

## ***Interactive comment on “The acid-sulfate zone and the mineral alteration styles of the Roman Puteolis (Neapolitan area, Italy): clues on fluid fracturing progression at the Campi Flegrei volcano” by Monica Piochi et al.***

### **Anonymous Referee #1**

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The manuscript examines the mineralogical and geochemical characteristics of the acid sulfate alteration zone in the solfatarica landscape of the Roman Puteolis area (Campi Flegrei volcano, Naples, Italy). New S and O stable isotopic data of the sulfate and sulfide minerals and the fluids and gases emanating in the solfataric field were presented. In addition, the authors propose a model on fluid movement in the subsurface of the field, in comparison with previous data on the area, in an attempt to interpret the collected mineralogical and geochemical data. It is an interesting work which provides some new data and contributes to the understanding of fluid dynamics

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in this terrain, that might extend to other similar sites. Therefore, it would be suitable for publication in Solid Earth. Nevertheless, the manuscript has some weaknesses which need to be addressed prior to acceptance. These weaknesses pertain mainly to the mineralogy results. Also, the discussion is verbose and often unfocused and the section 4.1 is not actually discussion but presentation of some data. Finally, the language needs improvement. Often the authors use terms which are not acceptable. For instance, the term realm should be replaced by assemblage. Also there are not FTIR peaks but bands. I have made several comments on the annotated manuscript. Some general points to be considered are underlined below. 1. The FTIR technique is not appropriate for detailed studies in multimineralic samples because of band overlapping. There are too many oversimplified assumptions which are not justified by the data. 2. The spectral range  $>4000\text{ cm}^{-1}$  belongs to the NIR region and the bands in this region are actually combination bands (not stretching and especially not bending bands). The text referring to these results needs total re-writing. 3. There is bad use of some references (e.g. Parafinium & Kruszewky, 2010). The band assignment in this work refers to ammonium alunite-jarosite. There are important differences with this study with the most important being the OH-stretching region. 4. I do not understand why the authors did not present XRD traces at least as a supplement, which would verify the presence of some phases. XRD is the most suitable and reliable method for this type of materials. 5. The presence of illite/montmorillonite at Solfatara is at odds with the environment of acid sulfate alteration and should be explained/discussed by the authors. Also, illite and montmorillonite are very easily distinguished by XRD. This is another reason why XRD data should be presented at least in supplement. 6. In general, the distribution of clay minerals is not presented clearly. Well-ordered kaolinite seems to be present but this also has to be verified by XRD. Table 3 does not include illite and montmorillonite, although these minerals are not included in the discussion and the results sections. However, this table includes local presence of halloysite which was not described in the text. In any case, how was halloysite distinguished from (disordered) kaolinite? 7. Most of section 4.1 contains actually results. It has to

be condensed considerably. One of the main conclusions about the role of climatic conditions (e.g. humidity) is well-known. Actually most of the sulfate salts are very soluble. Also, the discussion in sections 4.2 and 4.3 should be more focused. The biological imprint has not been verified in the text. The authors might wish to consider organic matter, present in the underlying sediments, as a potential source of N. 8. Several other points have been marked on the annotated text.

Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2019-53/se-2019-53-RC1-supplement.pdf>

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