REVIEW 1: *RC1 - Anonymous*
REVIEW 2: *RC2 - Alireza Malehmir*
REVIEW 3: *EC1 - Editor - Elias Lewi*

	Review	Comment	agreed / not agreed	Answer
General comments	RC1	The main problem with the manuscript is the extensive use throughout the article of the term "subrosion", which I strongly discourage. This because the term is not established in the scientific literature about sinkholes, and represents a potential source of confusion. I would suggest to use, as in the first line of the article, subsurface dissolution (which in many cases include also the leaching process). See also comments in attachment.	Agreed	We changed the term 'subrosion' according to the reviewers suggestion to 'subsurface dissolution / solution'.
	RC1	At a greater detail, the existence of a well-established classification (proposed by Gutierrez et al., 2014, with recent developments by Parise, 2019, 2022) should be considered as reference point, and the interpretation of the geophysical surveys including the attribution to mechanism of origin, should be done in accordance with the categories of the classification above. In many parts of the manuscript I pointed out the confusion deriving from using the term subrosion, I really hope the Authors could take into account such observations and comments. As for the sinkhole classification, as mentioned above there have been in the last years some updates published in the Encyclopedia of Caves (3rd edition) and in the Treatise on Geomorphology (2nd edition). I would suggest to quote also these recent developments. Below the complete references: Parise M., 2019, Sinkholes. In: White W.B., Culver D.C. & Pipan T. (Eds.), Encyclopedia of Caves. Academic Press, Elsevier, 3rd edition, ISBN ISBN 978-0-12-814124-3, p. 934-942. Parise M., 2022, Sinkholes, Subsidence and Related Mass Movements. In: Shroder J.J.F. (Ed.), Treatise on Geomorphology, vol. 5. Elsevier, Academic Press, pp. 200–220. https://dx.doi.org/10.1016/B978-0-12-818234- 5.00029-8. ISBN: 9780128182345.	Agreed	We incorporated the suggested classification by Gutierrez et al., 2014 and Parise 2019 into the manuscript and we changed the wording about the mechanisms from which the interpreted sinkholes originate.
	RC1	Risk: the term risk is not used in the proper way, in my opinion. In natural hazards, risk comprises all damage caused by natural processes, and include the economical and societal costs. These are not dealt with in the present manuscript, and the term risk is used with a meaning that should be (in my interpretation) corresponding to susceptibility, or, if including also temporal information, on hazard. I suggest therefore to change in the manuscript, and in the abstract as well, the word risk.	Agreed	We changed the term 'risk' to 'hazard/hazardous'.
	RC2	My main issue is the lack of details on the seismic data and the processing works. While the P-wave sections have good quality and show distinct and promising reflections, the SH sections are rather poor and risky to rely on. It would be good to spot any of the reflectivity in the shot gathers and show where they have ended up in the final sections. I am otherwise afraid to say that the SH sections have failed where the much of introduction attempts to say it would provide high-resolution images of the subsurface. Following this, I think much of the SH section interpretations are overdone or mainly based on the P-wave and gravity modelling work, which is fine but then the text and introduction should be adjusted.	Partly agreed	According to the reviewer's advice, we now provide more information on seismic data acquisition and processing. Also, as suggested, we now show images of individual shot records in which a prominent reflector from the final section is traced. However, with regard to the criticism of the SH-seismic interpretation, we cannot agree. It is true that the geologic model used in the gravimetric modeling is from the P-seismic. However, as described in the paper, it was never the goal of the SH-seismic to create a regional structural model. The SH-seismic is used to image the near-surface structures in interesting areas with high resolution and we have succeeded in doing so. Furthermore, we show that only by a combined interpretation, all relevant structures, e.g. the fractured areas, can be detected. We also show this in the chapter "Seismic facies analysis" where we explicitly compare the reflection patterns of P- and SH-waves and show that the same areas can have different characteristics. In the corresponding table we also show the improved resolution of SH-seismics in the near-surface compared to P-seismics, without which the detailed imaging of e.g. the collapse structures would not have been possible.
	RC2	Some resharpening of the text should be needed like: (1) P-wave and SH-wave, can be P- and S-wave … (2) Avoid using get and replace with "obtain"	Partly agreed	(1) Since in the last few years more and more studies have been published in which SV-waves were used, we have decided to leave the term "SH-wave" in the text as it is, since this makes it clear to the reader which type of wave we used in our study. Otherwise misunderstandings could arise. (2) We have replaced "get" with "obtain".
	RC2	As for extreme slow S-wave velocity please also note our works presented recently at NSGEAGE 2021 with S-wave reflections imaged in the vertical component data when spatial and temporal sampling was done in a great resolution (Malehmir, 2019 and 2021). You may also look into our work where we combined a similar approach for fault mapping in Sweden: Post-glacial reactivation of the Bollnäs fault, central Sweden – a multidisciplinary geophysical investigation (Solid Earth, 2016)	Agreed	We incorporated the reference about the S-wave imaging using vertical sources and receiver (Malehmir 2019).
	RC2	Avoid naming so many commercial names and software in the main text and move them to the acknowledgments.	Not agreed	We have decided to leave the software names in the text, because they don't fit into the acknowledgement. Since we used almost only commercial software for which we had to pay, albeit for pure research, it would be unusual to thank a company in the acknowledgement for whose product we had to pay. If we had received the licenses, e.g. for ProMAX, for free, it would have been different.
	EC1	As gravity data modeling is highly non-unique, it would have been appreciated if the authors try to describe the methods, they have used to constrain the density of the different layers and their shape. The integrated approach including borehole information can be a very good spring board for this analysis.	Agreed	We already mention the constraints we used to define the shape and the densities of the layers at different places in the text. On page 9 lines 181 to 182 and on page 18 lines 362 to 363 (in the first version of the submitted manuscript), we mention that we used the interpretation of the reflection seismic profile and a crosssection through the geological map as structural constraints. And on page 18, lines 363 to 365, and page 18, lines 376 to 379, we mention that the densities we used for the modelling are mean values from geophysical textbooks (e.g. Hinze et al. (2013)) and from forward modelling of adjacent areas (e.g. Gabriel et al. (2001), and references therein). And we state that locally-varying densities of the Permian, especially due to the dissolution of salt are plausible. However, in order to make it clearer to the reader, we have moved this basic information regarding the modelling parameters from the chapter 'Interpretation gravimetry' to the chapter 'Data processing - gravimetry'.

Chapter 2 - Geology	RC1	page 3 line 73 -The issue of salt springs in the Khyffauser hills is very interesting, and might deserve some additional detail. Is there any reference to hydrogeological works in this area? Could these (if existing) could be useful for a deeper understanding of the sinkhole problems?	Not agreed	In fact, we are not aware of any comprehensive hydrogeological work for the area on the southern edge of the Kyffhäuser, except for measurements of the salt content of the water. There is only one paper from 2013 (Adler et al.) that deals with a tracer test in the Barbarossa cave, which is located about 9 km to the northwest of our study area. In this work, it is also mentioned that there are oral traditions that tracer tests were already carried out in the 1950s, but they were not documented or their records have been lost. Therefore, we cannot discuss the hydrogeology of the salt springs in this study. Nonetheless, a detailed hydrological study of the area would be an interesting topic for a future project. Adler, A., W. Gossel & D. Mertmann (2013). Tracerversuche mit Na-Fluorescein in der Barbarossahöhle, Kyffhäuser. Aufschluss, Jg.64, H.2, S.109-17.
	RC2	page 2 line 54: what is 5 km2 "sink"? Looks strange wording.	Agreed	We changed the word from "sink" to "sag".
Chapter 4 - Data processing	RC1	page 8 line 145: Same sentence as in line 136. Could they be merged to avoid repetition? (see addtitional comments in attachment).	Agreed	Since we also need references regarding data processing in this section, omitting them is not an option. Instead, we have rewritten the sentence in the chapter "Data processig - SH-wave reflection seismic " so that they are no longer identical.
	RC2	page 7 line 130: You mean 3 repeated shot records vertically stacked?	Agreed	Yes. We have changed the wording in order to clarify this point.
	RC2	page 8 line 142: It is NMO corrections (plural) and static corrections	Agreed	We changed "...normal-moveout (NMO) correction, and residual statics correction." to "...normal-moveout (NMO) corrections, and residual statics corrections."
	EC1	page 9: ETRS89, which is the European Terrestrial Reference System 1989, is an Earth-Centered, Earth-Fixed reference system, which is based on the GRS80 ellipsoid. On the other hand, DHHN92 is a height system above mean sea level ("Höhen über Normalhöhennull, in DHHN2016"). From the first paragraph under section 4.4 it can be understood that the authors used the Somigliana's closed form formula to compute the normal gravity and they have computed the Complete Bouguer Anomaly using the formula given by Hinze et al. (2005). In that case, the effect of the geoid undulation on the data will not be taken care because the normal gravity is computed on GRS80 and the height used to compute the Complete Bouguer Anomaly is an orthometric height. In other words, the effect of the height between the ellipsoid and the geoid is not removed, though it will most probably, shift all data points constantly as your area is small. However, from the computational point of view it is still a mistake, and this constant shift should either be mentioned or the processing should be done using geometric height. This is well explained in the paper which the authors have cited (i.e. Hinze et al., 2005). They should have used height above the ellipsoid not above mean sea level. In that case also they have to remove the EGM96 geoid undulation from the DEM model as most of these models have geoid undulation from EGM96. As they haven't mentioned which DEM model, they have used it is hard to comment on this.	Agreed	This comment refers to the so-called "geophysical indirect effect" in the calculation of gravity anomalies. Traditionally, Bouguer anomalies are calculated using orthometric heights (reference: geoid), whereas the calculation of the normal gravity is related to the ellipsoid. Thus, from a physical point of view, the calculation is inconsistent. The data used in this paper indeed follows this traditional approach. However, as the area of investigation is small, this error does not affect the interpretation. In Germany, the mean value of the geophysical indirect effect is ca. 8.6 mGal (Skiba, 2011). Differences from this mean value vary between -1.5 mGal and 2 mGal. In Northern and Central Germany, these differences define a rather smooth regional N-S and NE-SW trend. Variations in the local investigation area are below 0.1 mGal and, thus, negligible. Moreover, a constant shift of the Bouguer anomalies by 8.6 mGal would not change the interpretation. Forward modelling, as done in this study, aims at modelling relative differences in the Bouguer anomalies along the profile, not absolute values. We decided to address this methodological aspect in the text instead of performing new calcucaltions, because figures 9b and 9c and therefore the overall interpretation will not change at all. Skiba, P.: Homogene Schwerekarte der Bundesrepublik Deutschland (Bouguer-Anomalien), unpublished technical report, 89 p, https://doi.leibniz-liag.de/doi.php?obj=rep-12346-1, 2011.
	EC1	page 9: As height has a deceive influence on gravity data reduction, it will be good if the authors explain how height errors have propagated into the Bouguer anomaly so that it is possible to appreciate the interpretation.	Agreed	This question is difficult to answer, because we, the authors, did not acquire the data along the profile ourselves. However, from an unpublished report we can assume that position and heights were observed by DGPS observations (own base station) and an electronic tachymeter. Thus, the precision can be expected to be high. If we assume the height error to be 20 cm in the worst case, an error of 0.04 mGal would propagate into the Bouguer anomalies (Bouguer plate and free air reduction). We now address this problem of height errors propagating into the Bouguer anomalies in the chapter 'Data processing - gravimetry'.
	EC1	page 9: Which DEM is being used? As the different DEM models have different accuracies, it will be great if the DEM used is mentioned to appreciate the interpretations.	Agreed	For the processing of the gravity data we used the DGM-D with 25 meter cell size provided by the Bundesamt für Kartographie und Geodäsie (BKG, 2009). For areas outside Germany, the height model was complemented by SRTM data. Following the datasheet provided by BKG, the accuracy of the DEM is ± 1-4 m for the position and ± 1-5 m for the heights, the latter depending on the roughness of the topography; height resolution is 0.01 m. Heights refer to DHHN92. We added this information in the text.
	EC1	page 9: The authors have stated that they have used a filtering method in the gravity data processing without mentioning the type of filter. The type of filter and the parameters used have an impact on the result and it will be good if the authors specify the type of filter and the parameters set for filtering to appreciate the interpretations. What changes has the filtering process has brought.	Agreed	The requested information about the applied filter is already mentioned in the originally submitted manuscript version, but not in the chapter 'Data processing - Gravimetry', but in the chapter 'Interpretation gravimetry' (page 17 line 346 to page 18 line 355). We tested several spectral domain filters and finally we applied a so called 'tilt derivative' or 'tilt angle' filter in order to locate possible sources of gravity anomalies and to highlight fine changes in the gravity field. This filter process generated maxima centered above the source of the anomaly. Its zero crossing is located close to the edges of the source bodies. All amplitudes are restricted to values between + $\pi/2$ and - $\pi/2$ (+90° and -90°), thus suppressing strong amplitudes and amplifying weak amplitudes. However, we have realised that this information is incorrectly placed there and have now moved it to the chapter "Data processing - Gravimetry", as the reader will expect to find this information there. Furthermore, the text itself was corrected, because the tilt angle is not calculated from the gravitational potential, as erroneously written.

Chapter 5 - Results	RC1	page 9 lines 186: What were the criteria for the selection of this stretch? (comment in attachment)	Agreed	The SH-wave reflection seismic was carried out after the P-wave reflection seismic so the results of the P-wave section were taken into account during the planning of the SH-wave survey. And since the western part of the P-wave section P1 showed some interesting structures we decided to carry out the SH-wave profile S1 in this area. The same goes for the profiles P2 and S2. In the text, we added the information that the selection of the SH-wave profile location was based on the P-wave interpretation.
	RC1	page 10 lines 214 to 216: Authors are here describing their interpretation of a sinkhole identified by sinkhole profiles. In line 216, they state it is a collapse sinkhole. This is just an example to outline how misleading is the use of the term "subrosion" (used few lines before by the Authors) that, on the other hand, would let the reader think to a completely different mechanism of origin, that is dissolution or suffosion. I once more insist on not using such a misleading term.	Agreed	We changed the term 'subrosion' according to the reviewers suggestion to 'subsurface dissolution / solution' and we incorporated the suggested sinkhole classification into the interpretation.
	RC1	page 11 line 228: This was already pointed out in the geological description.	Not agreed	Yes that is true, but because the KSMF has a special meaning in the interpretation of this profile, in particular, we wanted to briefly mention the fault again.
	RC1	page 14 line 298: Are you able to hypothesize what type of sinkhole (accordingto the classification) is this?	Agreed	According to the sinkhole classification by Gutierrez et al., 2014 and Parise 2019, SF1 and SF2 are collapse sinkholes and SF3 and SF4 are sagging sinkholes. We incorporated this information in the text.
	EC1	page 19: In the model of the gravity data, there are small sharp edges, which I think are directly taken from the controlled source seismic profile. I am sure that this can't be detected and resolved by the surface gravity survey and it would have been good to present what is only detectable and resolvable by gravity method.	Agreed	Yes, it is true that some of these 'sharp edges' come from the interpretation of the seismic profiles. We already mention in the text that we used the seismic profile and also a crosssection through the geological map as structural constraints for the modelling. Although the directly measured Bouguer anomalies of the gravimetry (red curve in Fig. 9c) also shows some edges due to local minima and maxima partly induced by dissolution processes and the corresponding mass movement. Nevertheless, the seismic profile and the gravimetric profile, for example, have different resolutions. For example, the station spacing of the gravimetry naturally sets a limit on its spatial resolution. In the discussion on page 23, lines 514 to 517, we mention that the station spacing for the gravimetry profile in this study was 100 m, which was appropriate to detect mass movement on a larger scale (e.g. the near-surface collapse structures producing local minima had a lateral extent of at least ca. 100m), but for the detection of small-scale subrosion, we suggest to use a smaller station spacing (e.g. < 50 m). We have adjusted the corresponding passage in the discussion a little to make this point clearer.
Chapter 6 - Discussion	RC1	page 20 lines 411 to 412: This is also in accordance with sagging sinkhole models of the classification by Gutierrez et al (2014) and later updates	Agreed	We implemented this information in the text.
Figures & Tables	RC1	Comment on Figure 1: do we need so many different colors if you then summarize them in single formations?The map is quite complex and not easily readable, I suggest to simplify it in 6 colours (the 6 groups listed in the legend) to improve readability.	Agreed	We simplified the map according to the reviewer's suggestion.
	RC1	There is inconsistency among the initial figures as regards the formations shown. Figure 1 groups them in a way different from figure 2, and this makes difficult for the reader to understand the link among different figures and what is stated in the text. Author should decide which grouping is the best for their manuscript and adapt to that subdivision all the figures and the text.	Agreed	We changed the grouping of the geological formations in Fig. 1, so that it matches now the other illustrations.
References	RC1	please check the reference Schriel & Bulow (1926). It is exactly the same, and repeated as 1926a and 1926b.	Not agreed	The references are not the same, because the two references refer to two different geological map sheets, namely the map of Frankenhausen (1926a) and the map of Kelbra (1926b).
	RC1	I suggest to add the following references: **Abou Karaki N**., Fiaschi S., Paenen K., Al-Awabdeh M. and Closson D., 2019, Exposure of tourism development to salt karst hazards along the Jordanian Dead Sea shore. Hydrol. Earth Syst. Sci., 2323, 2111-2127. **Bruthans J.,** Asadi N., Filippi M., Vilhelm Z. & Zare M., 2008 - Erosion rates of salt diapirs surfaces: An important factor for development of morphology of salt diapirs and environmental consequences (Zagros Mts., SE Iran). Environmental Geology, 53 (5): 1091-1098. **Bruthans J.**, Filippi M., Zare M., Churàćìkovà Z., Asadi N., Fuchs M. & Adamovicì J., 2010 - Evolution of salt diapir and karst morphology during the last glacial cycle: effects of sea-level oscillation, diapir and regional uplift, and erosion (Persian Gulf, Iran). Geomorphology, 121: 291-304. **De Waele J.,** Piccini L., Columbu A., Madonia G., Vattano M., Calligaris C., D'Angeli I.M., Parise M., Chiesi M., Sivelli M., Vigna B., Zini L., Chiarini V., Sauro F., Drysdale R. and Forti P., 2017, Evaporite karst in Italy: a review. International Journal of Speleology, vol. 46 (2), p. 137-168. **Dreybrodt, W., 2004.** Dissolution: evaporite and carbonate rocks. In: Gunn, J. (Ed.), Encyclopedia of Caves and Karst Science. Fitzroy Dearborn, New York, pp. 295–300. **Fazio N.L.**, Perrotti M., Lollino P., Parise M., Vattano M., Madonia G., & Di Maggio C., 2017, A three-dimensional back analysis of the collapse of an underground cavity in soft rocks. Engineering Geology, vol. 238, p. 301-311. **Filippi M.**, Bruthans J., Palatinus L., Zare M. and Asadi N. 2011. Secondary halite deposits in the Iranian salt karst: general description and origin. International Journal of Speleology, 40 (2), 141-162. **Goldscheider N.** & Bechtel T.D., 2009, The housing crisis from underground—damage to a historic town by geothermal drillings through anhydrite, Staufen, Germany. Hydrogeology Journal, vol.17, p. 491-493. **Iovine G.,** Parise M. & Trocino A., 2010, Breakdown mechanisms in gypsum caves of southern Italy, and the related effects at the surface. Zeitschrift fur Geomorphologie, vol. 54 (suppl. 2), p. 153-178. **KAUFMANN, G**. 2014. Geophysical mapping of solution and collapse sinkholes. Journal of Applied Geophysics, 111, 271–288. **KAUFMANN, G**. & ROMANOV, D. 2016. Structure and evolution of collapse sinkholes: combined interpretation from physico-chemical modelling and geophysical field work. Journal of Hydrology, 540, 688–698. **KAUFMANN, G.**, NIELBOCK, R. & ROMANOV, D. 2015b. The Unicorn Cave, Southern Harz Mountains, Germany: from known passages to unknown extensions with the help of geophysical surveys. Journal of Applied Geophysics, 123, 123–140. **Kaufmann, G.**, Romanov, D., Tippelt, T., Vienken, T., Werban, U., Dietrich, P., Mai, F., Börner, F., 2018. Mapping and modelling of collapse sinkholes in soluble rock: The Muìnsterdorf site, northern Germany. Journal of Applied Geophysics 154, 64–80. **KAUFMANN, G.** & ROMANOV, D, 2018, Geophysical observations and structural models of two shallow caves in gypsum/anhydrite-bearing rocks in Germany. In: Parise M., Gabrovsek F., Kaufmann G. & Ravbar N. (Eds.), Advances in Karst Research: Theory, Fieldwork and Applications. Geological Society, London, Special Publications, 466, p. 341-357. **Margiotta S**., Negri S., Parise M. & Valloni R., 2012, Mapping the susceptibility to sinkholes in coastal areas, based on stratigraphy, geomorphology and geophysics. Natural Hazards, vol. 62 (2), p. 657-676, DOI 10.1007/s11069-012-0100-1. **Margiotta S.,** Negri S., Parise M. & Quarta T.A.M., 2016, Karst geosites at risk of collapse: the sinkholes at Nociglia (Apulia, SE Italy). Environ. Earth Sciences, vol. 75 (1), p. 1-10, DOI: 10.1007/s12665-015-4848-y. **Parise M.,** Closson D., Gutierrez F. & Stevanovic Z., 2015, Anticipating and managing engineering problems in the complex karst environment. Environmental Earth Sciences, vol. 74, p. 7823-7835. **Perrotti M.**, Lollino P., Fazio N.L. & Parise M., 2019, Stability charts based on the finite element method for underground cavities in soft carbonate rocks: validation through casestudy applications. Natural Hazards and Earth System Sciences, vol. 19, p. 2079-2095. **Watson R.A**., Holohan E.P., Al-Halbouni D., Saberi L., Sawarieh A., Closson D., Alrshdan H., Abou Karaki N., Siebert C., Walter T.R. and Dahm T., 2019, Sinkholes and uvalas in evaporite karst: spatio-temporal development with links to base-level fall on the eastern shore of the Dead Sea. SE, 10, 1451-1468. **White, W.B.**, 2002. Karst hydrology: recent developments and open questions. Eng. Geol. 65, 85–105. **Zumpano V.**, Pisano L. & Parise M., 2019, An integrated framework to identify and analyze karst sinkholes. Geomorphology, vol. 332, p. 213-225.	Agreed	According to the reviewers suggestion we added some of the suggested references.
	RC2	Use of semicolon between references looks a bit odd as it breaks the sentence.	Not agreed	The semicolon between the references in parentheses is generated by the latex template of the journal. It is therefore journal-standard and cannot / should not be changed.