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Earth Surface Dynamics Discussions

Interactive comment on "Determination limits for cosmogenic ^{10}Be and their importance for geomorphic applications" *by* Sara Savi et al.

Anonymous Referee #1

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Summary: This ms applies what are considered standard approaches in analytical chemistry to determining detection limits for the analysis of cosmogenic 10Be. The ms uses a long term set of blanks and sample measurements from one lab but multiple operators to make calculations and determine several statistical parameters for the detection of 10Be at levels confidently above the blank.

The manuscript does not break new ground; it is an exercise done to understand the potential limits of one laboratory much more than a significant advance in cosmogenic nuclide science and therefore, not the type or style of paper for publication in a broad readership journal such as Esurf. Overall, this paper presents a data set that most cosmogenic labs have and have likely analyzed internally but the ms does little to advance cosmogenic nuclide science more broadly. As a referee and as a user of this

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literature, I consider the ms to be much more appropriate for an AMS-specific journal such as NIMs and as such suggest it be shortened and submitted for publication there rather than eSurf. It just does not fit well nor will be it be of interest to most of the surface process community. It is technical in nature and does not have any significant geomorphic impact.

Furthermore, the ms is proscriptive and narrow in its approach, which makes it less likely to be accepted by the community. The manuscript does not set the presented data in context because it does not include a critical evaluation of how previous workers have done blank corrections nor does it demonstrate how different blank corrections would change geomorphic outcomes of extant studies. There are several examples in the recent literature where workers have applied several different approaches to blank correction and tested the sensitivity of results to varying approaches; see for example Corbett et al., GRL (2017) on 10/26 ratios. These are not cited nor are they considered critically. The ms reviewed here is under-referenced, omitting numerous important citations both classical and recent that are germane.

Critically, the paper does not consider type 1 vs type 2 errors, that is in striving to be certain that 10Be is confidently detected, such as using 99.9% confidence, samples containing real 10Be are almost certainly being rejected. This will lead to errant science and must be considered head on in any revision before publication. It is a critical flaw and must be addressed before publication in any journal.

Lastly, boron, an isobaric interference is neglected. It varies sample to sample and the means by which it is rejected varies between AMS facilities. It can be a very real component of the blank measurements. At minimum, it needs to be presented and discussed in the context of the ms and the measurements within – better yet would be to consider it broadly across the community. Again, this is narrow, technical information of interest to a small section of the community but critical to the issues here.

In summary, the ms is not appropriate for Esurf, is too narrowly focused on one ap-

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proach to blank subtraction, ignores Type 1 vs Type 2 errors, does not consider 10B interference, and has little critical evaluation of the literature in which the current data need be considered in context. As is, it is a formulaic approach to analyzing an isotopic rather than geomorphic data set and not a significant advance of geomorphic science.

Suggestions for improvement:

I would encourage authors to expand their selection of references in particular citing more studies that make very low level measurements and how these studies have dealt with blanks as well to cite some of the many excellent review papers since 2010 that compile both erosion rate and exposure age data. For example, Carlson and Nelson have recently both published very low concentration measurements from Greenland, one for glacial dating and the other for sediment tracing and neither of these studies are cited.

P2, Ln 25: most AMS measurements in the 10-15 and 10-16 range are not very precise, reword.

P2, Ln 27: This would be an appropriate place to cite Corbett et al. (2016) who review in great detail lab and AMS issues affecting detection including blanks and AMS beam currents. They in turn cites others such as the work on precision by Rood at the LLNL facility.

P2, Ln 30-35: It is not clear why a standardized approach is needed or an improvement on the current approach; there is so much buried in blanks including which AMS, operator changes, real contamination. The paper would likely be better accepted by the community if it were to provide a means or variety of means by which the blank subtraction could be done. The way this section is written presumes the authors have defined “the” way to do blank correction not “a” way to do blank correction. This will not advance the science of AMS and 10Be.

P3, Ln 10: This sentence is incorrect. 10Be IS naturally present in earth materials.

P3, Ln 17: This set of references fails to cite the original 3 references for nuclides in sediments – a critical oversight that needs to be remedied.

P4, Ln1: This sentence omits an important part of running blanks, determining the dark current or background of the AMS system including cross talk in the source. This needs to be mentioned and cited properly.

P4, Ln 5-8: This is at least not correct for our lab and I think not correct for other labs. For us, every sample including blanks gets the same reagent amount and same open beaker time. Otherwise, how could we could compare process blanks and samples and do the subtraction in a meaningful way?

P4, ln 15-20: This section omits a critical issue in defining blanks on the AMS – boron as an isobaric interference and how that is handled. The process varies between AMS (some such as PRIME use GFM that completely removes 10B, others like LLNL make a very uncertain correction, others use post stripping). For low level samples, this is critical.

P4, ln 21-22: This is one way to do it but if the same amount of carrier is added to all samples, then the ratios can be subtracted. This alternative approach needs to be acknowledged and cited.

P4, ln 36: there needs to be more here. How do blanks change over time? Does contamination increase over time in labware?

P5, Ln 7-10: This is far too simplistic. Only considering the upper value of the blank will result is rejecting data that are likely real. The blank subtraction process is a probabilistic one and different for different purposes. The goal of determining whether something is confidently detected is very different than the goal of best estimating the blank for subtraction. The paper would be much stronger if it considered this subtlety.

P6, Ln 4: Unmentioned here is the fact that blanks are very imprecise measurements by their nature and because of the Poisson distributed counting statistics of AMS. The

lowest blanks can contain only a count or two (or even none). Implicit in the discussion above is that the blanks are normally distributed (parametric statistics). If these issues were discussed, the paper would be much stronger. Some of this discussion is in section 3 and could be moved up.

P8, Ln 5: “Although a minimum of 20 values” this has been stated repeatedly. No need to state again.

P8, Ln 35: It is not clear how the average blank constrains any temporal variance? Please explain.

P9, ln 18: I find this paragraph very hard to follow and not very informative. A table or graphic would convey the same information much more effectively. Some of the information is summarized in tables but not all of it.

P11, ln 13: This is a critical mis-understanding of Type 1 and Type 2 errors. Here, the authors suggest that, ”In general, the use of the long-term laboratory blanks (being based on many blank measurements) guarantees more reliable values for the statistics of the blank distribution and for the calculation of the determination limits; as such, they may be preferred.” The approach the authors suggest is very likely to introduce errors of rejection for data (samples) that contain actual 10Be above blank levels.

Section 5.4: This is not an adequate critical review of what others have done with low activity samples.

The data tables for AMS would be much more informative if they included the standard(s) to which the ratios were normalized, more about the boron counts and rejection procedures, comparison of sample beam currents to standard beam currents, the number of gated 10Be counts, and the actual uncertainties of the measured ratios.

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