I recently reviewed a similar manuscript by the same authors for another journal. I had raised several critical comments, and based on my review, the editor decided to reject the ms. I was happy to see that the authors decided to submit the ms again to GChron, but my main criticism remains after reading through the text. I summarise my main critical comments here. See also graphs in the supplement.

We kindly thank Referee 1 for their time considering and reviewing our submitted manuscript. We would like to highlight that yes, while the manuscript was previously submitted to another journal and rejected, the two reviews received reached opposing conclusions and recommendations. The other reviewer found our manuscript to provide a rigorous reassessment of previously published datasets and recommended only small clarifications, and the editor provided no independent assessment. Ultimately, it was unclear why one review was weighed on for the final decision.

Below we provide a response to each individual comment from Referee 1.

1) It is feasible that several eruptions of Katla occurred in the early Holocene, producing Vedde-type tephras. The results presented here, however, do not support this conclusion. The age-depth model is based on radiocarbon dates performed more than 30 years ago with standard deviations of several hundred years. Age-depth modelling of such old dates cannot give reliable ages and new samples for radiocarbon dating should have been submitted. The second lowest date (Ua-1888) is potentially an outlier, and by removing it from the model, all depths would get older ages in line with the interpretation of Björck et al. (1992)

We thank the reviewer for raising this point and allowing us to clarify our rationale. The dates that we calibrated were indeed generated several decades ago, but there is no reason to believe that they are any less reliable than ones generated today. The Björck et al. (1992) samples were dated with high-precision AMS techniques that remain the primary method used. While the uncertainty can be larger in samples dated several decades ago, the median ages of the original dates and those today remain similar. As an example, we compare 14C ages from the original Torfdalsvatn study (Björck et al., 1992) and another from the mid-late 2000s (Axford et al., 2007) – see Table below. Both samples were taken near the base of the G10ka Series tephra, and we recalibrated them using IntCal20 to make them directly comparable (Reimer et al., 2020). Given their similar stratigraphic location with respect the overlying tephra layers, the similar median age of the two samples is expected. The older sample from Björck et al. (1992) simply has a larger range of uncertainty, but the median age itself is not substantially different from a more recently dated sample (Axford et al., 2007).

Regarding the second lowest date (Ua-1888), there is no reason to believe that this sample is an outlier. First, evidence from Iceland shows that bulk sediment 14C outliers are typically stratigraphically too old due to the transport of terrestrial-derived carbon to the lake sediment (e.g., Geirsdóttir et al., 2009). However, the strong agreement between bulk sediment and macrofossil 14C ages from Axford et al. (2007) and Björck et al. (1992) (see Table below), respectively, suggest that these Early Holocene bulk sediment 14C ages are relatively accurate.

Moreover, given that this is a recently deglaciated landscape that likely has minimal terrestrial contributions due to a lack of soil formation, stratigraphically older ages from bulk sediment are unlikely. Second, in terms of a bulk sediment 14C age being stratigraphically too young, to the best of our knowledge, there is no viable mechanism that can naturally produce this scenario. In this sense, the age model should be weighted more towards younger ages as reflected in Fig. 3 for these records. Therefore, we believe that our interpretation of the data and derived age model remains valid.

We will expand our discussion in the manuscript (L79-86) to include the comparison of 14C ages published in 1992 and 2007 (Björck et al., 1992; Axford et al., 2007). Moreover, we will emphasize the higher uncertainty of our age estimates due to the old 14C dates, but that the median ages should be reliable. Ultimately, our results provide a baseline for future studies to improve age estimates and correlations to other localities. We hope this allays any concerns of unreliable ages.

Lab ID	Depth below G10ka Series	Material	Conventional ^{14}C age ± σ	Calibrated age BP ± σ	Reference
Ua-1890	8 cm	Moss macrofossil	9180 ± 210	10330 ± 370	Björck et al. (1992)
NSRL-14518	1.4 cm	Bulk sediment	9100 ± 25	10240 ± 10	Axford et al. (2007)

2) There is no discussion about the possibility of reworking of Vedde shards in the early Holocene. There are many examples from western Norway of reworking many thousand years into the Holocene. I agree with Mangerud's comments that a tephra count graph would be helpful here.

We thank the reviewer for raising this point and allowing us to clarify our rationale. We respectfully refer the reviewer to both the Section 3.2 (Tephra layer descriptions, L98-120) and Discussion (L122-130) sections where we discuss why reworking of these tephra layers is improbable. Our primary argument relies on the fact that all the layers contain pristine and non-abraded glass shards with the inclusion of minimal lithics and that each tephra layer features sharp upper and lower contacts with the interstitial organic sediment.

In addition to the above, it is also near impossible to get bulk reworking of a tephra layer as required by the presence of three discrete layers, taking place decades to centuries after its deposition, let alone two times in a row. Moreover, given that Torfdalsvatn's catchment is low relief with minimal topography, the bulk of the reworking would be wind derived, which primarily mobilizes sub-50-micron grains and hence the reworked tephra would be very fine ash and each storm input is expected to be normally size graded due to settling through the water column (i.e., Stoke's Law).

Finally, tephra grain counting between layers would not be useful to discern tephra redeposition in Iceland where tephra comprises the background. Given that it is a volcanic island, the parent material of all soils is volcanic (Arnalds, 2004). While glass shard counting can be useful for distal locations like Norway, where Icelandic tephra shards are either primary or secondary deposits, we always find various glass grains present in Icelandic lake sediment due to the constant mobilization of the soil into lakes from the surrounding catchment.

3) The lowest tephra layer, Tv-1 has a Hekla-like geochemistry and is believed to be one of the oldest basaltic layers from Hekla. The authors, however, do not mention that Tv-1 has been correlated with a tephra found in the NGRIP ice-core, NGRIP1519-1, dated to c 12,646 b2k (Mortensen et al., 2005; JQS). New data from NGRIP and GRIP confirms this correlation and firmly places the Tv-1/NGRIP1519-1 in the early part of Younger Dryas/GS-1 (Cook et al., 2022; QSR). The attached graph (Fig. 1 in the supplement) shows an alternative age model for the Torfdalsvatn core based on ice-core ages of the Tv-1 and Tv-4 tephras (Mortensen et al. 2005; Cook et al. 2022). It suggests that the Tv-2 layer (Vedde in Björck et al's paper) is firmly placed in the YD and not in early Preboreal. Biplots (Fig. 2 in the supplement) show that there is a generally good agreement between the Tv-1 tephra from Torfdalsvatn and the NGRIP1519.1/GRIP1654.05 layer from Greenland ice. There is more spread in the ice core samples which might be due to smaller shard that were analysed, but all major elements overlap.

We thank the reviewer for raising this point and allowing us to clarify our rationale. As previously outlined by studies, some which use the Vedde Ash as an example (e.g., Lane et al., 2012), chemistry alone is not sufficient to draw a tephra layer correlation. Basaltic and rhyolitic tephra layers from Iceland are known to share indistinguishable chemistries during the Holocene (e.g., Larsen et al., 2001; Óladóttir et al., 2020), meaning that independent age control is imperative to identify the tephra layer and derive secure correlations between geographic regions. While the reviewer is correct that the tephra in the Greenland ice cores shares similar major oxide geochemistry with the Tv-1 tephra layer in Torfdalsvatn, the objective age constraint we provide by calibrating the Björck et al. (1992) age model indicates that the Tv-1 tephra layer in Torfdalsvatn is younger and therefore must be from different volcanic eruption.

As noted by the reviewer, prior studies have suggested correlations between additional Torfdalsvatn and Greenland tephra layers. However, these correlations relied on the old ages estimates from Björck et al. (1992), which itself partially relied on the assumption that Tv-2 was the Vedde Ash (~12100 cal BP). The alternative age model that the reviewer provides uses the ages from the Greenland ice core for Torfdalsvatn's tephra layers based on the assumption that the original Torfdalsvatn age model was correct (e.g., Björck et al., 1992). We respectfully argue that using this alternative age model to justify older ages of Torfdalsvatn's Tv-1 tephra layer is circular reasoning. In this regard, we favor our objective approach that independently dates Torfdalsvatn's tephra layers with quantitative dating techniques from the same record. While certainly not confirming the age of Torfdalsvatn's tephra layers, there are contemporaneous tephra layers of similar composition on the North Iceland Shelf (Kristjánsdóttir et al., 2007) and in Hestvatn (Geirsdóttir et al., 2021), which provide possible correlations, as discussed in the manuscript (L131-147).

Given our above points, and our response to issue 1 regarding the reliability of our revised age model, we believe that our interpretation of tephra layer ages in Torfdalsvatn remains valid. We will expand upon our description of the Tv-1 tephra and its possible correlations to other North Atlantic sites in the manuscript.

4) Previous investigations at Torfdalsvatn by Rundgren (1995, QR) are mentioned in the text, but rather surprisingly, are not discussed in any detail. The pollen- and lithostratigraphy presented by Rundgren suggest an YD age for Tv-2 and an early YD age for Tv-1, in agreement Björck's paper, see also Fig. 3 in the supplement file.

We thank the reviewer for raising this point and allowing us to clarify our rationale for not discussing the published pollen datasets (Björck et al., 1992; Rundgren, 1995). First, we want to emphasize that the Younger Dryas Stadial (12900 to 11700 cal a BP) cannot be identified based on pollen and lithostratigraphy without independent chronological control. A relatively recent study emphasized this issue well through high-resolution dating of two terrestrial archives to demonstrate the times-trangressive nature of Younger Dryas-associated cooling in Europe (Lane et al., 2013). If these two records were dated according to their proxy records, such as biostratigraphy, they would both cover the same age range, which we now believe is not the case. Given that we calibrate the original Björck et al. (1992) age model, which was then adopted by Rundgren (1995) as highighted by the reviewer, the revised age constraint suggests that the pollen-inferred cooling post-dates the Younger Dryas. We prefer to not include further discussion of the pollen spectra in this paper as the vegetation history does not relate to our study's focus, which is the evaluation of the Torfdalsvatn's Early Holocene 14C ages and tephra layers.

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