REVIEWER 1

The authors present a curated compilation of geochronologic data that help constrain the late Quaternary history of the Iceland Ice Sheet. The data is exclusively geochronologic constraints; i.e., there are no interpretations of landforms or mapping of ice margin retreat through time, but efforts such as these are critical initial steps to creating accurate reconstructions of ice retreat through time (as the authors point out in the "Vision and future research" section), among other uses. Compilation efforts such as these are invaluable for providing robust and reliably filtered constraints for ice sheet, climate, and Earth System models that can be used to assess model performance and accuracy. It appears that while the authors highlight several geographic regions around Iceland where additional constraints are needed, there is a critical mass of currently available observations around Iceland that make these efforts to curate the data timely. I have a minor philosophical disagreement around the use of the term "database" when presenting a curated compilation of data with interpretations about data quality that the authors may wish to address. The manuscript is well-written and presented, figures are of general high quality, and the compiled data are available in a straight-forward excel spreadsheet with sufficient information for reproducibility. I support publication of this manuscript and compiled data after minor revisions.

- Joe Tulenko

We greatly appreciate Joe Tulenko's time and consideration reviewing our curated database manuscript and his general support of our efforts. His comments have helped improve the clarity of the description, which is important for users, and provided valuable suggestions for future development. Below, we respond to each comment individually in italics.

General comments:

My only overall general comment on this manuscript is the use of the term database; in my view, a database cannot contain any interpretations about data quality. The authors present a rigorous ranking and quality assurance process that I agree is necessary and valuable, but because of this, I see their product as a curated dataset as opposed to a database. I would encourage the authors to consider referring to their product as a curated dataset, but this is a minor consideration.

We appreciate this suggestion and highlighting this important distinction. We have edited the term to "curated database" accordingly.

Line-by-line comments:

line 54: revise "To date, a range of models of been developed…"
Edited.

Line 143: here and elsewhere when describing the CREp online calculator, it is important to note that the authors are using the development version (https://crep-dev.otelo.univ-lorraine.fr/#/init) that includes Cl-36, not the stable version of CREp (https://crep.otelo.univ-lorraine.fr/#/). Users may be confused when going to the main CREp landing page only to find no option for calculating Cl-36 exposure ages. I recommend adding a link and/or a brief statement in this section.
This is a good point to clarify and appreciate the suggestion. We have clarified this in Section 2.3, accordingly.

Table 2/Section 2.3: It takes important and careful interpretation based on experience to generate these criteria for data quality that I would encourage the authors to expand on for the benefit of readers. For example, why specifically are mollusc 14C ages given less weight compared to 14C ages from other sources? Perhaps the authors could briefly expand on and explain the relevance of some/all their criteria as a guide for potential data users in this section.
We appreciate this comment and have now expanded on some of the criteria from Table 2 in Section 2.

Line 273 (and as demonstrated in Figure 2 & Figure 3): it is indeed interesting that there are more, slightly older/less reliable terrestrial radiocarbon ages compared to marine radiocarbon ages, which the authors attribute to contamination from older carbon and then cite Brader et al., 2015. The citation is useful, but I might encourage the authors to briefly expand on where the old, contaminate carbon on land is coming from and why that appears to be less of an issue for the marine realm.
We appreciate this point and opportunity to clarify. The stratigraphically old dates from terrestrial samples are from bulk or humic acid fraction 14C dates, which incorporate carbon from multiple sources (e.g., lakes and soils). On the other hand, the marine dates are largely derived from macrofossils (e.g., mollusc shells). While the data is too limited to understand where the old carbon is from for terrestrial samples and not speculated on in the cited studies, it is possibly from small amounts of residual Last Interglacial carbon preserved beneath the Icelandic Ice Sheet and deposited into the lakes during the last deglaciation. These old carbon dates on land are also associated with very low carbon content (e.g., Brader et al., 2015), so it only takes a very small amount of old carbon to skew the age. The marine macrofossils that are reliable are from unbroken shells that were likely deposited in situ. The marine 14C ages that are stratigraphically too old are derived from broken shells within diamictons, that likely reflect reworking of the sediment during past ice sheet expansion during the last glacial period. These old marine C14 dates are flagged and commented on accordingly in the curated database. We will clarify this in the revised text with an slightly expanded discussion of terrestrial 14C dates in Section 2.1.

Line 276: minor detail, but technically exposure ages (at least exposure ages using the online exposure age calculator) are standardized to the date of collection or 2010 CE if date of collection is not specified. So the use of “ka BP” is slightly inaccurate since they are not standardized to 1950. I suggest simply using ka when reporting exposure ages.
We appreciate picking this up and have edited the text accordingly.

Line 292/Figure 4: same comment for line 276 for x-axis title.
Edited.

Line 300 (and paragraph starting at Line 413): more of a curiosity than actionable comment, but I am curious why the data coverage is relatively sparse in the SE portion of Iceland? Perhaps useful/cautionary information for future data collectors if there are specific reasons/challenges for collecting data in that region?
This is a good point. We are unaware of any specific reasons for data scarcity beyond lack of time and funding to complete that research. One possibility may be the occurrence of jökulhlaups (glacier outburst floods), although these events have recurring flood routes meaning that there are indeed areas not impacted by these sorts of geologic hazards. In any case, we are hoping to improve this through new data collection in the coming years and encourage other researchers so as well.

Line 305: revise “that constrain the spatial footprint off past ice limits” to “that constrain the spatial footprint of past ice limits”?
Revised.

Line 320/Figure 5: check grey color used to map troughs and the box in the legend, they don’t appear to be the same shade.
Double-checked and greys are the same. The darker blue ocean around the legend box and lighter blue/white around the troughs may be causing the odd visual. Thanks for pointing this out though.

Line 357-342: this is important context for readers that want to re-calculate Cl-36 exposure ages and is much appreciated. I agree that differences in calibration data used across the various calculators, and whether the calibration lithologies are geochemically representative of Iceland rocks likely explains the majority of the age discrepancies. Encouraging readers to utilize calibration datasets that are geochemically representative of their data and/or data curated here will likely produce more reliable Cl-36 exposure ages/interpretations in the long run.
This is a good point and we appreciate the opportunity to clarify. We are reluctant to explicitly state that a particular calculator is preferable over others, but agree that acknowledging the flexibility of CREp in this regard is worthy of pointing out for the reader. We have added a sentence in the text to clarify.

Section 4 Vision and Future Work: just a note that efforts such as the ICE-D project (ww.ice-d.org) are attempting to create more dynamic pipelines for serving geochronologic constraints useful in data compilations such as these, large data analyses, and data model comparison efforts. The authors may consider utilizing ICE-D resources for future versions of this product if useful.
This is an excellent point and would be excited to include the current TCN data in ICE-D. We’ll be in touch!

Line 436: small note on Ghub, it is not a certified data repository (e.g., AGU or other agency approved), and Ghub personnel encourage users to register Zenodo DOIs for their resources hosted on the platform. Zenodo will likely provide a more reliable, persistent DOI for archiving than Ghub, which is primarily useful for data discoverability.
We appreciate this comment! We have now registered ICEland-1 at Zenodo as well and have included the DOI in the Data Availability section (). We left the dataset at Ghub to assist in data discoverability.

REVIEWER 2
Summary

Harning et al present a new “quality-controlled” geochronological database for Iceland during the Late Quaternary, containing chronological data points constraining past glacier extent, relative sea level, and palaeoclimate records. The chronological data are from peer reviewed publications, PhD and MSc theses, previous compilations, and unpublished dates. The database covers both the marine and terrestrial realm, and include 14C ages, TCN exposure ages, and some tephra layers (Askja S, G10ka). They do not include any geomorphological data or interpretation of landforms. Data are handled to ensure FAIR principles are adopted and follows similar compilations (e.g. DATED). The downloadable data are well presented, and the added context markers are clear, allowing an understanding of the context for each age/cluster on ages.

There is some clear and intentional interpretation of data, and the quality control criteria are in places subjective. As such I am not sure if can be explicitly labelled as a database, which should be free of interpretation.

Beyond this I have a few general comments but otherwise consider the paper a valuable contribution.

We greatly appreciate Reviewer 2's time and consideration reviewing our "curated database" manuscript and the general support of our efforts. Below we respond to each comment individually in blue.

Comments

- I think it is useful and very transparent to have recalculated all the ages with all calculators, but it would maybe be useful to explain which you recommend using, or the key difference between them. For example, CREp allows the Ca production rate to be changed, using Licciardi's values which (I believe) is fairly standard.
  Similar to our reply to Reviewer 1, we are reluctant to state which calculator is preferable over the others as it opens us up to criticism outside of the review process (various researchers have strong feelings on this topic). However, as we responded to Reviewer 1, we have added a reminder in Section 3.5 that CREp allows the user to choose the Ca spallation production rate, such as those locally developed from Icelandic mafic rocks (Licciardi et al., 2008).

- TCN ages should be quoted as ka, not ka BP as they are not from "BP" (1950 CE).
  We appreciate this suggestion and apologize for the error. The presentation of TCN ages has now been corrected.

- It depends on editorial policy, but I would quote 14C ages as "cal. ka BP", but this is variable with journals.
  We appreciate this suggestion and will leave the decision up to the editorial staff pending acceptance of the manuscript.

- Figure 5 – which are QC 1 and which are QC2? Or are they not separated and just shown together?
  Correct, QC1 and 2 dates were combined as we suggest that both are suitable for reconstruction ice limits and it simplified the presentation of the data, which we have now clarified in the figure caption, as well as for Figure 6. The curated database includes all the relevant information for any user.

- The "comment" column in the database needs to be more clearly explained. Are these comments from the original sources, or are they comments from Harning et al, and therefore interpretations of the original data?
  Yes, the comment information is obtained from the original source and has been clarified in the text and Table 1.

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 understans 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 14C dates. However, the geochemistry analyzed from the sample (e.g., fraction modern 14C), 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 it's 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 fine any such additional file via Ghub]. 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 36Cl 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 glacimarine 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 uncompacted 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.