Overall, the revised manuscript has improved significantly and addressed most points made by the reviewers. However, two significant comments (made by both reviewers) were ignored or addressed only very briefly, which I did not find satisfactory.

Note: I have copy-pasted the original comment (blue) and response (green) from the author's response document, and write my new comment below (yellow). Some parts of specific relevance are marked bold.

> 2. In part, the comparisons made with other studies in the results and discussion sections are difficult to justify, or they are not explained well enough. For instance, I had trouble seeing how absolute permeability values of a single fracture of a real sandstone can or should be compared to permeabilities of fracture networks in an unspecified material (probably synthetic). The same applies to the comparison with trends of PH-derived permeability of porous media.

> We partly agree. The comparisons presented in this study are used to show that persistent homology is used for permeability assessment in a single fracture in addition to porous media and discrete fracture networks. In addition, the general trend is to be shown that persistent homology slightly overestimates the reference value. It is by no means intended to draw conclusions that a single sandstone fractures (bedding plane) behaves exactly as 3D printed discrete fracture network (DFN). Nevertheless, we elaborated the section (L286-L296): "Of particular interest for this study are the permeabilities of fracture networks, which are displayed as dark gray diamonds in Figure 5, since they are also based on fractured instead of porous material. In general, it can be identified that permeabilities of fracture networks are distributed closer around the 1:1 line compared to porous media values (light gray crosses) in Figure 5. **In addition, it is also not surprising that the results of this study have permeability values closer to those of fracture networks rather than porous rocks. This is due to mechanical aperture of the individual fractures, which form a fracture network, being of a similar order of magnitude to the single fracture investigated here.** Since the most values from fracture networks are results of the analysis of fracture networks with plane fracture surfaces in the study of Suzuki et al. (2021), it is possible to estimate the influence of surface roughness as well. **The rough single fracture studied here shows the same trend of permeabilities, the majority of which are overestimated slightly, as the planar fracture networks addressed.** This suggests only a minor influence of the roughness on the final result of the PH analysis. However, it should be considered that typically fracture surfaces have roughnesses of H > 0.5, whereas the roughness of the used fracture is slightly lower (Hx = 0.48 and Hy = 0.42)."

- I understand the intention, but here the comparisons remain questionable in my view:
  - The authors state it is not the goal to suggest that fracture networks and single fractures behave the same. Yet, they bring

up **absolute** permeability values for comparison to make the results seem plausible. Two “fractured media” are insufficient as grounds for such a comparison of absolute values. Matching absolute values may be coincidental, even though the mechanical aperture seems to be similar as stated. But I would actually be surprised if in that case a non-rough network and single frac result in the same k values.

  - NOTE: the stated goal of the paper is to show the applicability of PH for single fracture permeability estimation. This has been shown nicely in the paper by comparing to other methods. I’d argue that this is the value of the present study. The comparison of the results to absolute values of other studies with different study objects is unnecessary and hard to justify. I srongly suggest to delete them.

- On the other hand, it is useful to compare trends for overestimation (if any, see comment below) and state that they seem in line with other studies. This is because one compares methods, not specific samples/media.

3. The authors conclusions on overestimation of permeability when using PH do not seem to convincingly match their own data. Note that is not necessarily a bad thing, because the presented permeability estimates match those from the other methods rather well.

NOTE: this is similar to the comments of reviewer 1 for L261 and L271 in the original submission. It seems like the authors chose to ignore these two comments entirely, which in itself is unsatisfactory.

We partly agree. The data shows that the experimentally or numerically determined reference p are slightly exceeded for the majority of the estimated permeabilities in this study (67 % of estimated permeabilities exceed their reference value). In fact, the overestimation of permeability is rather low compared to the other permeabilities presented. Since the same trend can be seen in the study of Suzuki et al. (2021), we have included the conclusion on overestimation of permeabilities in this study.

- I remain with my concern that the conclusion on overestimation seems rather forced given the presented data. I suggest to rephrase it (see below). Interestingly, even the authors themselves state that ”the overestimation of permeability is rather low compared to the other permeabilities presented”. Then why is overestimation presented as one of the conclusions, rather than for instance a “good match”?

- Specifically, the 67% (8 of 12 points) seem like a clear trend, but I think the presentation of only this number is a bit misleading. This is because the 12 points only represent 6 measurements, compared to two different methods at 3 different resolutions. Looking closely, it is clear that the conclusion on over-/underestimation is also resolution and direction-dependent. In my view, this is a strong indication that a general

statement in terms of over- or underestimation is not possible based on this data.

- As a hopefully constructive suggestion, I think the authors can conclude that differences to the results of other, established methods are small (=good match), and more data are needed to analyse the impact of resolution and anisotropy on the results. In this context, the results of Suzuki et al. (2021) can still be stated to give scientific context – but the presented study here does not convincingly confirm their results in my view.
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