

This manuscript presents a valuable one-year, minute-resolution dataset of CO₂ and O₂ concentrations from two infiltration wells at the same site. The observations are potentially useful for understanding gas transport in open wells. However, several central conclusions currently exceed what can be supported by the experimental design and analyses. The manuscript therefore requires substantial methodological clarification and revision of its causal claims.

We thank the reviewer for the constructive comments, which we believe will help us significantly improve it. Detailed answers are provided below in blue font. The main revisions we propose in response to the three reviewers' comments include the following:

- (1) A new quantitative analysis was added to include the contribution of diffusion.
- (2) New analyses were added with further discussion indicating why wind-induced convection and thermal-induced convection were less significant.
- (3) Eq. 1 was corrected. Units and calculations were double-checked and corrected as detailed below.
- (4) A new paragraph discussing potential sources of CO₂.

Major comments

1. The manuscript attributes the differences between the wells primarily to dry versus wet well type. However, the wells also differ in total depth, screened interval, sensor spacing, gas-column length, saturation conditions, and operational history. In addition, only the wet well received controlled infiltration and several months of operational recharge. Consequently, the effects of saturation status cannot be separated from those of well geometry, depth, screened interval, and recharge disturbance. The present study compares two specific wells rather than providing a replicated comparison of dry and wet well types. Statements such as “well type strongly controls” should therefore be substantially weakened, and this limitation should be clearly acknowledged throughout the manuscript.

We thank the reviewer for this important emphasis. We consider this experiment as the closest possible comparison between dry and wet well types. Any other comparison will also include other inherent limitations. For example, two wells of similar depth, one dry and the other wet, indicate different locations, which in turn correspond to different climates or geological formations. This is why we treated this as a comparison study. Nevertheless, we fully agree with the reviewer that this is not a replicated comparison and there are limitations as noted in the comment. Therefore, we decided to state it clearly, acknowledge it when needed, and include it in the limitations. Here are only a few examples of the more important changes we propose:

- The word “comparative” was deleted from the title, which was changed from “Barometric Pumping as a Driver of Subsurface CO₂ Emissions: A Comparative Study of Vadose Zone vs. Groundwater Wells” to “Barometric Pumping as a Driver of Subsurface CO₂ Emissions: A Study of Vadose Zone vs. Groundwater Wells
- The last sentence in the abstract was changed from “These results demonstrate that well type strongly controls air transport mechanisms and associated CO₂ emissions” to “These results demonstrate the air transport mechanisms and associated CO₂ emissions for each well type”.

- “This unique comparative approach allows direct in-situ evaluation of the controlling air transport mechanisms, such as BP and TIC, as a function of well type.” was changed to “This unique approach allows direct in-situ evaluation of the controlling air transport mechanisms, such as BP and TIC, of both well types”
 - We deleted the phrases indicating that the wells have similar soil conditions (e.g., L.364, L.296)
 - In the conclusions section, the sentence “This study presents a novel comparison of air transport mechanisms in two types of infiltration wells...” was changed to “This study presents a novel investigation of air transport mechanisms in two types of infiltration wells...”
2. The manuscript explicitly states that air velocity and CO₂ flux were calculated only for the dry well. The decision not to calculate wet-well flux was based on low CO₂ concentrations near the wellhead and the inferred absence of strong advective transport. However, low concentration does not demonstrate low total flux. The wet well may still exchange CO₂ through diffusion, weak or intermittent convection, groundwater degassing, or recharge-induced gas displacement. Therefore, the manuscript does not currently support the conclusion that CO₂ emissions from the dry well were greater than those from the wet well. Unless wet-well flux is independently quantified, the authors should limit their conclusion to stating that periodic advective transport was quantified in the dry well, whereas no comparable periodic advective signal was detected in the wet well. Comparative emission claims in the title, abstract, discussion, and conclusions should be revised accordingly.

Following this important comment, we added a new analysis in which we focus on diffusion, thermal-induced convection (TIC), and wind-induced convection, as these are the main potential mechanisms, in addition to barometric pumping.

Diffusion – we conducted an additional calculation to evaluate the potential diffusive CO₂ flux. The calculation is based on the set of equations below. Even in the case of a relatively high CO₂ concentration in the lower part of our wells of 50,000 ppm, the calculation shows that the diffusive CO₂ flux is relatively low, and 3-4 orders of magnitude lower than that calculated using the advection component (Eq. 2 in the manuscript). Therefore, we conclude that although diffusive flux is occurring at all times, it can be neglected compared to the advective one in the dry well. However, it is relevant to the wet well, as no barometric pumping occurs there.

According to Fick's first law of diffusion:

$$Q = -DA * \frac{dc}{dx} \quad (\text{Eq. 1})$$

where Q is the CO₂ flux (mol/min), D is the diffusivity of CO₂ in air (m²/min), A is the surface area of the well (m²), dc is the concentration difference between the bottom of the well and the atmosphere (mol/m³), and dx is the depth of the air in the well (m).

For example, for a maximum concentration gradient, we use 420 ppm at the atmosphere above and 50,000 ppm at the lower well boundary (i.e., $C_1 = 50,000$ ppm $C_2 = 420$ ppm). Then, we convert ppm to mol:

$$C_{\frac{mol}{m^3}} = C_{ppm} * 10^{-6} * \frac{P}{RT} \quad (\text{Eq. 2})$$

$$(P = 101325 \text{ Pa}, \quad T = 293.15 \text{ K}, \quad R = 8.314 \frac{J}{mol \cdot K}, \quad \text{mw}(\text{CO}_2) = 44.01 \text{ gr/mol})$$

Therefore,

$$C_1 = 50000 * 10^{-6} * \frac{101325 \text{ Pa}}{8.314 \frac{J}{mol \cdot K} * 293.15 \text{ K}} = 2.044 \text{ mol/m}^3$$

$$C_2 = 420 * 10^{-6} * \frac{101325 \text{ Pa}}{8.314 \frac{Pa \cdot m^3}{mol \cdot K} * 293.15 \text{ K}} = 0.0175 \text{ mol/m}^3$$

$$\Delta C = 2.044 - 0.0175 = 2.0265 \frac{mol}{m^3},$$

Implementing this ΔC with the Δx of each well (40 m for the wet well and 27 m for the dry well) in Eq. 1, we can estimate the diffusive flux; assuming constant conditions of

$$D_{CO_2}(20^\circ \text{C}) = 9.4 * 10^{-4} \frac{m^2}{min}, \quad A = \pi r^2 = \pi * \left(\frac{0.203m}{2}\right)^2 = 0.032365 \text{ m}^2:$$

$$Q_{wet} = 9.4 * 10^{-4} \frac{m^2}{min} * 0.032365 \text{ m}^2 * \frac{2.0265 \frac{mol}{m^3}}{40 \text{ m}} = 1.5 * 10^{-6} \frac{mol}{min} = 6.8 * 10^{-5} \frac{gr}{min}$$

$$Q_{dry} = 9.4 * 10^{-4} \frac{m^2}{min} * 0.032365 \text{ m}^2 * \frac{2.0265 \frac{mol}{m^3}}{27 \text{ m}} = 2.3 * 10^{-6} \frac{mol}{min} = 1.0 * 10^{-4} \frac{gr}{min}$$

The CO_2 flux calculated from the advective velocity is in the order of ~ 0.1 gr/min (**Fig. 5b** in the manuscript). Therefore, the conclusion is that during advective transport:

$$Q_{diffusian} \ll Q_{advection}$$

Wind-induced convection – here, we conducted an additional FFT analysis comparing wind-speed frequency with CO_2 and O_2 within the dry well (Figure 6b in the manuscript). The results showed that the wind speed had a maximum frequency of ~ 1 cycle/day (as expected), which cannot explain the CO_2 and O_2 dynamics within the well of a maximum of ~ 2 cycles/day. This

relatively low effect of wind-induced transport within wells at depths greater than a few meters is also supported by the literature (Levintal et al., 2020).

Thermal-induced convection – as far as we know, there is no validated model to calculate the air velocity within the wells/borehole associated with TIC. Therefore, we adopt a different approach to evaluate TIC's contribution to the total measured advective air velocity. To do so, we compared the measured advective air velocity in the dry well (Section 2.4.1 in the manuscript) between summer (i.e., $\Delta T_V < 0 \rightarrow$ thermal stability within the well $\rightarrow$ no TIC) and winter (i.e., $\Delta T_V > 0 \rightarrow$ thermal instability within the well $\rightarrow$ conditions for TIC onset). The velocity histogram for summer wasn't significantly different than winter; therefore, we concluded that TIC was not a significant source for air velocities in the dry well. However, in the wet well, as barometric pumping is suppressed, even a small additional contribution from TIC can be significant, as also stated in the comment above. This is also the case for diffusion. We also acknowledge that additional investigation of TIC air velocities is needed using new measurements or modeling tools.

We proposed to:

- (1) Incorporate in the discussion all of the above explanations and calculations for diffusion, wind-induced convection, and TIC using a simple multi-component transport model for each well
 - (2) Emphasize that periodic advective transport was quantified in the dry well, whereas no comparable periodic advective signal was detected in the wet well. This will be done in the abstract by adding:
 “The two-cycles-per-day advective transport was quantified in the dry well with an average air velocity of 0.246 m/min (during outflow events), whereas no comparable periodic advective signal was detected in the wet well”
 - (3) Finally, as noted in the previous comment, we also removed the comparison terminology from various locations in the manuscript, including the title, abstract, and conclusions.
3. Equation (1) appears dimensionally and numerically inconsistent. The standard conversion from ppm to mole fraction is not represented correctly, and the unit-conversion factor shown in the equation is unclear. The magnitude shown in Figure 5 may indicate that the code used the correct conversion and that the problem is limited to the printed equation. Nevertheless, this must be explicitly demonstrated.

We thank the reviewer for pointing out this mistake, and we apologize for it. We replaced Eq.1 with the new form shown below. We also validated the flux calculations again; indeed, the results in Fig. 5 are the same, and the problem was in the printed version, not in the code used to calculate the flux.

$$[CO_2]_m \left[\frac{gr}{m^3} \right] = \frac{[CO_2]_{ppm} \times 10^{-6} * M_{CO_2} \left[\frac{gr}{mol} \right] \times P[Pa]}{R \left[\frac{Pa * m^3}{K * mol} \right] \times T[K]}$$

We note that the units in the equation are shown here for clarity, but in the revised version, the parameters with their units will be listed below the equation, following the standard writing format.

4. The dry-well velocity is reported as approximately 0.2–0.3 m/min, but the comparison with the previous study is expressed in m/s. This represents a factor-of-60 discrepancy and directly affects the interpretation of why the measured flux was lower than previously reported values. The authors should verify the units used in the text, Figure 5, Equation (2), the Python code, and the cited study. All velocity and time units used in the flux calculation must be consistent.

We apologize for the mistake; the numbers are correct, but the units in line 304 should be m/min, consistent with the manuscript and the previously cited study (Fig. 7b in Levintal et al., 2020b). We corrected the units and double checked that all units are in m/min.

Minor comments

1. Explain why the selected five-day period is considered representative and provide objective annual statistics for comparison.

We added the following explanation:

“In order to better discern trends, five representative consecutive days during June 2025 were selected; the selected time series was defined as representative because no major atmospheric changes were observed during these five days”

We also provide the full-year data for the wet well (Fig. 7) and for the dry well (Fig. S2 in the supplementary information). In addition, the full analyses are conducted on the entire time series and not on the 5 days (e.g., Fig. 5, Fig. 6b)

2. The distance between the two wells is reported as approximately 5 m in one section and approximately 7 m in another.

The distance is 5 m and was also correct, where it was written ~7 m.

3. Use “semi-diurnal” consistently when referring to two cycles per day.

We correct the terminology, so the full term throughout the manuscript is consistent: “semi-diurnal oscillations of two cycles per day”

4. The in-text citation for Forde et al. gives 2017, whereas the reference list gives 2019.

We corrected the in-text citation to 2019.

5. Correct “Perriaer” to “Perrier.”

Done.

6. Correct the incomplete NGWA citation.

Done.

7. Correct capitalization and spacing inconsistencies in subsection headings.

Done.

8. Distinguish the two authors with the initials “EL” in the author-contribution statement.

Done.

9. Report saturated measurements as at least 40,000 ppm.

Done. In the results section, we now state that: “CO₂ concentrations at both the top (-1 m) and bottom (-20 m) nodes ranged from atmospheric background levels (~420 ppm) up to at least 40000 ppm, the saturation limit of the sensor.”

10. Revise the Figure 5 caption to specify the time and velocity units, treatment of atmospheric background, treatment of inflow, and handling of missing periods.

Done.