Response to review 2 (anonymous)

We thank this reviewer for a careful review. As with the first review, this review was clear and helpful, and we appreciate this reviewer's attention to the paper. Overall, this reviewer was supportive of the paper but had one major concern and a number of minor suggestions.

The major concern has to do with environmental influences on Be-10 deposition in NAVC sediments:

All calculated bi-annual and decadal 10Be and 9Be flux time-series from the NAVC sediments strongly resemble changes in the mass accumulation rate (MAR) and not the variability in the originally measured 10Be and 9Be time-series. This is a common feature when 10Be flux time-series are calculated from sediment records (e.g. Berggren et al., 2013 J. Paleolim., Czymzik et al., 2015, EPSL). However, comparing the MAR with the measured 10Be and 9Be time-series indicates that major changes in MAR are most often not accompanied by comparable anti-phased changes in 10Be or 9Be (e.g. Figs. 11 and 16), suggesting that influences of changes in MAR on sedimentary 10Be or 9Be contents are assumedly rather small. Based on the above assumptions there might be a substantial environmental signal in the calculated 10Be flux and, consequently, 10Be fallout time-series. Please discuss this subject.

This comment is quite dense so we will try to clarify it as we understand it before proceeding.

First, because total Be-9 or Be-10 flux (atoms/cm2/yr) is calculated by multiplying measured concentration (atoms/g) and MAR (g/cm2/yr), the only way that flux and MAR could be *uncorrelated* would be if there was a systematic *anticorrelation* between concentration and MAR.

This could be the case in a situation such as an ice core: if one hypothesizes a constant fallout flux of Be-10, variable snow accumulation rates, and no other sources of Be-10 (i.e., no dust input), then one would observe a perfect anticorrelation between MAR (in this case MAR is the snow accumulation rate) and Be-10 concentration, and equivalently zero correlation between MAR and total flux (because total flux is the same as fallout flux and is constant).

In lacustrine sedimentary records, however, as we discuss at length in the paper and the reviewer points out in this comment, this is not possible unless the sediment is delivered to the lake with zero Be-10. In our situation, the sediment is delivered to the lake with a high concentration of Be-10, so the total flux of "inherited" sediment-bound Be-10 is much greater than the Be-10 fallout flux to the lake. We found that the Be concentration in sediments strongly depends on grain size, but if we assume variable MAR and constant grain size, then multiplying an approximately constant Be-10 concentration by variable MAR always results in a total Be-10 flux that is highly correlated with MAR.

Thus, in a sedimentary system where the majority of Be-10 is already adsorbed to sediment when it is delivered to the lake and only a minority is supplied by subsequent fallout, we expect strong correlations between MAR and total Be-10 flux. The purpose of our linear model for unmixing inherited and fallout Be-10 flux is to remove the effect of variations in MAR (and, more importantly, grain size in our case) from the record of total flux, so as to identify the component of the flux that is not affected by environmental factors and therefore may represent fallout.

So, to specifically address the comments in this section of the review:

All calculated bi-annual and decadal 10Be and 9Be flux time-series from the NAVC sediments strongly resemble changes in the mass accumulation rate (MAR) and not the variability in the originally measured 10Be and 9Be time-series.

This is correct, and is simply a consequence of the fact that the majority of Be-10 delivered to the lake is adsorbed to sediment and is not from fallout into the lake. Thus, changes in the sedimentation rate are equivalent to changes in the total Be-10 flux.

However, comparing the MAR with the measured 10Be and 9Be time-series indicates that major changes in MAR are most often not accompanied by comparable anti-phased changes in 10Be or 9Be (e.g. Figs. 11 and 16), suggesting that influences of changes in MAR on sedimentary 10Be or 9Be contents are assumedly rather small.

Assuming that by "contents" the reviewer means "concentrations" (atoms/g), this is correct. As we note above, observing that MAR and total Be flux are positively, not negatively, correlated indicates that the majority of Be-10 present is inherited and not from fallout. However, a secondary issue is that directly comparing measured concentrations with MAR is oversimplified for our data because, as shown in Fig. 10, Be-10 concentrations in NAVC sediments are strongly dependent on grain size. Thus, the reasoning here would only be strictly valid if MAR was changing but grain size was held constant. Potentially, if MAR and grain size variations were correlated, then changes in MAR would be accompanied by changes in concentration due to the grain size dependence. Leaving aside this complication, however, this statement is essentially saying that because the majority of Be-10 present is inherited, variations in Be-10 fallout have a small effect on measured concentrations. This is true, and we have highlighted this throughout the paper as the major obstacle to reconstructing Be-10 fallout from NAVC sediments.

Based on the above assumptions there might be a substantial environmental signal in the calculated 10Be flux and, consequently, 10Be fallout time-series. Please discuss this subject.

Assuming that "calculated 10Be flux" here refers to the reconstructed *fallout* flux, it is certainly true in a general sense that there might be an environmental signal in the fallout reconstruction. However, we disagree somewhat with the reasoning here. The presence of a dominant environmental signal in the *total* Be-10 flux does not by itself imply an environmental signal in the reconstructed *fallout* flux. The purpose of our linear model based on Be-9 concentrations is to identify variations in the total flux that are independent of environmental factors and therefore may represent the fallout flux. Regardless, an environmental signal in the reconstructed fallout flux might arise for two reasons. First, if we incorrectly applied the model (for example, by incorrectly assuming constant R_S, or using the wrong value) we might infer spurious variations in the fallout flux that were correlated with environmental parameters. Second, there could be true variations in the flux of fallout Be-10 to the sediment, regardless of whether or not we can reconstruct them correctly, that were caused by environmental effects.

We could test for both of these possibilities by looking for correlations between environmental parameters (e.g., MAR, grain size, or total Be flux, which includes both of these effects) and reconstructed *fallout* fluxes. However, if we observed such correlations, we would not be able to determine whether they were the result of (i) incorrect application of the linear model (in which case the correlation would be an artifact of the error), (ii) environmental effects on the Be-10 fallout flux to the lake that were independent of the fallout rate (for example, if precipitation changes varied the efficiency of delivery of fallout Be-10; this could create apparent variations in fallout that did not reflect true changes in the fallout rate from the atmosphere), or (iii) simultaneous solar forcing of fallout rate and climatic parameters (which could increase or decrease the magnitude of recorded fallout variations, but not create spurious variations). However, because we cannot distinguish between these options without additional data, the presence or absence of a correlation between reconstructed *fallout* flux and some environmental parameter cannot be used to prove that we have or have not correctly identified fallout variations.

Because looking for correlations between reconstructed fallout fluxes and environmental parameters, therefore, has limited usefulness in diagnosing the accuracy of our reconstructed fallout fluxes, we did not focus on this in the paper. We did, however, do these calculations, and we observed quite variable results that did not lead to any systematic conclusion about the validity of our fallout reconstructions. One interesting example comes from the Newbury biennial record (Figs. 19-21 in the paper). Figure 1 below compares total Be-9 flux to the sediment, which can be considered a measure of all environmental factors affecting Be delivery to the lake, with reconstructed Be-10 fallout flux using our linear model, for the glacial and paraglacial parts of the Newbury sequence.

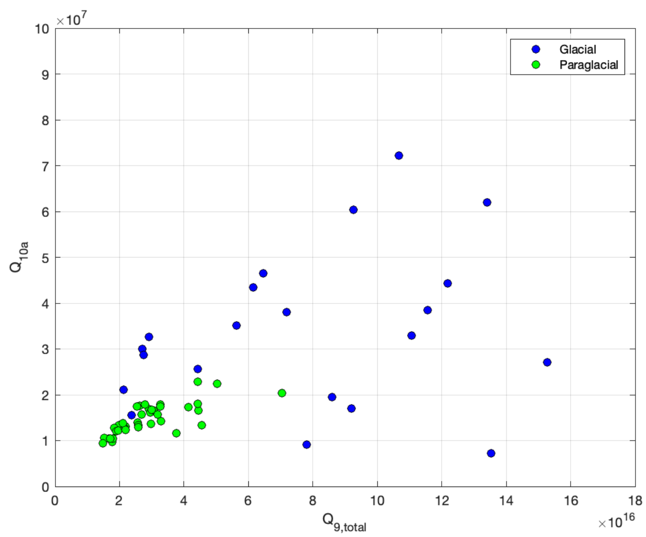

Figure 1: *Reconstructed Be-10 fallout fluxes for glacial and paraglacial sections of Newbury decadal record (section 4.2.2 in paper) compared to total Be-9 fallout.*

For the glacial part of the section (blue symbols), reconstructed Be-10 fallout flux shows weak and not highly significant correlation with environmental effects as represented by Be-9 flux (the correlation coefficient is 0.27; $p = 0.21$). This is consistent with the hypothesis that our unmixing model has successfully removed environmental effects from the reconstructed fallout flux (although, as noted above, it does not prove it). Therefore, this analysis does not provide any evidence that the reconstructed fallout flux is unsuitable for correlation with ice core records. This is important in the overall context of the reviewer's comment that environmental influences may obstruct or invalidate correlations with other records, because much of our discussion about correlating NAVC and ice core Be-10 records focuses on the Newbury glacial section, and this analysis shows that it is not possible to prove that environmental influences invalidate the correlation (of course, there are lots of other reasons that the correlation may be invalid that are not addressed by this analysis at all).

On the other hand, reconstructed fallout flux for the paraglacial part of the section (green symbols) is significantly correlated ($c = 0.7$; $p < 0.001$) with total Be delivery to the lake, which agrees with our argument on p. 34-36 and elsewhere in the paper that Be-10 delivery to the lake under paraglacial conditions is strongly buffered by watershed processes and is unlikely to record true fallout variations.

To summarize our response to this section of the review, the reviewer is correct that the presence of a strong environmental signal in *total* Be-10 delivery to the lake makes it quite difficult to prove conclusively whether or not our reconstructed *fallout* fluxes are uncontaminated by environmental effects. This is one of the key motivations for our approach of defining a systematic procedure for reconstructing fallout fluxes using a linear unmixing model, and then, having done so, proceeding to ask whether the fallout reconstruction resulting from this approach is or is not consistent with expectations for ice core records. To address this in a revised paper, we propose the following:

(1) Add some discussion of this issue to the description of our unmixing model in the methods section on pp. 8-9. Although the purpose of the unmixing model is to separate environmental from fallout variability, we did not make that clear in those words. We can make this clearer for readers.

(2) Add explicit discussion of this issue to the conclusions. Although we allude in various places to the issue of environmental vs. fallout variability, the submitted draft did not include an explicit discussion of whether or not the reconstructed fallout estimates are or are not contaminated by environmental factors. We can add this to section 5.

In the original submission we decided not to include figures like Figure 1 above in this review, because, as discussed above, their diagnostic value is somewhat ambiguous, and they do not lead to any clear conclusion. As this paper is already quite long with many figures, we would like to avoid adding additional ones. We note that because of the open review procedure, this discussion will be available to readers, and in addition we have provided all the data plotted in Figs. 11-15, 16-17, and 19-21 as simple supplementary tables, so that readers can easily make similar calculations in a spreadsheet.

Considering the possible presence of a substantial environmental signal in the calculated 10Be fallout records questions the robustness of the synchronization of NAVC 10Be with Greenland ice core 10Be. Prerequisite for such studies is a reliable production rate signal in all applied 10Be records. This should be considered in the discussion of the synchronization and paleoclimate comparison.

As discussed above, we agree. However, as also discussed above, this is a particularly difficult aspect of this study because with the data we collected, combined with the inherent imprecision of applying our unmixing model in this situation where Be-10 is dominantly inherited and fallout variations have only a small effect on the observed data, there is no unambiguous way to prove that reconstructed fallout variations are entirely independent of environmental factors. Now that we know more about Be-10 systematics in NAVC sediments, it is possible to think of some ideas for how to do a better job of this, for example by focusing on sections of the NAVC where we expect to observe anomalously large peaks in Be-10 fallout that are represented in the ice core records. We have discussed some of these ideas in the conclusions of the paper.

In addition, it would be also interesting to see how the originally measured 10Be concentration and 10Be/9Be-ratio time-series compare with Greenland ice core 10Be fluxes, e.g. to evaluate how much common variability is incorporated.

We agree that this might be interesting, but it is asking a completely different question. Our results clearly show that the total Be-10 (and Be-9) flux to NAVC sediments is mainly controlled by environmental factors, presumably mainly forced by climate. Thus, comparing total Be-10 flux records to Greenland ice core Be-10 fallout records would be equivalent to asking whether solar variability had an effect on local climate. Of course, this question would be better posed by comparing Greenland Be-10 records directly with properties of the NAVC that are more clearly linked to climate, for example varve thickness records, rather than indirectly through measured Be-10 concentrations. With regard to the measured 10/9 ratios, what we are doing in this paper by using a linear model to separate fallout and environmental effects could alternatively be described as simply comparing our measured 10/9 ratios to Greenland ice core Be-10 fluxes – all we have done is apply a linear transformation to the ratio.

That finishes with the main substantive issue in this review. We now address several additional minor substantive comments.

(1) Calendar year time-scale: The Greenland ice core chronology includes its own uncertainties (e.g. Muscheler et al., 2014, QSR). Therefore, in my opinion the NAVC is not synchronized to the calendar year time-scale, but to the Greenland ice core GICC05 time-scale.

Reviewer 1 also pointed this out. Of course, the broader objective is to synchronize the NAVC to both the ice core and calendar year timescales. We will clarify this section of the abstract.

(2) Page 2/28-31: I addition to the atmospheric fallout, please also mention weather and catchment effects on 10Be deposition.

We will clarify this section.

(3) Pages 22/11 to 23/2: Please shortly discuss these two contradictory results, and their implications for the calculation 10Be fallout variability.

A closer look at this section of the text indicates that the submitted version of the paper is misleading in suggesting a conflict here. The purpose of mentioning the Rittenour study was simply to highlight that the only relevant previous work did not show any evidence for the presence of solar-cycle variability in varve thickness variations. Regardless of the Rittenour result, however, the spectral analysis of biennial records in our paper identified spectral power in the 11-year band at 90% confidence only in one out of ten data sets from the NHV and KF sites (5 data sets from each site as shown in Figs. 11 and 12), which is exactly as expected for the null hypothesis in which there is no systematic variability in any of the data sets in this frequency band. Thus, our statement in the original draft that "...spectral analysis of grain size variations...may suggest otherwise" in this section of the text is not, in fact, supported by the analysis. We can clarify this section of the text to make clear that our results are not in conflict with the Rittenour study.

Finally, this reviewer made a number of minor corrections and stylistic suggestions, as follows:

Page 1/Line 4: proglacial and ?paraglacial??

Page 3/15-16: Only one ?Ridge, 2012??

Page3/17-24: Please provide references.

Page5/1: Please shortly described the com- position of the ?glacial sediment?.

Page5/3-8: Provide references.

Page 8/2: Provide reference.

Page 9/31: Provide reference.

Page 18/1: Add ?and discussion? after ?re- sults?.

Figure 1: Caption: Define the abbreviations in the caption. Add information about the meaning of the long black line.

Figure 14: Caption (line 2): Correct ?tallout?.

We thank the reviewer for these suggestions and corrections. These areas can be clarified in the revised manuscript.