Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2018-18-RC1, 2018

Earth System Dynamics Discussions

Interactive comment on "Pipes to Earth's subsurface: The role of atmospheric conditions in controlling air transport through boreholes and shafts" *by* Elad Levintal et al.

A. Kowalski (Referee)

andyk@ugr.es

Received and published: 4 June 2018

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 generalise 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.

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.

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).

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.

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).

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 paper would be greatly strengthened by broadening the dataset to include a seasonal range of temperatures/humidities for every geometry. Otherwise, the generali-

sations inferred by the authors should be reduced in scope to take into account their seasonal representativeness.

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.

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

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.

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".

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 suensors" 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.

6/24 Homogenize header format.

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.

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.

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

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.

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

10/23-27 This paragraph regarding CO2 source/sink behaviour 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.

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

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2018-18, 2018.