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Discussions

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Interactive
comment

Interactive comment on "The effect of rock composition on muon tomography measurements" *by* A. Lechmann et al.

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We thank the anonymous referee for the constructive, insightful and detailed comments that we think are very helpful to improve the manuscript. In this Author Comment we will first address the major points that were raised, alongside our answer and how we improve the manuscript by implementing the reviewer's suggestions.

Reviewer 2 raises 3 key issues, the first of which being the uncertainties of the simulation which propagate to the flux ratio, i.e. Eq. (6). This point can be split into two separate problems. On one hand the propagation of the systematic error of the used flux model and on the other hand the propagation of the systematic errors that are present in the cross-sections used for energy loss calculations. Furthermore, it is sug-

gested to use another flux model to validate our results. We agree with the reviewer that, as can be seen in Eq. (1), there are inherent model error in both the lower integration boundary, i.e. E_cut, that stems from errors in the interaction cross-sections, as well as in the integrand, i.e. dI/dE , representing the error on the cosmic-ray flux model. Consequently, the simulated flux, i.e. left side of Eq. (1) must be represented by a probability distribution. However, we want to emphasise the fact, that the flux ratio is a fraction of two simulations, in which the same flux model and the same interaction cross-sections have been used. The only model parameters that have been changed were the density and the compositional parameters Z & A of the material (atomic number & atomic weight). This implies that the errors on the flux simulation in the numerator and in the denominator are correlated one to one. Furthermore, the errors within the simulation can be assumed to be of gaussian nature and independent from each other. These prerequisites allow us to treat the error on the flux ratio in an efficient way. In the revised manuscript, we will prepare an additional figure that will visualise the above reasoning and clarify the origin and the propagation of these errors to the flux ratio. This will be implemented in a new section dedicated to the discussion of the role of uncertainty in our study. Moreover, we will modify our flux ratio figures in such a way that the error will be clearly displayed. We also follow the reviewer's suggestion and show the same calculations for another flux model.

A second issue concerns the lack of experimental data within our study and the request to test our sensitivity study against it. We acknowledge that we cannot offer quantitative measurement results to test and constrain our model with experimental data, mainly because the required data is not available. We do see major advances in this field if quantitative data on the dependency of muon flux attenuation on lithology would be available. Nevertheless, we based our inferences on the same conceptual framework as those that have been applied for standard rock, and as a result of this, we find differences if the rock variables are changed. Therefore, we see the need that interpretations of muon fluxes need to be adjusted according to the geological architecture. We implement this in our revised manuscript by discussing the role of experimental

data for this sensitivity study.

The third point addresses the amount of studied rock types, as to the community it could be more useful to have even more different rocks and soil structures covered by this study. Beyond that, it is requested to extend the rock thickness to 3000-3500 metre-standard-rock equivalent. (which corresponds roughly to 1100-1300 metres of rock). The rock compositions in our study were chosen such that they cover the most important varieties of igneous and sedimentary rocks. All other rocks, with the exception of metamorphic rocks, have a composition somewhere intermediate between these corner stones. The fact of a rock exhibiting a notable lower density can predominantly be traced back to its porosity, i.e. the inclusion of air (or in the case of volcanoes also carbon dioxide) between the minerals. As the material density is very susceptible to changes in porosity, the compositional parameters Z/A and Z^2/A are not affected by much. This can be understood from Eq. B23 and Eq. B24, where the density of the j-th mixture component acts as a weight to the total compositional parameters. This means that the (admittedly rather low Z^2/A value of air and CO2, both are around 3.7, compared to 5-6 for rocks) has practically no effect as it becomes downweighed by a factor $\sim$1000 (fraction between rock and air density). It follows that the low-density rock variants, after a density normalisation, are almost equivalent to their pure counterparts. We want to stress, that a similar problem exists already in the manuscript, as can be seen in Fig. 4. Limestone and Aragonite, that show a density difference of $\sim$10% have the exact same composition. After a density normalisation the difference at 1km thickness is below 1%. We gladly accept the suggestion to extend the simulated rock thicknesses. It remains however questionable if it makes sense graphically, as the figures in the manuscript use a log scale for the thickness, such that 200m more would not be a large extension. On the other side, an extension by a whole log-step, i.e. to 10km, although scientifically interesting, would be practically irrelevant, as the resulting muon flux would be too low for any practical purpose. We add a more detailed discussion on how to cope with low density rocks in the manuscript, as the method remains valid. As for the extension of the simulated rock thickness, we ought to seek a

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compromise between scientific interest, practical relevance and figure readability.

We appreciate all additional suggestions by the reviewer and we will adapt our revised manuscript accordingly.

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