Pipes to Earth's subsurface: The role of atmospheric conditions in controlling air transport through boreholes and shafts

Letter of response:

Reviewer 1:

Levintal and colleagues have measured the state and composition of air in two boreholes (one connected at times to a large, underground cavity) over some brief periods and tried to generalize about transport processes through these "pipes". Such a study could be a welcome addition to the literature regarding the dynamics of vadoze-zone air and its exchange with the atmosphere, about which little is still known. However, the paper suffers from some significant shortcomings, both technical and in terms of perspective, that should be improved before the paper can be acceptable for publication. Therefore, I recommend major revision along the following lines.

We would like to thank Prof. Kowalski for his comments and fruitful review. The manuscript was significantly changed. The main changes were: expansion of the dataset of the large-diameter borehole for the spring season; addition of a sensitivity analysis for the use of absolute humidity as a tracer for airflow direction according to half a year of measurements; and changes to Figs. 2, 6, and 7 according to the virtual temperature analysis that was suggested in this review. A detailed response for each comment is presented below in blue font.

General comments

Technical comment on air density

A major theme is thermal-induced convection (TIC), which the authors attribute to "unstable density gradients resulting from temperature differences". This is not quite correct. At a given height/pressure, air density depends predominantly on temperature but also on air composition. This can be important particularly when dealing with such a humid cavity. As a simple example with data taken from inspecting Fig. 2, atmospheric air at 25_C with 50% relative humidity and 400 ppm of CO2 is warmer but denser than saturated cavity air at 24_C with 2000 ppm of CO2. The authors cite Sánchez-Cañete et al. (2013) but apparently without having appreciated the message of that paper. Rather than the temperature, it is the virtual temperature (easily calculated using an excel spreadsheet offered by Sánchez-Cañete and colleagues, and that can be downloaded from http://fisicaaplicada.ugr.es/pages/tv/!/download) whose gradient should be examined to diagnose convective instability. Note that the virtual temperature depends predominantly on the temperature, secondarily on humidity, and tertiarily on the CO2 concentration. The CO2 dependence is only of relevance for the high CO2 concentrations found in some underground environments.

We fully agree with this comment and after re-reading the manuscript by Sánchez-Cañete et al. (2013), we revised our MS accordingly:

(1) A summary of the virtual temperature concept (*Tv*) was added to the introduction: "Although air density depends mainly on temperature, it is also depend on the air humidity, and to a lesser degree on the air's gas composition (Kowalski and Sánchez-Cañete, 2010). Integration of these three effects (temperature, relative humidity, and air composition) into a single parameter named virtual temperature (*Tv*) was proposed by Sánchez-Cañete et al. (2013)…. For a given altitude level, the *Tv* differences will determine the onset of TIC." (Page 2, lines 20-27).

(2) In Figures 2, 6, and 7, the data are now presented as a function of *Tv* rather than T, according to the excel spreadsheet offered by Sánchez-Cañete et al. (2013).

We note that due to the fact that we didn't have continuous CO_2 measurements in this study, the *Tv* within the boreholes was calculated using the T and RH continuous measurements with a constant CO_2 concentration according to preliminary results that we took over two weeks during the 2017 winter season (~2000 ppm). Obviously, the CO_2 concentration varies throughout the day within the boreholes; however, we think that this is a reasonable choice because we know from these two weeks of measurements that the CO_2 inside the borehole did not increase above 2000 ppm, and the values are relatively low compared to other underground cavities such as caves (as also mentioned here in the reviewer's next comment). In addition, as emphasized by the reviewer in this comment, the CO_2 changes are only the third parameter in importance after temperature and RH for air density convection, and therefore we believe this to be a reasonable assumption.

Perspective on CO2 concentrations

The abstract cites "High CO2 concentrations (2000ppm)". In underground environments, CO2 concentrations of over 100,000 ppm have been measured (Amundson and Davidson, 1990, J Geochem Explor, 38, 13-41), and values exceeding 10,000 ppm are not uncommon (Denis et al., 2005, Geophys Res Lett, 32, L05810, doi: 10.1029/2004GL022226; Benavente et al., 2010, Vadose Zone J. 9:126–136; Sánchez-Cañete et al., 2010, Geophys Res Lett, 38, L09802, doi:10.1029/2011GL047077). In this context, 2000 ppm is not high, and might even qualify as particularly low. Unlike that found in many caves, it is even too low to affect air density (but note that the water vapour content of the cavity certainly does, which is why the virtual temperature is needed here).

The word "high" was deleted from the last line in the abstract. We also added three of the above references with a suitable clarification that "In environments of high CO_2 concentrations compared to the atmosphere, the importance of the gas composition on the *Tv* becomes more pronounced. Such underground environments can be karstic areas of carbonate rocks (Sanchez-Cañete et al., 2011), caves (Denis et al., 2005; Guillon et al., 2015), and soils (Amundson and Davidson, 1990) where CO_2 concentrations can be very high, ranging from 10,000 to 100,000 ppm and above." (Page 2, lines 23-27).

Perspective on seasonal variability of atmospheric conditions

The manuscript presents borehole/shaft observations under atmospheric conditions that (1) are limited to a few weeks for each geometry and (2) vary seasonally among geometries, but the analysis fails to take into account these data limitations when generalizing about findings.

As we did measurements in the large-diameter boreholes for a longer time, we could change its time-series from February 2017 to April 2017 such that the analysis of both boreholes would be in the same season (i.e., spring). We note that there are still some differences in the atmospheric conditions between the boreholes, this is because (1) the small diameter borehole was studied in 2016, whereas the large-diameter borehole was studied in 2017, and (2) the two boreholes are not exactly at the same geographical location. For example, the average atmospheric temperature and RH during April 2016 were 20.2° C and 69.1%, respectively, (at the small diameter borehole location) compared to the average values during April 2017 of 18.2° C and 50.4%, respectively, (at the large-diameter borehole location).

Unfortunately we don't have complete one-year results of the boreholes due to technical restrictions, and we agree that this is a limitation of our study. We added this limitation in the conclusions section: "…This mechanistic explanation was validated using the winter and spring season's dataset. Although we show that theoretically the transport mechanism observed for winter and spring should hold, with reduced significance for summer and autumn, further data are needed to verify the theoretical calculation." (Page 11, lines 26-28).

For example, the abstract claims that "absolute humidity was found to be a reliable proxy for distinguishing between atmospheric and cavity air masses and thus to explore air transport through the three geometries". This may be so for the spring (borehole, shaft) and mid-winter (large-D borehole) data in the paper, when atmospheric absolute humidity is considerably below that of the underground environment. However, in summer it is likely that atmospheric values of absolute humidity could equal, surpass, or oscillate about the subterranean value, and this proxy variable could well lose all of its utility. The same is true about the ability to determine shaft airflow directions using temperature sensors (p5, line 28).

Following this comment, we broadened the absolute humidity dataset according to our available measurements and performed a sensitivity analysis of the changes in absolute humidity for the *large-diameter borehole* between 02/2017 and 08/2017 (see Fig. S2 and S3 in the Supporting information – also attached here below). A summary of the sensitivity analysis was added to the article to provide the limitation of using absolute humidity as a proxy for airflow in these types of boreholes:

(1) In the abstract we added: “Absolute humidity was found to be a reliable proxy for distinguishing between atmospheric and cavity air masses (mainly during winter and spring seasons), and thus to explore air transport through the three geometries.” (Page 1, lines 14-15).

(2) In the discussion we added: “The use of AH as a proxy for airflow direction is suitable mainly for winter and spring seasons when the atmospheric AH is lower compared to AH within underground cavities (Figs. S2 and S3– supporting information). During the summer season, there are periods in which atmosphere and cavity AH are in equilibrium, and thus the use of AH as a proxy for airflow directions would not be reliable (see supporting information for AH sensitivity analysis).” (Page 5, lines 12-16).

(3) In the conclusion we added: “Use of AH changes during the winter and spring seasons was shown as a practical tool to identify the source of air parcels within the three geometries, namely atmospheric vs. lower-borehole/cavity, and thus to determine the direction and effect of the air transport.” (Page 11, lines 15-17).

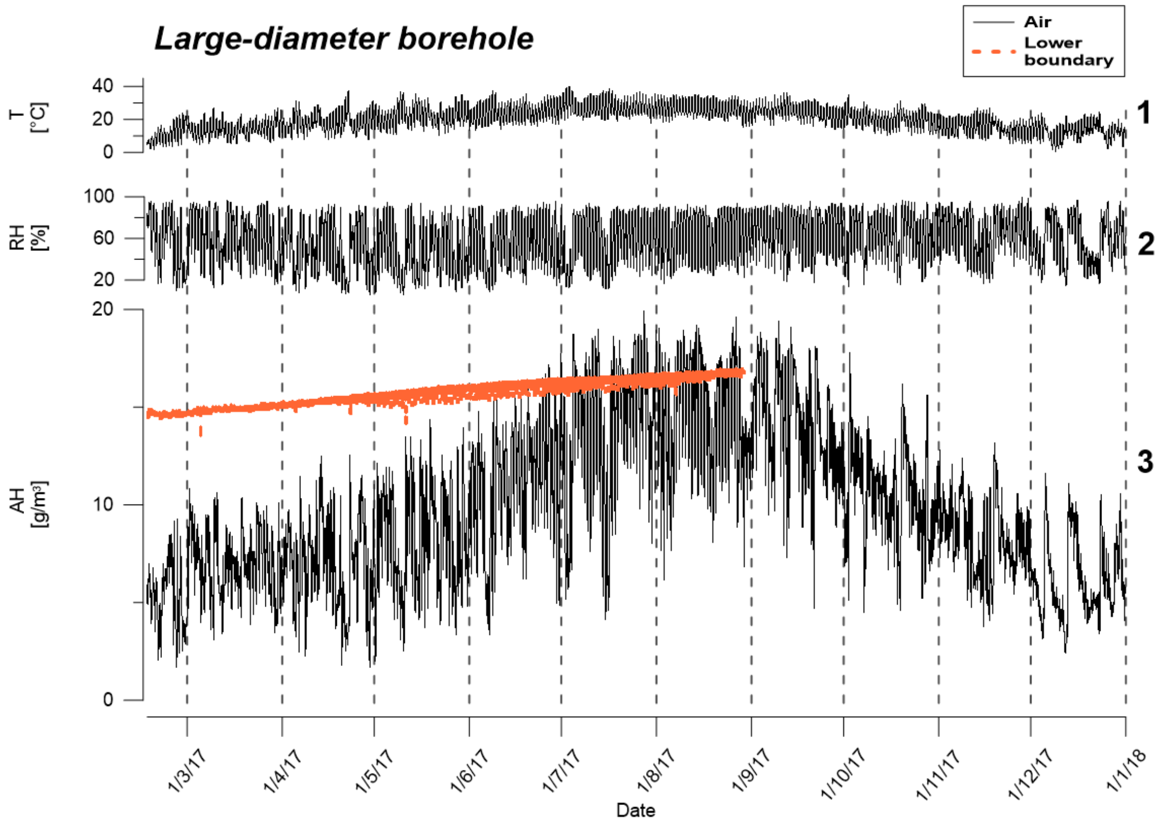

Figure. S2. Seasonal time series results of absolute humidity (AH) from the *large-diameter borehole*. From mid-February to the end of June, the use of AH as a proxy for airflow direction was valid due to the differences of AH between the borehole's lower boundary and the atmosphere. Between July and August (summer season), the AH differences were no longer constant (see also Fig. S3), and therefore we can conclude that AH was no longer a suitable proxy for the identification of airflow directions within the borehole. AH data for the borehole are missing from September until December; however, it is reasonable to assume that during that period, the AH as a proxy was also relevant because AH within the borehole was stable.

In Section 3.4, the authors contrast *winter* air movement detected in the large-D borehole with those noted in the shaft/borehole during *springtime*, when the external air has likely become far less dense. Convective instability for these "pipes" must generally be far greater for either geometry in February than in May. Nonetheless, the ensuing analysis suggests that dT/dz can be considered constant. There is no justification for this spurious assumption, particularly given that the authors have data that enable its direct assessment. I have little doubt regarding the authors' conclusion that convection is most important for the large-D borehole, whereas barometric changes dominate for the other geometries, but this needs to be demonstrated climatologically and without making invalid assumptions.

The purpose of this section was to demonstrate an ideal comparison in order to show the transition between governing air transport mechanisms with the increase of diameter (from BP to TIC). Following this comment and also the comment from the second reviewer regarding this analysis, we agree that indeed our comparison and assumptions were over- simplified. Therefore, we deleted the comparison paragraph that contained the potentially problematic assumptions. We left only the theoretical equations that can be used for a general discussion regarding the impact of the radius on both mechanisms (i.e., BP and TIC).

The paper would be greatly strengthened by broadening the dataset to include a seasonal range of temperatures/humidities for every geometry. Otherwise, the generalisations inferred by the authors should be reduced in scope to take into account their seasonal representativeness.

We agree. Unfortunately, due to technical restrictions and limitations, we could not obtain long-term measurements in some of our field sites. We broadened our *large-diameter borehole* dataset by a few months according to our available data. Because it is still not a full seasonal rang, we reduced our conclusion to a seasonal aspect as mentioned above in our answer to a previous comment: "…This mechanistic explanation was validated using the winter and spring season's dataset. Although we show that theoretically the transport mechanism observed for winter and spring should hold, with reduced significance, for summer and autumn, further data are needed to verify the theoretical calculation" (Page 11, lines 26-28).

Specific Comments

In addition to the General comments above, which require significant revision to the analysis and presentation of the findings, I offer the following specific suggestions (by page/line) to improve the manuscript:

2/16 Do not neglect differences in air composition.

We added to the introduction the importance of RH and air composition to the overall density, including suitable references: "Although air density depends mainly on temperature, it is also depend on the air humidity, and to a lesser degree on the air's gas composition (Kowalski and Sánchez-Cañete, 2010). Integration of these three effects (temperature, relative humidity, and air composition) into a single parameter named virtual temperature (*Tv*) was proposed by Sánchez-Cañete et al. (2013). In environments of high CO_2 concentrations compared to the atmosphere, the importance of the gas composition on the *Tv* becomes more pronounced. Such underground environments can be karstic areas of carbonate rocks (Sanchez-Cañete et al., 2011), caves (Denis et al., 2005; Guillon et al., 2015), and soils (Amundson and Davidson, 1990) where CO_2 concentrations can be very high, ranging from 10,000 to 100,000 ppm and above. For a given altitude level, the *Tv* differences will determine the onset of TIC." (Page 2, lines 20-27).

3/11 Homogenize the font size "first 42 days"

Done.

3/13 That "the pipe touched the cavity floor" does not ensure a hermetic seal. Either the cavity floor geometry should be described (horizontal, smooth?), or the isolation of the pipe from the cavity should be somehow justified.

We validated this assumption by visual evidence from a camera that was lowered to the bottom part of the pipe – the pipe–soil interface. This is described in the materials and methods section in the following sentence: “In addition, a televiewer was lowered into the pipe to verify that the pipe was intact and was either connected to or disconnected from the underground cavity in the shaft or borehole, respectively.” (Page 4, lines 5-7).

4/1 In equations, it is preferable that each variable be represented by a single symbol, with necessary subscripts. A reader might interpret "AH" in equations (1) and also in the derivative at page 5, line 5 (and elsewhere) as the product of the variables "A" (volume flow rate per unit length) and H. Rather than "AH", I recommend the use of the standard rho (greek symbol for "r") with a subscript (v) to indicate that it is the vapour density that is synonymous with the absolute humidity. Similarly, relative humidity could be represented with "U" instead of "RH".

Done, AH in the equations was changed to ρ_v, *dAH/dt* in the text was changed to $d\rho_v/dt$, and RH in the equations was changed to *U*.

5/16-19 The "typical" situation described is valid only for nighttime data (and is furthermore dependent upon season). Thus, a "decrease in temperature in the shaft observed by the temperature sensors" would not typically describe penetration during a daytime barometric increase since the external air temperature would exceed that of the cavity.

5/28-30 The ability to estimate the direction of the airflow in the shaft is not general. For example, a pressure change that occurred at a moment when the external air temperature coincided with the cavity temperature (as occurs twice per day in Fig. 2, and perhaps during many hours on summer nights) would not be detected.

Following the two comments above, we changed and combined these two paragraphs to a single paragraph. In the new paragraph, we added the above clarification that: “Stages 2 and 4 in Fig. 4 are valid mainly during winter and spring night-times when atmospheric temperatures are lower than within the borehole”. (Page 6, lines 7-9). The lines describing the use of temperature sensors for airflow direction analysis were deleted.

6/24 Homogenize header format.

Done.

7/29 These cases that occur only at noontime are specific to winter, when the air temperature rarely exceeds the cavity temperature. In summer, it would be most often the case.

The sentence was changed to a more general definition which relates the air transport mechanism to thermal-stability and not to a certain time of the day: "The cases of AH increases at 10 m occurred when thermal-stability controlled the air movement inside the *large-diameter borehole*, and only then did AH at 10 m increase to the saturation conditions that characterize the lower boundary (marked as gray columns in Fig. 7a)" (Page 8, lines 7-9).

8/6-7 The validity of this hypothesis is very dependent on season. "Given the same climate conditions..." I agree, but the paper does not examine these geometries under the same climate conditions.

9/13 The assumption that dT/dz is constant makes no sense. Although BP and TIC are compared using the same climate conditions, TIC depends on dT/dz whereas BP does not. Increased values of dT/dz in winter make for greater convective instability, whatever the borehold diameter. But the geometries are not examined under the same climate conditions, since the shaft and borehole are examined in spring, whereas the large-D borehole is examined in winter.

We agree. These lines were changed as mentioned in the response to the major comments above.

8/11-etc. Much of the following 1.5 pages constitutes methodology, and would better be provided in section 2.

The equations presented in these pages are the basis for the discussion that follows them and therefore we think it is more suitable to present them in the scope of the discussion rather than in the methods section.

10/3 The conceptual model is a very good idea.

Thanks.

10/23-27 This paragraph regarding CO2 source/sink behavior is particularly weak, and I recommend deleting it entirely. To speak of sources/sink requires information regarding the origin/destiny of the CO2 in the cavity. In any event, I think it is absurd to state that the geometry is a source or sink. It might better be described as a storage space for CO2 that could otherwise have been emitted to the atmosphere (but from what source?). To appreciate the complexities of the source/sink issue, the authors are encouraged to consult section 3.1 of Serrano-Ortiz et al., 2010, Agric. and Forest Meteorol. 150, 321–329.

After reading the above reference, we realize that this paragraph is indeed too simplified, and we decided to delete it and change section 3.5 accordingly. We also added the above reference to direct the reader to the optional CO_2 sources in underground cavities (Page 10, Lines 28-29).

11/12 This "conclusion" is very much specific to site and season, and must be qualified as such or revised.

A clarification was added “Use of AH changes during the winter and spring seasons was shown as a practical tool to identify the source of air parcels within the three geometries, namely atmospheric vs. lower-borehole/cavity, and thus to determine the direction and effect of the air transport” (Page 11, Lines 15-17).