

REVIEWER 3

First I would like to congratulate the authors on taking on this task of compiling chronological data for Iceland, and that I expect to be a useful resource for those interested in the palaeoclimate and glacial history of Iceland and wider North Atlantic. This is a timely and welcome synthesis. I also recognise the time and attention to detail required to achieve this type of work and in general the data is well presented and documented. I focus my comments on where the manuscript and dataset can be further refined such that it has the stated potential and is made most useful and accessible to potential users.

We greatly appreciate Anna Hughes's time and consideration reviewing our curated database manuscript and their general support of our efforts. Their thoughtful comments have helped improve the clarity of our database, which directly benefits its future users. Below we respond to each comment individually in blue.

After completing my review I read and noted the comments of the other two reviewers. I am less concerned about the nomenclature of database/dataset as (philosophically) I am not sure that we can completely take out interpretation from palaeo-data; and that the usefulness of the ages themselves come about only when taken together with geological information on their context. This is often based on qualitative information, such as field descriptions and interpretations of the sediment units or landforms the samples are taken from. There is a notion that quantitative age estimates are objective truth, whereas in actuality these are also generated by way of theoretical assumptions and interpretations we hold about each dating method. While we can measure the amount of Be-10 or C-14 in a rock or organic sample, to turn this value into an age we rely on assumptions that we hold about the application of the particular dating method (a point not unappreciated here by the authors). We also have to estimate the processes that each sample may have undergone based on the geological setting of samples and understand how these may have influenced the age estimates. So these two aspects cannot be fully separated in order to interpret the palaeoclimate or glaciological implications of a particular age or set of ages; and I think it can cause problems for further use and application of the dataset to have it presented without these additional pieces of insight explicitly stated and included.

This relates somewhat to point I want to raise on the use of terms such as subjective/objective in the manuscript. Subjectivity/objectivity is referred to at several points in the manuscript (e.g. line 87) and used to justify exclusion of some tephra layers and focus on radiometric methods. Accepting that an Icelandic wide tephrochronology is a substantive undertaking in itself I understand and agree with the exclusion of most of this information here and also consider that a compilation of radiometric data alone is a worthwhile and important endeavour. However, the inclusion of some chronological marker horizon information from tephra immediately raises the question of how this interpretation of the degree of subjectivity is applied/undertaken, as well as how these are presented, as the stratigraphical information becomes even more significant in order to use the ages to understand glacier and palaeoclimate changes. Being extremely pedantic but are radiometric dating methods completely objective? I am not sure we can imply this when there are multiple caveats and ways to generate ages (e.g. issues with marine radiocarbon calibration and/or different means to calculate TCN ages). It gives a false sense of significance to some information over others. The authors do also seem to recognise this issue in section 3.5 which presents a useful explanation of some of the remaining issues for geochronological method development.

We greatly appreciate these thoughtful and insightful comments on objectivity vs subjectivity, and the opportunity to improve our clarity. Of course, we do not take radiometric dates as the simple truth, which we hope is highlighted in Section 3.5 and our use of the term "most

objective” rather than simply “objective”. This is particularly evident given the range of ages that can be obtained from, for example, different TCN exposure age calculators and whether or not aged carbon is considered for glacial age marine sediment ^{14}C dates. However, the geochemistry analyzed from the sample (e.g., fraction modern ^{14}C), which is then in turn used to calculate an age estimate, results in a fairly limited range (on Quaternary time scales) of possible ages on the order of 1000s of years. In contrast, while it is important to take stratigraphical context into account for tephra layers, there is nothing that geochemically limits a tephra layer age in the same way. Hence, we argue that these radiometric ages, compared to tephra layers, are the “most” objective. Understanding, these limitations of tephra layers, as stated in the text, we did believe it was useful to include 2 layers that had generally distinct stratigraphic or geochemical attributes, as well as relatively robust age constraint. The inclusion of a more comprehensive tephrochronological component to future ICEland database iterations is of high priority. For QC ratings, we have removed the term objective in the text as we agree there is an inherent degree of subjectivity imparted by us on those ratings.

The title could be more precise; it explains what the dataset could be useful for, but this is not actually achieved in this output. Arguably we also use the geochronology to reconstruct the timing and rate of palaeoclimate patterns and glacier changes rather than reconstruct those events. These issues are also brought forward in the text and I also found that there were good and thorough explanations of the nuance that exists here (also relating to my point above) but this did not come across clearly in the start of the manuscript. For example in the discussion of the QC ratings, (lines 239-241) you make the important statement that QC = 3 ages can still provide useful and relevant constraints. But this then becomes a bit unclear for the reader if all QC = 3 ages are reported as 'unreliable'. They fail many criteria that we can use to attempt to secure robust ages and check we have applied our dating methods correctly; there is scope they may still provide accurate ages however.

We appreciate this comment and opportunity to expand our reasoning on the title. In our view, the current title is appropriate as the inclusion of less specific word “patterns” encompasses not only the timing and rate of changes, but also the events themselves. We were very deliberate in this word choice to ensure that the title adequately reflected the intended purposes of the database. While we do not use the database to reconstruct these patterns, simply leaving the title at Late Quaternary geochronology would leave the title far too vague for the reader to understand it's purpose. The dates included in the database were taken with glacial history, relative sea level, and paleoclimate in mind and thus we respectfully believe it is important to keep the title as is (except replacing Late Quaternary as described in the next comment's response).

We will clarify in the text the Q3 dates are unreliable as “dates”, specifically, to avoid any confusion for the user. We apologize if this was unclear but do agree that it is critical for readers.

The abstract and at several points in the text the dataset is summarised as being 'comprehensive'. Is this possible? Later the it is stated that some tephra records are intentionally excluded. It would also be useful to see a more explicit statement of the time period included: 'Late Quaternary' is a little vague. The data appears to include ages back to ~60 ka. Is this an outcome of the compilation, did authors select to compile ages only relating to build up and retreat of the last glacial (MIS2) ice sheet and evolution of ice caps and glaciers through the Holocene? Or was ~60 ka used as a cut off and older ages also exist? Similarly it was a little unclear (especially early on in the manuscript) what data was included and what choices have been made. At line 63 it is described as 'glacier geochronological database' but then later (lines

65-66) it appears that RSL, tephra and palaeoclimate relevant ages are also included. While there is a rationale to include these it would serve to be more precise in the text upfront. This is not clear what is included and what is not until lines 84-85 in the methods section. At some point in the text or a figure it would be insightful to have a sense of the distribution of the palaeoclimate vs RSL vs glacial ages within the dataset.

We appreciate this comment and opportunity to clarify our thoughts. While we have considered nearly all dates, including tephra, during the construction of this database, the reviewer is correct that perhaps by excluding most tephra ages, then this is not comprehensive in that sense. To avoid confusion, we will remove the term comprehensive and revise to text to state detailed instead.

Second, we agree that Late Quaternary term is vague as it does not have a formal definition. We will change the title and manuscript to state 60 ka. There was no temporal cut-off for this that we implemented, it was simply the nature of the existing data. We compiled all “quasi-objective” Quaternary dates related to ice sheets, relative sea level, and paleoclimate that can serve data-model comparisons. This included 14C that has a range of ~50 ka and TCN which is largely limited to the last glacial period due to prior ice sheet erosion of nuclides. We have revised the final paragraph of the introduction to more accurately reflect the broader rationale behind our data compilation.

Finally, we have added a brief discussion of the distribution of Glacier, Relative sea level, and Paleoclimate dates in the database in Section 3.1 (Dataset inventory)

I appreciate the thorough explanation of the interpretative classes and discussion of maximum/minimum ages. Though there is still some room for greater precision in these summaries (see specific comments below). The ‘glacial’ category is a useful addition for He-3 derived ages but the name chosen is a little confusing since you already have the upper category of ‘glacier’. There are also some categories that are included in the database itself that are not mentioned here; ‘Jökulhlaup drainage’, ‘Debris-cover glacier collapse’, ‘Debris-covered glacier stabilization’, ‘Rock glacier stabilization’. While these appear self explanatory nonetheless they should be explained here to fully document the database for users.

We appreciate this comment to improve clarity and have now included Jökulhlaup drainage, Debris-cover glacier collapse, Debris-covered glacier stabilization, and Rock glacier stabilization classifications in Section 2.4.

I was interested to see how the issue with calibration of marine radiocarbon ages had been handled here. There is a very clear and thorough explanation of the caveats and known issues of applying Marine20 in higher latitudes though it would be useful to add some sense of the absolute offset of ages and when this might be most extreme (Line 135).

We appreciate this comment but prefer to not add additional subjectiveness to the database. As these potential age offsets are only “possible” and unconstrained, we believe that simply highlighting potential shortcomings serves the reader just the same and prevents any incorrect assumptions if future data suggest minimal age offsets, for instance.

I agree with other reviewers that the reporting of ‘ka BP’ should only be used for radiocarbon derived ages. It is useful if reporting calendar ages to make this explicit by using cal. ka BP also. All other ages should be reported as ka.

Edited.

Table 1: It could be highlighted more clearly which metadata are taken from source publications and which are introduced by the authors [see database comments below].

We have added some clarification to Table 1 to help improve where metadata is taken from the source publications vs introduced by us. As this is a curated database, the only metadata introduced by us is the QC rating.

Figure 1: This figure is great and shows the distribution of different data types well. But lacks any geographic labels either on land and in the ocean. If the scope of the dataset is the last glacial cycle then it would be informative to add the (asynchronous) maximum extent of the ice sheet to see how the coverage compares to the known understanding of the LGM. The background data also needs proper citation. At the moment the sources are not sufficiently detailed to know where this is coming from, and I think not included in the reference list? GEBCO for example should be cited with the version: e.g. GEBCO Compilation Group (2025) GEBCO 2025 Grid (doi:10.5285/37c52e96-24ea-67ce-e063-7086abc05f29). ESRI and Garmin are insufficient citations as do not specify what product is being used.

Thank you for bringing these points to our attention. We deliberately chose not to include any LGM ice extent limits as those are currently poorly constrained empirically, and mostly derived from ice sheet models. The background data citations have now been included.

Figures 2-4: For ease of comparison these histograms should have same maximum on the x axis.

While we acknowledge that is helpful in many cases, the x-axis range for marine 14C (Fig. 2) is nearly twice that of terrestrial 14C (Fig. 3) and over 6x that of TCN (Fig. 4). Hence, to best visualize the trends for each grouping, we respectively argue to keep the x-axes as is. However, we will edit the figure captions to note the different range of x-axes for each.

Figure 5: Has same background data source issue as Figure 1. Here it would be useful to use graduated circles as in Figure 1 to give sense of density of ages as well as their location. Or, if each dot relates to a single age, this should be stated. What makes an age 'relevant for reconstructing IIS deglaciation'; is this only 'deglacial' class, or some other combination? Nearly all the dots reflect one date, so for simplicity's sake and for easier visualization, we prefer to keep the data points as is. We have clarified this in the figure caption, as well as the classifications included that are relevant for IIS deglaciation (i.e., Glacier and Relative sea level contexts, see Section 2.4). Background source data have also now been properly cited.

Figure 6: It would be useful if this showed the data as graduated circles as I assume that there are multiple ages at at least some of these sites? Same background source data issues as other map figures. The 'powered by ESRI' is insufficient citation as the topographic and bathymetric datasets are not fully reported.

These data points each represent one age, which has now been clarified in the figure caption. Background source data have also now been properly cited.

Dataset comments:

The data is presented as a .xlsx spreadsheet comprised on 4 tabs. This makes sense to make the number of columns manageable, though it would also be convenient to have all the data combined. It would be useful for a subset of users to have the data presented as GIS shapefile or .shp in addition. The data is generally clearly presented. I went through the dataset closely and raise the following points for consideration:

We appreciate this point, however, given the different metadata and columns required for different dating techniques, we felt that it was best left as 4 different tabs, rather than combining. If we combined the number of columns would be cumbersome and perhaps more challenging to find relevant metadata for entries.

'Stratigraphy' field is only giving the nature of the unit the samples are from, rather than summarising the position of the unit relative to units above and below. This limits the information that is available to users on the context of the sample and its interpretation. The stratigraphic position of appears to be presented in the 'Comment' field instead. Suggest re-titling the 'stratigraphy' column to 'Unit' or something similar. The comment field could be more clearly labelled as 'stratigraphic interpretation' or similar; I did not see many other types of comments here, though summarised in the text as potentially holding such information. It would also be useful to know if the interpretations are taken from the source references, or are new or re-interpretations by the authors here (e.g. in Table 1).

Thank you for catching this opportunity to improve clarity of the database. We will adjust the column names accordingly.

Some samples have a 'Comp_Ref' but no 'Source_Ref'. For the data that are published for the first time here in this dataset this is fine, though arguably the source ref should then be 'This Study' as well. For others presented in previous compilations it is not clear why there is no source reference given. Of the data are presented for the first time in a compilation then that becomes the 'source reference'; i.e. I read that these fields should be such that all items should have a 'Source Ref' and only some should have a 'Comp_ref' in addition. If there is some other reasoning then this needs to be stated in the manuscript.

Thank you for highlighting this error. Yes, we agree the "this study" reference should have always been in the Source_Ref, but was mistakenly in the Comp_Ref for some. This has now been edited. We also edited the data entries that did not have a Source_Ref, which were all from INSTAAR Date Lists. The Comp_Ref for each of these has now also been copied into the Source_Ref, as this is where the dates were first published.

M627 has no reference information provided. Where is this sample taken from?

Thank you for catching this, it should have been "this study". This has now been corrected.

Several samples have no C-14 error provided, yet they have been calibrated. How has this been achieved? If you used a nominal error then this should be stated in the manuscript which is essentially operating as a 'ReadMe' for users of the data [I did not find any such additional file via Github]. As the dataset can be accessed with the potential for users to not carefully read the manuscript [not ideal but realistic] then I suggest that the nominal error is listed here and/or a comment added to explain it in the 'Comment' field [though this may require an additional comment field to be added if you take my suggestion above].

Thank you for catching this and we apologize for the oversight. We had imported all the days into OxCal in bulk and these were the calibrated ages that were returned. It is unclear how OxCal handled this as, at least for dates input individually, the uncertainty is required to calibrate the date. In any case, we have now removed the calibrated uncertainty from the database for dates that do not have 14C uncertainty. We feel this is the best approach rather than to use a nominal error.

There appears to be an extra row at in the 'Terrestrial – 3He' tab that needs to be combined into the header field. This will be lost if users attempt to do any sorting of the table. Similarly the information at row 67 column O – this should be reported above the header row for ease of reuse of the data.

Thanks for catching this, it's been removed.

Specific comments:

Line 15: ...understanding of the chronology of Late Quaternary... [suggest adding 'chronology']
Edited.

Line 30: Northeast Greenland Ice Stream [capitalisation]
Edited.

Line 50-51: Here the role of landforms such as moraines etc is omitted. The examples seem to focus on how the Holocene chronology has been developed, not the overall history of changes in glaciers and ice caps.

We revised the text from "Holocene history" to "Holocene chronology" to more accurately reflect our intended meaning.

Line 58-59: Presumably you are talking about the Iceland context here, as globally this statement is not true; you state some examples in the next sentence. Sentence needs to be clearer.

Yes, we are referring to Iceland and have clarified the sentence accordingly.

Line 67: 'quality of empirical data' non-quantitative information such as the position of moraines is also empirical information which you explicitly have not included here, suggest change to 'geochronological information'.

Edited.

Line 77: Are you referring to unpublished data of the authors, or of others that has been provided to you?

This is our unpublished data, and the sentence edited.

Line 80: 100% support reporting of decimal degrees for all dates, but the statement 'machine readable' is not necessary; we are at a point where other formats are also machine readable, decimal degrees is just a little easier/convenient.

Edited.

Line 81-82: This is a frustrating state of affairs I have also experienced in compiling especially older literature. Sometimes a site name can be used to identify an approximate location. It would be useful to indicate an estimate of the number of ages excluded due to this constraint.

Edited.

Line 82: it would be useful for those not familiar with Iceland geography to have these regions marked one of the map figures.

Edited.

Line 95: recent evidence needs citation (this comes at the end of the sentence, but since a long sentence should be placed here next to this statement).

Edited.

Line 108: Do you have plans in place to update the dataset? How frequent might this be? Will updates be associated with a new publication or only occur on Ghub?

This is a good point and appreciate the comment. We do have plans to maintain this and publish updates, but the exact timeline is not set (e.g., every 2, 5, 10 years, etc), which is

partially due to the uncertainty of how much new data will be published and when. Updates will be associated with a new ESSD publication.

Lines 137-140: This is important but is this not also handled as part of your quality control assessment of these ages?

Yes, but we believe including it in the dating tools section is important for clarity.

Lines 145: I have also experienced this issue in compiling data. Did you attempt to obtain this data from the original authors of these publications? As this dataset now becomes the version of record that most will use it would be useful to have made clear which samples where assumptions have been applied and what used as assumptions. I see in the dataset that you have marked for example where density is assumed. State here in the text how you have highlighted these assumptions for users of the dataset.

Thanks for raising this point and opportunity to clarify. We did not attempt to reach out to authors as we have found that to be largely futile in past experience. Our aim for this database was to collate all AVAILABLE data from the literature. The densities marked as assumed in the original data file were actually assumed by the original publication. Following those, we assumed 2.7 g/cm^3 for all samples where density was not measured, which has now been corrected in a revised data file to be uploaded to Zenodo and Ghub upon publication of the manuscript. We have also added some text in the manuscript to clarify these assumptions (density and others).

Lines 151: Could you give a % assessment of the degree of change that corrections for snow cover, erosion and/or uplift might make to individual ages?

This is a good and important question to clarify. Unfortunately, giving a % assessment of change is challenging as ^{36}Cl does not respond uniformly to erosion. Samples with high concentrations of native Cl tend to get older with some erosion, while samples with low native Cl tend to get younger. Snow cover and uplift are also tricky as there are various ways to attempt addressing them with none of them widely accepted as accurate. Estimating snow cover from thousands of years ago is particularly uncertain. Therefore we prefer not to provide an estimate as it may deviate from reality.

Lines 162: how are these categories defined? Looking at the dataset I see that some fit in multiple categories. Which was my expectation when reading the manuscript though it is not stated. Useful to confirm that here in the text as well.

We have revised this section to help clarify how these categories were defined. This is largely based on original interpretations provided in the source publication. However, a small number of paleoclimate sites that contain complete postglacial stratigraphies with dates near the glaciomarine-glaciolacustrine-postglacial sediment boundary have also been included as Glacier sites and may not have been originally interpreted in the publication as such.

Line 165: See my comment on the 'stratigraphy' column in the dataset – there is not always sufficient information here to track your advance/margin/deglacial/etc classification as it only appears to summarise the host unit rather than the overall position of the sample.

See reply to prior comment. We have now revised the database naming scheme to help clarify this.

Line 181-2: This is an important statement for use of the dataset and needs further explanation as to why this should be the case. Why does the age of the sample override the QC rating in terms of reliability?

We have edited to the youngest "Q1" date should be taken as most reliable.

Line 185: Unclear phrasing – do you mean a glacial till when referring to glacial diamicton? As if so then this does represent glacier overriding. Or are you referring to glacial marine diamictons as proglacial sediments? 'glacial diamicton' is ambiguous.

Since these sediments were not overridden by ice, as we note in the sentence, this is not glacial till. Hence, we use the term uncompactd glacier diamicton, which based on our understanding of the definition, refers to poorly sorted and non-stratified sediments deposited proximal to the glacier.

Line 200-201: 'may reflect'
Edited.

Line 208: As above, explain more fully why you interpret the oldest age as most reliable. Similarly, we have edited to the oldest "Q1" date should be taken as most reliable.

Line 258: cut 'objectively' (see above points)
Edited.

Line 270: the age and the stratigraphic sequence / setting indicates this
Edited.

Line 270: LGM is undefined; can be taken to mean a range of different time periods.
We refer the reviewer to manuscript's second paragraph where the LGM's time period is defined (~28 to 22 ka BP).

Line 316-318: This sentence appears to be contradictory suggest rephrasing.
We have revised to improve clarity.

Line 330: suggest removing 'and bias' this is covered by 'unquantified uncertainty'. The fact that here the authors recognise this unknown uncertainty or bias also supports my point above that we cannot claim age estimates to be 'objective'.
Edited.

Line 368: may help to delineate
Edited.

Line 369: Fig. 6 does not show timeslices though?
Removed reference to Fig. 6.

Line 381: two
Edited.

Line 384: to reconstruct OR to reconstruct and map glacier margins
Edited.

Line 386: Elaborate to explain for the unfamiliar how this dataset is more complete synthesis than these previous reviews.
Revised and elaborated.