	Lines 668-674 – This reads a bit too colloquially – the discussion prior to this centers on why the different calculators may be returning different ages. But then these sentences dismiss this question and say that it is not the central task of this paper to figure this out. Suggest rewriting to this to emphasize what is important, rather than what isn't and to tone down colloquial language.
AR 82	We will edit the paragraph to emphasize what is important. The new paragraph will read: "We hypothesize that the ~5 % age difference is the result of the calculators using different algorithms for converting a suite of calibration samples into a single value (be it a non-dimensional LSDn scaling factor or a SLHL production rate) for scaling to other locations. In this manuscript, we focus on two tasks: (1) assessing which calculator is more likely producing more accurate ages for the samples in the dataset we report; and then (2) placing these dates within the context of previous paleoclimate research."
RC 83	Overall, I suggest fewer parenthetical observations like the example in Lines 682-683. These are distracting and are used unevenly. Just include ideas in the sentences proper.
AR 83	We will revise for clarity. The new sentence will read: "If we accept the interpretation that Tuolumne Meadows deglaciated before 15.4 ka, as suggested by the Greenstone Lake radiocarbon date (Fig. S2; Clark et al., 2003), and that the high-elevation lake basins of the Sierra Nevada deglaciated at 15.75 ± 0.5 ka (Phillips, 2016), as suggested by the bulk-organic radiocarbon dates (Sect. 2.2.2), then the probability distributions…"
RC 84	Line 734 – missing word "Accepting the bulk organic radiocarbon dates as accurate **provides** minimum limits…" or something like this.
AR 84	Thank you for catching this typo. We will correct the passage.

RC 85	Lines 741-749 – suggest adding the ages of the outliers into the paragraph in the appropriate places to aid comparison with the mean.
AR 85	We will incorporate this change.
RC 86	Line 759 – “probably” should be “probable”
AR 86	Thank you for catching this typo. We will make this correction.
RC 87	Lines 783-788 – add the numbers into this narrative so that it is easier to follow and see what your interpreted chronology is.
AR 87	We will make this correction.
RC 88	Section 5.4 – it is not clear where the constraints on temperature and precipitation changes given in this section are coming from. It does not appear that independent glacier mass-balance modeling was undertaken for this study. Where do the estimates of 2 degrees and 35% precipitation change come from? The papers cited in this section are not unique to changes in the Sierra Nevada. The Wolfe 1992 paper is also not included in the reference list. The authors should carry out the glacier mass-balance work for their location specifically or at the very least be clearer about how they arrive at these numbers for temperature and precipitation change. They can also find independent estimates of temperature and precipitation changes during this interval from climate modeling work, such as the TRACE simulations mentioned later, to compare with their estimates.
AR 88	Thank you for catching the missing reference, we will include it in our revised manuscript. With regards to the modeling, the minimum amount of warming required to deglaciate the central Sierra Nevada (assuming no change in precipitation) comes from multiplying the ELA rise responsible for the deglaciation (at least 600 m; Phillips, 2016) by a lapse rate of 3 °C km^{-1}, which is the lower end of the values observed by Wolfe (1992) in the Sierra Nevada and Cascade ranges. The minimum reduction in precipitation to deglaciate the range (assuming no change in temperature) comes from the corollary of Oerlemans’s (2005) statement that a 25 % increase in precipitation is required to offset a 1 °C warming. To derive the estimated winter precipitation change, we simply convert the 1.8 °C warming from our ELA calculation above to a minimum-precipitation-reduction of ~35 % applying the estimation of Oerlemans (2005). We will add a section to the supplement describing this methodology.

RC 89	Lines 888-889 – The Great Basin lakes that are included in this comparison need to be listed by name and latitude as the timing of lake high stands varies with latitude during the deglaciation (see for example McGee et al., 2018).
AR 89	We will add a table to the supplement and refer to it as needed in the main text.
RC 90	Lines 892-893 – Again suggest that you expand to other North Atlantic records to document the timing of the Heinrich Event and not the regional Ostolo Cave record on its own.
AR 90	We will strengthen the manuscript here by (1) clearly differentiating Heinrich Event 1 from Heinrich Stadial 1 and (2) citing other records from the North Atlantic region and elsewhere that demonstrate that Heinrich Event 1 occurred between ca. 16.5 ka and ca. 15.9 ka (e.g., Ridge et al., 2012; Deplazes et al., 2013; Pérez-Mejías et al., 2021; Martin et al., 2023).
RC 91	Line 903 – change the partial derivative symbol being used in the carbon isotope notation to a lower-case delta symbol
AR 91	Thank you for catching the error.
RC 92	930-931 – As stated above, 16.2 is not a defined beginning of Heinrich Event 1, rather there are records from the North Atlantic indicating that IRD and AMOC slowdown began well before this
AR 92	As discussed above, we will emphasize the difference between Heinrich Events and Heinrich Stadials to limit any future confusion.
RC 93	Line 938 – It is not clear where the estimate of a 40% reduction in North Pacific moisture is coming from. Please provide citations or analysis to support this number.
AR 93	We will add the following passage to the supplement: "In making this calculation, of a 40 % reduction in North Pacific moisture, we assume the moisture arriving in the west-central Sierra Nevada from North Pacific and the tropics during the deglacial period had $\delta^{18}O$ values of -14.0 ± 0.4 ‰ and -4.9 ± 2.5 %, respectively. These distributions are the modern (2006–2011) distributions (Oster et al., 2012) uniformly adjusted +1.5 ‰, to reflect the deglacial ocean's higher $\delta^{18}O$ value (as a result of the preferential accumulation of ^{16}O in glaciers and ice sheets). Our adjustment of +1.5 ‰ offsets the observed decrease in benthic $\delta^{18}O$ from ca. 16 ka to the present (Lisiecki and Raymo, 2005). These adjusted $\delta^{18}O$ distributions suggest North Pacific moisture delivery to the west-central Sierra Nevada decreased

	from 54 % of all moisture to 31 %, a 43% relative reduction in North Pacific moisture, which we round to ~40 %.”
RC 94	Lines 941-942 – The increase in speleothem d^{13}C beginning at ~16.25 ka does imply that conditions may start to become drier, but overall the McLean’s Cave record indicates relatively wet conditions between ~16.4 and 16 ka.
AR 94	Thank for suggesting the needed clarification. We are suggesting drying relative to the conditions at that time – albeit not necessarily arid. The reduction in precipitation – if it occurred during winter – would reduce glacier mass balances, even if the conditions remained wet relative to later on / more modern conditions. As such we changed the sentence to now read as (new text in blue): *“Thus, the δ^{13}C record implies relative drying of the west-central Sierra Nevada starting at 16.20 ± 0.13 ka and the δ^{18}O record permits both it and warming.”*
RC 95	Lines 986-989 – Again, these dates are not an appropriate choice for the start of the Heinrich event based on evidence form the North Atlantic.
AR 95	As mentioned above, we will alter our manuscript to clearly define the terms and differences between Heinrich Stadials and Heinrich Events. We will adopt the more conservative start date of ca. 16.5 ka for HE1.
RC 96	Lines 1018-1028: This section linking the WAIS divide record to the tropical Pacific is missing the subsequent link to western North American climate. If this is important, include a description of this mechanism with citations. The last two lines “implying a substantial thinning of the LIS” require citations.
AR 96	Thank you for prompting us to think more about this issue. The discussion about WAIS divide is not essential to the manuscript and we will remove it.
RC 97	Figure 9 – illustrates the proposed ice sheet thinning –While the majority of the text suggests a big change at 16.2 – this figure shows the resultant ice sheet much later at 15.6 ka. This is also based on another reconstruction than ICE-5G, while ICE-5G is emphasized earlier. I’m not sure how much this figure adds given the inconsistencies with the timing and other aspects of the hypothesis.
AR 97	This figure is based on Art Dyke’s reconstructions of LIS *extent* over time (Dyke et al., 2003). Dyke et al. (2003) provides ice-extent reconstructions for 16.3 ka and 15.6 ka. These are the LIS ice-extent reconstructions that most closely bracket HE1. We will clarify that there isn’t a Dyke et al. (2003) (or other) ice-extent reconstruction for ca. 16.2 ka, the most likely time for when HE1 began (e.g., Bernal-Wormull et al., 2021; Pérez-Mejías et al., 2021; Martin et al., 2023).

	ICE-5G is the ice-*thickness* reconstruction used by the TrACE-21k model. Thus, when discussing ice thicknesses within the context of the TrACE experiment we must discuss the ICE-5G model. That said, Dyke et al. (2003) is a more granular and precise reconstruction of LIS ice extent over time. We will clarify why we are discussing both in the text.

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