The authors present a summary of the geology of Fuerteventura and present mineralogical and geochemical data to evaluate the viability of some of the rocks and their weathered counterparts as potential sources of the REE. The paper is an interesting thought exercise, but based on the results, the authors should be absolutely clear that these rocks have 0% chance of ever being a REE mine – even on a small scale.

The authors appreciate the reviewer's positive feedback and are pleased that our study has been found interesting. However, we would like to clarify that our work is part of a scientific research, focused on the detailed characterization of the mineralogy and geochemistry of these particularly exotic rocks, within a geological context that has received scant attention. Our study does not, in any capacity, aim to conduct an economic assessment of these lithologies for the purposes of a mining project. This task falls outside the scope and objectives of our research. Therefore, we believe that such a comment is not applicable.

The authors focus on the carbonatites as these have the highest grade – reaching 1 wt.% total rare earths in a single grab sample. While some comparison is made to REE grades in existing REE mines, such a comparison is disingenuous, and in many deposits 1% REE would barely make the cut-off grade.

The authors believe that the use of the term "disingenuous" is completely inappropriate. Our comparative analysis is not disingenuous, nor has it been approached from the perspective of economic geology. Our goal was to juxtapose the concentrations found in the richest sample from Fuerteventura against those from other well-documented locations with significant concentration data. It is clear that a comprehensive economic geology study and the viability assessment of these lithologies as ore deposits would require a broader range of factors to be considered. However, we wish to reiterate, that is not the purpose of our article. Our focus is not on evaluating the mining potential of Fuerteventura's rocks; instead, we present a mineralogical and geochemical comparison with other locations of international renown.

Moreover, the samples presented are relatively mineralogically complex, with the REE spread across three different mineral – with little indication of which mineral would be a focus. The carbonatite bodies are small and discontinuous, further rendering them an economic non-starter. Lastly, all the rocks studied appear to be located in a protected area – perhaps outside of the scope of the thought exercise, but an important point nonetheless, and one that should probably be made and emphasized in case a lay-reader might misinterpret the paper.

The authors regard our study as a presentation of unpublished data in the fields of mineralogy and geochemistry. We reiterate that we are not conducting a mining study. As we have already emphasized in the discussion, conclusions, and abstract of the paper, any mining assessment would require much more information and must adhere, as it can only be, to the current environmental and land management regulations and constraints. We believe these points are sufficiently clear, do not lead to misunderstandings, and are well-argued. Moreover, they have been complemented with the constructive comments of Dr. Anenburg (Review 1 on egusphere). We consider that they do not require more further clarification.

I was surprised by the lack of attention given to the weathered alkaline rocks. Considering these have a profile of a metre thick in places, could there be thicker weathering profiles elsewhere? Should the authors wish to take the thought exercise further, they may way to look into some of the samples as potential ion-adsorption type deposits. Interestingly, only a few of the samples actually seem to have weathered to clay, so I suspect that there will not be a large amount of easily leachable REE, but it may still be worth an inquiry.

The authors disagree with the reviewer's comment. The alteration profiles of alkaline rocks have been described and studied from a mineralogical and geochemical perspective, as can be seen in the results and discussion sections of the manuscript, as well as in the corresponding figures (see, for example, Figures 6 and 7). The study options proposed by the reviewer, which could be very valid, would once again be focused on mining studies. We would like to emphasize again that mining evaluation is not the objective of this article.

Lines 38-53, could be condensed to a few sentences.

The authors believe that this introduction is suitable. It assists the reader in understanding the general context about REEs and offers relevant information that highlights the research objectives. Introductory information can be always condensed (or expanded) based on authors preferences. In this case, we do not see the need to summarize it.

Line 76, the example minerals you give here are fluorcarbonates.

The authors agree with the reviewer and the sentence has been amended according to this comment.

Line 115: remove 'ago'

The authors agree with the reviewer and the sentence has been amended according to this comment.

Line 126: 'associated to' à 'associated with'

The sentence has been amended according to this comment.

Line 319: 'carbonatite profiles'… on line 308-311, you mention that there is no weathering profile associated with the carbonatites, except for the development of calcrete veins. Perhaps you should be more specific on line 319, and say that the samples are of calcrete. How does the formation of these calcrete relate/differ from the calcretes mentioned on L180-184, described as forming from calcarenites, rather than an igneous precursor?

The authors appreciate these comments from the reviewer as they exemplify that certain aspects regarding the relationship between calcretes and rocks of the FBC were not entirely clear. Calcretes are spatially associated with rocks from the Fuerteventura Basal Complex (carbonatites, syenites, etc.), but not genetically. What has been assessed in this work is whether these spatially associated materials have had chemical interaction, especially concerning REEs. To clarify this point, the manuscript has been modified in accordance with the reviewer's insightful comments and questions.

Line 407: It is disingenuous to take the average value of 2581 ppm REE across the whole complex, when the areas which actually define the resource are much higher concentration. No-one would take the felsic rocks from around the mountain pass area into a resource calculation, and the carbonatites have LREE

contents over an order of magnitude higher than the Fuerteventura samples. A single sample of 1 wt% REE, while high for the Canary Islands, would barely make the cut-off grade for many carbonatite-hosted REE deposits. It is also somewhat disingenuous to compare grab samples (especially the highest-grade grab samples) and compare these with resources from a select handful of other carbonatites. The values from most other carbonatites will reflect average grades over an area considered economically feasible to mine.

The authors strongly disagree with the reviewer's assessments. We never claimed that Fuerteventura's carbonatites are economically comparable to other deposits. Our aim is simply to conduct a geochemical comparison of our samples with those from other carbonatites globally, where more extensive data is available. While we acknowledge that our sampling is limited compared to these deposits, we believe our comparisons are illustrative, transparent, and contribute to understanding REE resources in oceanic carbonatites like those in Fuerteventura. We always cite our data sources and present our contribution modestly, aiming for a comprehensive and honest global perspective. While other comparisons or approaches might be more suitable, we reject the notion that our work, based on objective data comparison, is disingenuous.

Line 414: check full stop after 'Figure 11)'

The authors agree with the reviewer and the sentence has been amended according to this comment.

Line 419-427: I wouldn't make too much of this relatively flat HREE profile. The HREE are challenging to extract from carbonatites, and where these profiles are elevated, are commonly hosted in a different mineral to those which can be exploited commercially, and consequently lost during minerals processing. See https://doi.org/10.1016/j.mineng.2020.106617

Line 428-433: That the REE are split between three discreet phases only means they will be diluted further during processing.

The authors consider that these two considerations are made from the perspective of mining treatment and the economic benefit of the mineralizations. As we have reiterated in different answers, this is not the objective of the current article. Our aim has been to characterize the geochemistry and mineralogy of these lithologies in relation to REEs, but we have not conducted any mining study based on the potential economic exploitation of the mineralizations.

Line 434-441: I don't quite follow the logic here. What relevance does the presence of calcite have on the presence of REE-fluorcarbonates?

The authors agree with the reviewer that the sentence was no clear. In fact, the first sentence about calcite was removed according to the comments of Dr. Anenburg, and now has been amended clarifying that REE carbonates are, indeed, fluorcarbonates.

Lines 462-465: the examples given are all of carbonatites with significant weathering and the development of regolith up to (and over) 100 m thick. The carbonatites in Fuerteventura have developed calcrete veins up to a few cm, locally. Why make the comparison?

The authors consider this comparison to be valid, as it has been made to provide the reader with information through examples where there is enrichment in REEs in different lithologies associated with alteration processes, not necessarily in carbonatites. In fact, in the case of Las Mercedes, for instance, this enrichment is associated with karstic bauxites.

Lines 443-472: There's no consideration given here to ion-adsobtion type deposits which, given only the weathering profile above the syenite is developed more than a few cm, could perhaps be of consideration. Ion adsorption deposits require at least 50% of the REE in the weathering profile to be easily leachable using a medium pH reagent, such as ammonium sulfide. In these cases, the REE are loosely bound to clays developed on the weathering profile, and can be easily stripped from the clay and recovered. Ion adsorption type deposits have much lower cut-off grades where relatively cheap in-situ leaching can be applied, and low-grade resources can be economic – especially where HREE contents are high. I am surprised that this avenue hasn't been explored.

We appreciate the consideration provided by the reviewer and will take it into account for future research on these lithologies. However, in the present article, the extraction of REEs through clay treatment at the plant is not an objective of our research. Although it is a very interesting topic, it is beyond the scope of our current investigation.

Lines 475-477: Based on the geochemical data, maybe, but based on the field observations, it is clear that the extremely small size of these bodies does not warrant any further investigation.

Once again, the reviewer confuses basic research studies on geochemistry, mineralogy, distribution of critical elements, lithologies, etc., with studies on mining and economic exploitation of mineralizations. We reiterate that our research is not focused from this perspective, as has already been emphasized in different sections of the manuscript and in this response document.

Line 477-478: Grade is not everything. Size and mineralogy are important too. A large, mineralogically amenable, low grade deposit can be much better than a small, mineralogically complex, high grade body.

The authors agree with this comment. However, these criteria are important from the perspective of mineral treatment and the beneficiation of rare earths in the mining industry. Our work is not focused from this perspective, and this is why these aspects have not been addressed.