Comment on essd-2022-279
Anonymous Referee #2

The submitted manuscript presents a database—called Valgarður—that contains petrophysical, mineralogical, and chemical properties of rocks from Iceland. Although there are gaps in the database, such as the lack of mechanical and thermal properties, I think that it will prove to be a useful resource. I recommend publication after the following comments have been addressed to the satisfaction of the editor.

Line 70: Valgarður.

Figure 2: “Kr” is Krafla?

Table 2: I think it’s odd to describe lavas as “fine-medium grained” and “medium-coarse grained”. Lavas do not contain grains, but crystals. Would aphanitic and porphyritic be more appropriate descriptors?

Line 153: The authors could refer here to the lava shown in Figure 1.

Line 165: Suggest to change “ash flows” to “pyroclastic density currents”.

Line 175: Can the authors refer the reader to the paper(s) describing these rocks, or are these observations unique to this study?
Line 194: Presumably the measurements of permeability were made under a small confining pressure?

Table 4: Acronyms should be explained in the table caption.

Line 203: I suggest to add that “effective porosity” is often called “connected porosity”. This porosity is that connected to the outside surface of the sample. I suggest that the authors refine their definition here.

Line 218: Suggest to change “brine” to “liquid”.

Line 224: It might be worth noting that this may not be the case for clay-rich, altered materials.

Line 225: Another source of error is that helium pycnometry requires the sample dimensions. The triple-weight method, however, only uses measurements of weight. Laboratory measurements of weight are often more accurate than measurements of length, which is another advantage of the triple-weight method over helium pycnometry.

Line 227: Unless dried/filtered, air can also contain water, which can also influence measurements of porosity (especially clay-rock, altered materials).

Line 229: Given the variability and heterogeneity of volcanic rock, this seems very likely.

Line 250: Presumably this is because the alteration mineral assemblage formed at depth contains denser minerals? Is this true?

Line 259: Higher, and also much more accurate. I think that the authors should clearly state that estimating porosity using thin section images is problematic and often underestimates the porosity. For example, it is often not possible to identify micropores and small microcracks on thin section images, which can form a large proportion of the porosity in some volcanic rocks.

Line 264: What was the confining pressure used by Gudmundsson et al. (1995)?
Line 265: What is meant by the “stationary method”? The steady-state flow method?

Line 287: The authors should also discuss the Forchheimer correction. This correction is often needed when measuring the permeability of porous rocks using gas. The data were also checked for the Forchheimer correction? If yes, the authors should discuss this here. If not, I think that the authors should clearly state that these data were not checked for the Forchheimer correction, and so may not represent the “true” or “intrinsic” permeability.

Line 289: The authors should state/discuss whether these data are influenced by rock type. It’s also interesting to note that the lava samples cover almost the entire permeability range.

Line 289: What was the concentration of brine used?

Line 290: The authors should offer a reason for this here, in my opinion. This difference is often attributed to the presence of swelling clays (see, for example, Tanikawa and Shimamoto, 2009). Even in clay-free volcanic rocks, liquid permeabilities can also be lower than gas permeabilities due to water adsorption on narrow, tortuous microstructural elements (see Heap et al., 2018).

Line 292: Is it not worth adding a plot that shows permeability as a function of porosity? It would be interesting to show whether permeability increases as a function of porosity, as seen in, for example, Farquharson et al. (2015). Is it worth adding another plot that differentiates the data by their alteration?

Line 353: Why not show formation factor and/or surface conductivity as a function of porosity for the data in the database?

Lines 356-357: These acronyms have already been defined above.

Line 370: Is there a reference for these standard techniques?
Line 410: Is it worth adding another plot that differentiates the data by their alteration? Or providing a plot that shows that the saturated velocities are higher than the dry velocities?

Line 411: I think it would help to state that this trend of often seen for rocks, including volcanic rocks (with references).

Lines 412-413: The authors should provide a reference in support of this statement. The scatter in these data is a result of the fact that porosity is just a scalar, and elastic wave velocities are sensitive to the nature of the porosity (microcracks versus pores).

Lines 418-420: Although the data are few, I think the authors should offer more details as to how these data were collected and, briefly, describe the data obtained.

Line 443: I think it would also be interesting to measure thermal properties.

Line 446: “these important data”