Reply to the comments of Anonymous Referee #1 to the manuscript entitled ‘Local Beryllium-10 production rate for the mid-elevation mountainous regions in Central Europe, deduced from a multi-method study of moraines and lake sediments in the Black Forest’

Dear reviewer,

We thank you for your thoughtful and critical comments that resulted in considerable improvements to the manuscript. We thoroughly considered all comments and revised the manuscript accordingly. For responses to the comments, see the table below. We hope that the manuscript will be accepted for publication in its revised form.

Thank you very much for your kind consideration.

On the behalf of all co-authors,

Felix Martin Hofmann

Line, Figure, or Table	Reviewer comment	Authors' reply
-	**Potential errors in data reporting:** After a thorough review of the data presented in this manuscript, I believe I might have found some mathematical errors that I strongly encourage the authors to double check, mainly in the calculation of cosmogenic ^{10}Be concentrations that I found to be 2-3% too low compared to my own calculations.See below for more information on that. Moreover, the authors remove from consideration three samples in the calibration dataset - one sample is an extreme value that could rightfully warrant removal in my opinion, one sample they argue had sample measurement issues and should be disregarded, but the last one (FS-01a) seemingly does not have any explanation from the authors. My only thought is that perhaps it was removed because the sampled boulder was situated on a moraine stratigraphically above the bog and the authors only wanted to consider the modeled radiocarbon date as a minimum age constraint. However, the normalized concentration that I calculated for this boulder is nearly identical to the other samples within the resolution of the dating method so I am not sure it should be removed. In fact, the radiocarbon constraint from the bog could conceivably be considered a maximum age constraint for the younger moraine. I would like to see either a much clearer explanation as to why they removed this sample from consideration, or I feel the authors should reconsider including it in the calibration dataset.	We carefully checked our calculations for potential errors. See the attached table for further details. We did not include the Beryllium-10 concentration in the sampling surface on the FS-01a boulder in the calibration dataset, as moraine formation at position FS-01 might have postdated the onset of the deposition of lake sediments at the Feldsee Bog. We added this information to the revised methods section: "We also sampled the surface of the FS-01a gneiss boulder on the moraine at position FS-01a for age calculations. However, we did not include the sample in the calibration dataset, as moraine formation might have post-dated the onset of deposition of lake sediments at the FSM coring site (Fig. 3b)." We agree that the basal age at the FSM coring site provides a maximum age for the moraine at position FS-01. Unfortunately, only one boulder was available for sampling on this landform.

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-	**Sediment coring approach:**: Although I am not requesting the authors specifically address this if it is outside their scope, I am very curious about their (and previous studies') sediment coring approach. As far as I can tell, the authors only measured radiocarbon on macrofossils collected in one sediment core even though there have been 13 cores recovered from this bog according to the text and figure 4. Is there a specific reason the authors noted all the other cores in this manuscript even though I assume they are not reporting radiocarbon dates from any of the other cores? Have the authors (or original core collectors) recovered macrofossils in any other cores to help corroborate the results from the one core presented here? I recommend the authors either shift focus away from, and perhaps even omit mention of the other cores, or present radiocarbon data from the cores if they exist to help support the reported dates. Afterall, the independent age constraint essentially hinges on just 3 three radiocarbon constraints from one section in one core in the bog.	Lang et al. (1984) obtained 13 sediment cores from the Feldsee Bog. To the best knowledge of the authors, the cores do not exist anymore. Therefore, the authors of this study undertook a coring campaign to retrieve new sediment cores. To make this clear, we reformulated the beginning of the methods section as follows: "To the best knowledge of the authors, the cores obtained by Lang et al. (1984) do, unfortunately, not exist anymore. We thus obtained sediment cores at the FSM coring site during fieldwork in 2021 CE."
-	**Figures and tables:** In terms of general comments, I feel that readers would benefit from revisions to some of the figures and tables in the paper. See specific comments below. I also recommend the authors include one additional figure of normalized concentrations from every sampled boulder so readers can more clearly assess the measurement results from each boulder relative to each other (see my comments on table 6 and suggested additional figure), and one additional figure plotting normalized concentrations versus boulder heights for all samples.	We revised some of the figures and tables according to the suggestion of the reviewer. For example, the sampled boulders were added to Fig. 3. In addition, we added the two suggested figures to the manuscript in order to improve the clarity of the text.

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-	**Alternative explanation for the relatively low reference production rate:** The authors identify (Line 250) an important point about post-depositional disruption and exhumation impacting ^{10}Be accumulation. Because this PR calibration site is so much lower than other sites, it forces me to wonder if there really is an exhumation/stabilization issue going on here. I recommend the authors dedicate more discussion around the morphology of the moraines. In my experience, boulders embedded in moraines (as opposed to fully atop or even better, clast-supported) are likelier candidates for exhumation issues, especially if there is local, late Pleistocene seismicity. Additionally, the authors observe a lack of protruding quartz veins on their boulders (Line 610-611), which in my experience could mean the boulders were shielded from weathering and potentially exhumed long after deposition. Can exhumation issues be truly ruled out here? As written, I am not fully convinced, and I recommend the authors discuss this issue in more detail.	The first author of the manuscript undertook fieldwork and carefully double-checked the sampled boulders for protruding quartz veins. In contrast to the sampling campaign, he observed protruding quartz veins on three moraine boulders (on the FS-02b boulder and on two random moraine boulders at position FS-03). The protruding quartz vein on the FS-02b boulder had a height of 1 cm. See Fig. 8b for a photo of the exposed quartz veins on a random moraine boulder at position FS-03. Regarding landform stability, it should be noted that the sampled moraine boulders at position FS-03 were large (see Fig. 10c). Some parts of the moraine at position FS-03a only consisted of large boulders (Fig. 8c), such as the portion of the landform where the FS-03a, FS-03b, and FS-03c boulders have been sampled. We comment on the moraines' morphology in one paragraph in the revised methods section: "Since the study of Tomkins et al. (2021) demonstrated that landform stability mainly influences the scatter in age distributions from moraines (and thus in 10Be concentrations), only well-embedded boulders were selected to avoid underestimated ^{10}Be concentrations due to boulder rotating as well as post-depositional and post-stabilisation exhumation. As the moraine at position FS-03 consisted of clast-supported diamicts and some parts of the moraine were solely composed of boulders, identifying stable and large boulders proved to be straightforward. The same was true for the moraine at position FS-02 although this landform consisted of matrix-supported diamicts. Identifying large and stable boulders on the moraine at position FS-01 turned out to be difficult, as this landform consisted of matrix-rich diamicts and since the moraine exhibited only a few boulders. We thus only sampled one large boulder."

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		"Hofmann and Konold (2023) mapped a kettle on the proximal side of the moraine at position FS-03 and on the moraine at position FS-02 in the centre of the Feldsee Bog (Fig. 3), pointing to paraglacial reworking and delayed moraine stabilisation (cf., Porter et al., 2019). To minimise the risk for paraglacial reworking issues, we avoided sampling boulders in the vicinity of these landforms." We also discuss landform stability and the influence of the height of the sampled boulders in Sect. 6.1.
Figure 3	I recommend the authors give readers some better geographic context for the samples collected, especially given the high-resolution basemap here. Please consider adding dots or some sort of markers to the figure (with labels) for each sample collected. I recognize that the authors more or less did this on Figure 7 but it would be helpful in this zoomed in image. Moreover, the moraine delineations are a little complicated and confusing simply as outlines using the same color for the lines. I recommend coloring each moraine with differing shades of light, transparent fill or something like this so readers can more easily distinguish moraine boundaries.	We added dots and labels in the revised figure. We used different colours for the moraines and added a transect in panel (b) to improve the clarity in Fig. 3.
Figure 4	As previously stated, I am unsure what the purpose is of including every core collected from the bog if they are ultimately not used in the study. I recommend either removing the cores from the figure, or if there is relevant data from multiple cores, include that data in the paper to help corroborate the radiocarbon results from the single (I am assuming?) core. At the very least, the authors need to identify which of the 13 cores on this figure was sampled for radiocarbon dating because I cannot easily tell from the figure. It might even be helpful (if possible at this scale) to put stars or some sort of marker for the relative depths of sample collections for radiocarbon and IRSL.	None of the cores sampled by Lang et al. (1984) was ultimately used for this study. As we only discuss the palyonological data from core "5" in Lang et al. (1984) in further detail, we discared the Fig. 4 in the original submission and marked the coring site "5" of Lang et al. (1984) in Fig. 3.

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Figure 6	A general comment on the approach to generating the age-depth model shown in this figure. Why did the authors not include the IRSL ages in the age-depth model? If they are not used in the age-depth model, I am unsure why they are even included in the study. In fact, if the lowermost IRSL age is considered in the age-depth model, it might impact the modeled independent age, at least in respect to the uncertainty in the modeled independent age assignment. I feel this is important for the authors to reconcile, especially if they are concerned with leaning too heavily on just one independent dating method (Lines 33-37). If OxCal cannot accommodate IRSL ages in the age-depth model, I recommend the authors use different software like BACON to generate an updated age-depth model that incorporates the IRSL ages. Second, could the authors somehow make it a little more obvious in the figure that the tephra layer is hypothesized specifically as the Laacher See Tephra? I got a little confused here.	Thanks for the suggestion regarding the age-depth model. We included both the ^{14}C and the IRSL ages in the model. The error of the basal age turned out to be slightly lower. We marked the tephra in Fig. 5 as "Laacher See Tephra".
Tables 1 and 3	Stylistically, I would recommend the authors combine these tables, I am not sure what the purpose is of separating this information. In fact, table 3 comes after figure 6 in the text so readers see the age-depth model before they even see the raw radiocarbon dates and calibrated ages.	The tables were combined in the revised manuscript.

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Table 6	I am not sure why this information needs to be separate from the information in table 5. Moreover, I am unsure why the authors did not report information for the samples they elected to remove from the dataset. I recommend combining the two tables. I also recommend the authors move this combined table up in the text closer to the paragraph in line 280. As is, I had to scroll back and forth several times between the table and the relevant text while reading. Here and in table 5, based on the information provided, I recalculated ^{10}Be concentrations (and I commend the authors for providing sufficient data to do so), but they are not identical to the concentrations provided. For example, the first sample in table 5 (FS-01a), the authors report a concentration of 134500 at/g but my calculations for that sample were 137938 at/g, approximately 2.5% higher. All other reported concentrations are lower than my recalculations at roughly the same percentage. Except FS-03a, which was somehow 10% lower than my calculation. Because this is a production rate calibration and has important implications for calculating exposure ages elsewhere, I strongly encourage the authors to reaffirm their reported concentrations and/or if my calculations are correct, update the tables and the entire manuscript accordingly. As a final point, I am not sure how useful the 'normalized concentrations' column is. These reported concentrations may be normalized for shielding and thickness, but they are not scaled down to SLHL so one still cannot compare 'apples to apples'. I recommend the authors make the full effort to normalize concentrations by including the scaling factor as well as the shielding and thickness corrections and report the completely normalized values.	Tables 5 and 6 were combined in the revised manuscript (Table 4). As mentioned above, we carefully double-checked the presented Beryllium-10 concentrations. See the attached table for further details. The reported concentrations were scaled down to SLHL for suitable comparison. Fig. 6 in the revised manuscript shows the normalised Beryllium-10 concentrations with respect to the error-weighted mean Beryllium-10 concentration. See Table 3 for fully normalised Beryllium-10 concentrations (at sea-level and high latitudes).

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Additional Figure 1	Building off the fully normalized concentrations that I feel should be reported for every sample measured (even the one with a low AMS current), I recommend that the authors make an additional figure to graphically display the normalized concentrations. My preference would be for the authors to make normal probability density functions for each sample and a summed pdf (e.g., 'camelplot') so readers can see the normalized concentrations in the context of each other to quickly assess the distribution, but I leave that up to the authors how they want to graphically display normalized concentrations.	We added an additional figure to the manuscript (Fig. 6) showing the normalised Beryllium-10 concentration with respect to the error-weighted mean ^{10}Be concentration.
Additional Figure 2	Because the authors are identifying issues with shielding of cosmogenic production, a commonly adopted approach to mitigate some of these issues is by selecting only the largest boulders (higher likelihood of being wind-swept of snow, less likely to have been exhumed post-depositionally or significantly covered by soil/vegetation, etc.), so I recommend the authors consider adding a plot of normalized concentrations versus boulder height. If there is a trend, that might support some of the conclusions drawn by the authors and/or highlighted in this review.	The study site is located in a sheltered position, as a dense forest composed of Norway spruce, beech, and silver fir covers the study site. Sampling large boulders would therefore not help to mitigate the issue of snow shielding. However, we agree that selecting the largest boulders would allow for mitigating post-depositional and post-stabilisation issues. We plotted the boulder height versus the normalised ^{10}Be concentrations to check whether there is any trend. Figure 8a reveals the absence of a clear trend (R^2 = 0.01, p = 0.83). The lack of a clear relationship between these factors supports the idea that other factors (e.g., individual exposure histories) explain the variations in normalised ^{10}Be concentrations.

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Line 74	You surveyed and sampled 3 moraines, correct? Fix please. Could say something like "the bog is situated stratigraphically between some of the moraines" if that is correct.	We reformulated the sentence as follows: "We chose the Feldsee Cirque (8.0 °E, 47.9 °N WGS 1984 coordinate reference system) because (i) we observed multiple large, quartz-bearing boulders on two well-preserved moraines and because (ii) a bog, the Feldsee Bog, is situated in the tongue basin of the former glacier whose sediments are stratigraphically younger than these ice-marginal moraines (Lang, 2005; Hofmann and Konold, 2023)." We hope that it is clear that we included ^{10}Be concentrations in moraine-boulder surfaces at two ice-marginal position in the calibration dataset.
Line 242-244	I am confused by this paragraph. You collected samples from FS-03 and FS-02, and then one sample from FS-01, which is the moraine that dams the lake, correct? As written, it makes it seem like you collected more than one sample on FS-01, which I think is not true. I recommend rewriting this paragraph and including the total number of samples collected per moraine (perhaps in parentheses).	We rewrote the paragraph as follows: "For establishing the BFPR, we collected surface-rock samples (Table 3) from (i) six gneiss boulders on the moraine at position FS-03 and (ii) four gneiss boulders on the ice-marginal moraine at position FS-02. We also sampled the surface of the FS-01a gneiss boulder on the moraine at position FS-01a for age calculations. However, we did not include the sample in the calibration dataset, as moraine formation might have post-dated the onset of deposition of lake sediments at the FSM coring site (Fig. 3b)." For clarity, we added the ice-marginal positions to Table 3 and 4.

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Line 250-252	Here is where I think you could inject a little more discussion on the morphology/stability of the moraines themselves. Are they mostly matrix supported and susceptible to degradation, is local seismicity an issue, etc.	Thanks for this remark. We added information on the additional information on the moraine's morphology and stability: "Since the study of Tomkins et al. (2021) demonstrated that landform stability mainly influences the scatter in age distributions from moraines (and thus in 10Be concentrations), only well-embedded boulders were selected to avoid underestimated ^{10}Be concentrations due to boulder rotating as well as post-depositional and post-stabilisation exhumation. As the moraine at position FS-03 consisted of clast-supported diamicts and some parts of the moraine were solely composed of boulders, identifying stable and large boulders proved to be straightforward. The same was true for the moraine at position FS-02 although this landform consisted of matrix-supported diamicts. Identifying large and stable boulders on the moraine at position FS-01 turned out to be difficult, as this landform consisted of matrix-rich diamicts and since the moraine exhibited only a few boulders. We thus only sampled one large boulder. Hofmann and Konold (2023) mapped a kettle on the proximal side of the moraine at position FS-03 and on the moraine at position FS-02 in the centre of the Feldsee Bog (Fig. 3), pointing to paraglacial reworking and delayed moraine stabilisation (cf., Porter et al., 2019). To minimise the risk for paraglacial reworking issues, we avoided sampling boulders in the vicinity of these landforms."

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Line 305	There are more potential factors that get integrated into a 'baseline' production rate, e.g., glacial isostatic adjustment, atmospheric redistribution, etc. that are elegantly accounted for with reference production rate calibrations. It might be worth mentioning these other factors as well.	We agree that we should have mentioned these factors in the manuscript. We reformulated the sentence as follows: "Following the approach in a previous calibration study (Fenton et al., 2011), a "baseline" production rate was first calculated, i.e., a production rate that accounts for the site-specific bias induced by snow cover, vegetation cover, soil cover, and postdepositional weathering and by other factors, such as changes in atmospheric circulation."
Line 307	Just curious, how do the resulting reference production rates compare between using CREp and the online exposure age calculator? Are they virtually identical?	The production rates are similar. However, the production rate calculated with the online calculators formerly known as the CRONUS-Earth online calculator came with a larger uncertainty (CREp: 3.64±0.11 atoms g^{-1} quartz a^{-1}, CRONUS-Earth: 3.65±0.20 atoms g^{-1} quartz a^{-1}). Note that we comment on the production rate derived with the online calculators formerly known as the CRONUS-Earth online calculator in the results section of the paper. See also Table 6.
Line 320	I am not sure how appropriate it would be to use the erosion rate estimated from Reuther, 2007. The erosion rate is certainly environmentally controlled, but it is also controlled by the lithology – density, age, grain size, etc. Unless the authors specify that the bedrock at their Black Forest site is of a similar lithology, age, density, grain size, etc. to the site in the referenced paper, I feel it would be difficult to assess the validity of using this erosion rate	As we newly identified a protruding quartz vein on the FS-02b boulder, we were able to calculate a site-specific weathering rate (0.06 cm ka $^{-1}$). This weathering rate was based on the basal age of the lake sediments at the FSM coring site and the height of the quartz vein.
Line 328-329	Might be a sentence/spelling error in this sentence.	Exactly. This was a typo.

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Line 465	Just to confirm, the ^{10}Be concentrations reported in table 5 and 6 are the blank corrected concentrations, right? The text is slightly vague here. I would recommend explicitly stating that "values reported in the table are blank corrected" so there is no ambiguity.	We have accordingly revised the manuscript. We hope that it is clear that we only refer to blank-corrected concentrations.
Line 610-611	I think it is a useful finding that there were no protruding quartz veins in the sampled boulders, unlike what was observed in Reuther (2007). To me, this could signify that boulder surfaces were better-preserved and much less weathered than the authors hypothesize. If true, this observation might lend support to the minimally discussed idea of moraine stabilization/exhumation processes impacting cosmogenic nuclide inventories in sampled boulders. I recommend the authors consider and discuss this possibility in more detail.	After sampling, we did not observe quartz veins on the freshly exposed rock surfaces on the boulders. However, we recently went to the field again and carefully inspected the boulders. In contrast to previous field surveys, we noted protruding quartz veins on the FS-02b boulder and on two random moraine boulders at position FS-03. The presence of a quartz vein with a height of 1 cm on the FS-02b boulder suggests that the sampled boulders underwent significant weathering and removal of rock. Note that the weathering corrected production rates reported in Table 5 are based on the site-specific weathering rate. See the previous comments for further details.
Line 652-656	In terms of data availability, I suggest the authors consider contributing their cosmogenic nuclide measurements to ICE-D (www.ice-d.org) for community discoverability and use.	The calibration dataset will be submitted to ICE-D after the acceptance of this manuscript.