

Review of the manuscript

## **Novel insights into deep groundwater exploration by geophysical estimation of hard rock permeability**

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This manuscript uses controlled-source audio-frequency magnetotellurics (CSAMT) resistivity inversions and borehole information to create an empirical relationship between apparent resistivity and permeability of an area within the Jinji region in China. This empirical relationship allowed them to estimate 2D and 3D fields of permeability for the whole region and interpret areas where groundwater has the potential to be present. As the authors point out, their approach has the potential to cut down on the need for costly borehole tests. It allows for a more thorough assessment of aquifer potential, providing a tool in deep groundwater exploration.

I have read the manuscript with interest. The manuscript requires considerable work. The authors need to provide clarifications in parts of their methods and conclusions, and rephrase parts of their manuscript to correct for misleading statements. Below, I have listed comments, hoping they may help improve the manuscript's quality.

### **Specific Comments**

1. The authors should be mindful and careful while presenting their results and conclusions, as some of the sentences throughout the text can be misleading. Below are some of the examples.
  1. Lines 42 to 35 in the abstract state, “The results demonstrate that CSAMT can effectively characterize deep subsurface variability and generate **accurate**, spatially continuous hydrogeological models in hard rock terrains, particularly where drilling data are limited or **unavailable**.”
    - i. First, all these resistivity-based geophysical methods rely on ground data, such as boreholes, to aid in their interpretation. In fact, the authors use borehole data to construct their empirical model. So, it is misleading to say that this approach will work in areas where ground data is **unavailable**. Consider rephrasing this part.
    - ii. Secondly, CSAMT relies on ill-posed inversions. Thus, stating that this geophysical method can provide an “**accurate**” hydrogeological model of the deep subsurface can also be misleading, especially if there is not enough information at depths of 1 km to validate these results. Consider rephrasing this part of the statement as well.
    - iii. Finally, the abstract and conclusions are misleading, as they emphasize the importance of using CSAMT for characterizing hydraulic parameters, specifically permeability, but they fail to mention the crucial role of ground information in fitting the empirical model and validating their results. I urge the authors to consider adding these silver linings to their study, as borehole information is essential in their research and is not mentioned until later in the manuscript.
  2. In lines 39 to 40 and other parts of the manuscript, the authors state that this study introduces, for the first time, the use of CSAMT to estimate the distribution of permeability. While this reviewer could not find other work specifically using CSAMT

- to estimate hydraulic properties and state variables, there have been other studies where resistivity-based geophysical methods have been used with this objective (e.g., Daily et al., 1992; Herckenrath et al., 2012, 2013; Hinnell et al., 2010; Pollock & Círpka, 2012), using techniques known as hydrogeophysical inversions (A. Binley et al., 2010, 2015; Ferré et al., 2009). I encourage the authors to look into this literature and acknowledge these advances in their introduction, and discuss how the advances provided in this manuscript extend this body of literature.
3. In line 98, the authors say that “Permeability is a crucial parameter for characterizing the ability of geological formations to store and transmit water.” This is partially correct, as permeability refers to the ease with which a fluid moves through a porous medium. However, this parameter is not a measure of the porous medium's ability to store water. Storage is better characterized by porosity. Consider correcting this statement.
    - i. A similar statement is presented in lines 114-115: “... while still enabling accurate evaluation of groundwater storage capacity within prospective rock formations.” Are the authors aiming to characterize other parameters besides permeability? Please clarify.
  4. In line 173, the authors state, “Numerous parameters significantly affect permeability.” Which parameters affect permeability? Consider being more specific.
2. This reviewer acknowledges the current length of the manuscript. However, I encourage the authors to mention other important electromagnetic-based geophysical methods in the introduction briefly. Such as Magnetotellurics (MT) and time-domain electromagnetics (TDEM), which allow for deep vertical exploration. Can these same results be achieved with such geophysical methods?
    1. Incidentally, Magnetotelluric information has been used for the interpretation of circulation patterns in mountain systems (Jiang et al., 2014), and has the potential for detecting regional groundwater flow paths that supply water to lowland aquifers (Gonzalez-Duque et al., 2024). Consider this information for discussing the results in light of groundwater assessments and security.
  3. As the authors state in lines 196 to 198, “A selected number of boreholes were strategically drilled at critical points within the study area. Following this, several CSAMT lines were conducted, encompassing both the borehole locations and their surrounding zones.” How were the locations of these boreholes determined? Is there a certain number of boreholes needed for the method framed in this study to work? What would happen if the authors removed one borehole from their research and validated it at that point? Would you get the same field of permeabilities? These are questions that arise from the approach taken that the authors should address in the text.
  4. In the sentence from lines 486 to 488, the authors state that “The resistivity-porosity can be indirectly extended to infer permeability, especially when combined with other petrophysical models (Andrew Binley & Kemna, 2005; Revil & Cathles III, 1999).” The citation of Binley & Kemna (2005) is misleading because it focuses on direct current resistivity and explains some physical concepts that are not directly related to the sentence. Consider removing this citation from the paragraph.

5. Similarly, the statement in lines 493 to 495 could potentially have misleading references. First, in Jardani et al. (2007) there are no empirical or semi-empirical models that connect electrical resistivity to permeability. Consider removing it. Also, this author could not find the citation (Jiang et al., 2014), with the reference presented below. Consider adding the DOI number in all the references.

Jiang, Y., Wu, X., and Shi, Z.: A novel model to estimate permeability from formation resistivity, *Journal of Petroleum Science and Engineering*, 124, 15–23, 2014.

6. The presentation of the Kozeny-Carman equation in this study seems out of place. While it is important to stress how this equation is used in the field, there is no use of the equation in the study. Consider emphasizing the practical application of the relationship and removing the equation if it is not used.
7. The empirical relationship found in the study (Equation 4 and Figure 5) is interesting and seems to have a pronounced change around  $1,000 \Omega \cdot \text{m}$ . This relationship reminds me of previous permeability models fitted to depth that use an exponential model to calculate permeability near the surface and a potential model after a depth threshold (e.g., Ingebritsen & Manning, 2010; Manning & Ingebritsen, 1999; Saar & Manga, 2004). Have the authors considered using a similar modeling approach for their empirical relationship? Is their new model better than these previous approximations that depend on depth? Consider adding some discussion.
8. I recommend adding more information on the reasoning behind selecting the thresholds for resistivity to define each of the primary lithologies, as picking other thresholds can potentially change the conclusions of the manuscript.
9. One of the main concerns with the results is the direct linking between the lithology permeability and the apparent or bulk resistivity obtained from the CSAMT inversion. As the authors mentioned several times through the manuscript (e.g., lines 170-173), “Electrical resistivity [...] is influenced by various factors. These include rock type, porosity, weathering extent, connectivity of pore network, saturation levels, structural features like faults and fractures, and the salinity of pore fluids.” However, there is no mention of these other factors in their study. Do the authors know the salinity of the water after 1 km? How do the authors validate that the resistivity results they see at these depths are specifically due to the porous media characteristics and not to the presence of brackish or saline water or a combination of both? Idealized simulations on mountainous systems suggest that the salinity of water, combined with the lithologic characteristics, can change the bulk resistivity inversions and, thus, their interpretation (Gonzalez-Duque et al., 2024). I recommend that the authors discuss these potential limitations in their study, as not accounting for these changes could lead to misinterpretations.
  1. Additionally, the authors mention doing a 2D and 3D groundwater assessment of the water-bearing capacity of the rock mass for groundwater evaluation using solely the permeability fields. A water-bearing assessment should also include the storage capacity of these lithological units that can be related to other hydraulic parameters, such as porosity. Did the authors calculate porosity, specific yield, and specific storage

as well? Do they have information on water levels in the area to validate where the aquifer is located? I recommend clarifying these definitions and correcting the text when needed.

10. It is not clear to this author how the 3D model was calculated. Did the authors perform a 3D inversion, or are these values interpolated? What is the certainty in their estimations at the corners, where no data is present? Please provide clarification and a discussion on the potential uncertainty behind these estimations. I recommend that these estimations of uncertainty or sensitivity be shown as contours in the figures with 3D interpretations.
11. It is not clear how the matching percentages in section 3.5 are calculated. I recommend adding this equation for interpretation purposes.
12. I encourage the authors also to consider releasing the tables with the permeability and sounding information for reproducibility.

### **Technical Corrections**

Besides the comments described above, I have a few technical recommendations for the manuscript.

1. Part of the methods and results in the manuscript feel redundant. Information that is presented in tables is described in multiple ways in the text. I encourage the authors to condense the information to reduce the overall length of the manuscript. As an example, the 2D and 3D groundwater assessment and the depth-wise groundwater assessments sections can be condensed into one, reducing the description of specific depths for each profile (which can be seen in the figures) and following a storyline that avoids repetition.
2. In the abstract (line 37), when the authors refer to “shallow depths,” I recommend clarifying what they mean by that, specifically whether it is greater than 200 m, 500 m, or 1 km.
3. A citation is missing at the end of the sentences between lines 167 and 170.
4. Similarly, a citation is missing in the sentence between lines 170 and 173.
5. Figure 1a shows the profiles taken in the study region. However, there is no information related to topography, geology, and/or water bodies. Consider adding this information on the figure to give context to the reader, especially if this information is provided in lines 242 to 249 and the paragraph beginning in line 250.
6. Remove one of the periods in line 369.
7. In line 746, consider changing “%match” to “percent match.”

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