

This study investigates seasonal dynamics of gas transport and exchange between subsurface wells and the atmosphere. The research was conducted at a field site where different types of wells (i.e., dry and wet wells) are used for managed aquifer recharge. The study compares gas transport in a dry well that extends through the unsaturated zone with a wet well that penetrates below the groundwater table. The aim was to evaluate the role of barometric pumping and thermal convection in driving subsurface CO₂ emissions in the dry and wet wells. The study provides high-resolution sensor-based measurements of gaseous O₂ and CO₂ for temporal dynamics, along with in situ depth-specific monthly CO₂ measurements. The authors concluded that CO₂ fluxes are controlled by barometric pressure in dry wells, whereas the contribution of this mechanism to CO₂ exchange in wet wells that are below groundwater is minimal. Instead, the CO₂ fluxes in such wells are controlled by thermal dynamics. I found this study interesting and relevant, and I believe the dataset is valuable, but I cannot recommend the current version of the manuscript for publication in HESS. I believe the analysis needs to be considerably extended and become more quantitative, perhaps by including a process-based model to test the hypothesis presented in the paper.

We thank the reviewer for finding this manuscript interesting and relevant and 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.

In the following, I list the major points that the authors need to carefully address:

- In the dry well, the authors attribute the dynamics of the O₂ and CO₂ exchange to advective transport that is induced by the barometric pressure fluctuations, which is a well-known and well-studied phenomenon. In addition to this mechanism, I think diffusion of these gases in the free air phase is a relevant process at the time scale of this study. However, the author did not discuss or include it in their analysis and found barometric pressure as the main driving force. The authors indeed found a direct relationship between pressure gradients and changes in CO₂ concentrations (which is well expected), but this does not rule out the importance of other processes. Additionally, since the wells are open to the atmosphere, wind flow can also intensify gradients and mixing and influence the magnitude of fluxes through advection and diffusion. I guess it may be difficult to distinguish the relative contribution of these mechanisms by just looking at the data, and that is why I think a simple multi-component transport model can help to disentangle these contributions quantitatively, and strengthen the arguments and conceptual model presented in this study.

Following this comment, we quantitatively evaluated the additional mechanisms in order to provide an estimation of their relative importance. We focus on diffusion, thermal-induced convection (TIC), and wind-induced convection as these are the main potential mechanisms, in addition to barometric pumping (as also stated by the reviewer).

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 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., C₁ = 50,000 ppm C₂ = 420 ppm). Then, we convert ppm to mol:

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

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

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

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

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

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

The CO₂ 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 CO₂ and O₂ 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 CO₂ and O₂ 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 wells/boreholes 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 (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. 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 incorporate all of the above explanations and calculations for diffusion, wind-induced convection, and TIC into a simple multi-component transport model in the discussion, as suggested by the reviewer.

- Additionally, the manuscript focuses on the mechanisms governing O₂ and CO₂ exchange between the subsurface and the atmosphere, but provides limited discussion of the sources and sinks of these gases (i.e., which biogeochemical processes produce CO₂ and consume O₂ at the study site, and how these processes vary spatially and temporally). For example, the manuscript states that the sediment is primarily composed of calcium carbonate (CaCO₃), suggesting that carbonate dissolution and precipitation reactions may influence subsurface CO₂. Furthermore, microbial respiration is likely an important source of CO₂ and sink for O₂. The relative contributions of these processes are expected to vary seasonally in response to changes in temperature, soil moisture, groundwater conditions and recharge events. Since the study focuses on seasonal variations in gas exchange, a more comprehensive discussion of the biogeochemical processes controlling CO₂ production and O₂ consumption

would strengthen the interpretation of the observed fluxes and help distinguish gas production from gas transport.

This is an important comment that we didn't discuss properly in the manuscript. The focus of this study was on the physical transport mechanisms and potential flux, and not on the sources, as we think that with the current experimental setting, we cannot validate the source of the CO₂ and O₂ dynamics. Nevertheless, we understand that we cannot neglect this important issue, which is why we propose adding a new paragraph to the discussion that addresses potential sources as a future open question. In this discussion, we use the measured CO₂/O₂ ratio as an indicator of potential sources; we also acknowledge that additional research is needed. The proposed paragraph is:

“This study focuses on the gas transport mechanisms rather than the sources of CO₂ and O₂ (production and consumption). Yet, the CO₂-to-O₂ ratio measured in both wells, also defined as the apparent respiratory quotient (ARQ) (Angert et al., 2015), can provide a potential source. ARQ of ~1 indicates microbial aerobic respiration, whereas ARQ $\neq$ 1 may be explained by other processes, such as carbonate dissolution, iron oxidation, and sub- or anoxic processes (Nguyen et al., 2025). In both wells, the measured ARQ was ~1 (e.g., 1.01±0.15 for the dry well throughout the experiment), thus indicating that the source of the CO₂ was microbial respiration. Nevertheless, the calcium carbonate (CaCO₃) present in the sediment surrounding the wells suggests that carbonate dissolution and precipitation reactions (i.e., the carbonate system) may also influence the subsurface CO₂ dynamics (Angert et al., 2015, Bekin et al., 2026). The relative contributions of these processes cannot be determined in this study and need further research, especially as they are expected to vary seasonally in response to changes in temperature, soil moisture, groundwater conditions, and recharge events.”

Additionally, below I list some minor points that authors need to consider:

- Line 49: contribute needs to change to "contribute to".

Done.

- Line 179: What about diffusion then? The changes in the concentration between the two sensors can also be due to diffusion. Please see my comments above.

We revised the manuscript in several locations to discuss also diffusion; please see additional details in our answers to major comment #1.

- Line 190: Section 2.4.2 only focuses on a simple calculation and a short paragraph. I think this does not require a separate section and you can simply mention that somewhere in the text.

We incorporated this section with the next one under a single title: "2.4.2 CO₂ flux calculation".

- Line 200: I am not sure if we can call this a flux calculation. Flux is commonly defined per unit area.

We agree that this is the common term for soils or water bodies, yet we consider wells a unique case for flux calculation because the flux contribution is per well, not per area (e.g., m² or km²). To avoid confusion, we strictly defined it in the first sentence of this paragraph:

"To calculate the CO₂ fluxes, which in this case is the flux per well and not area,..."

- Line 245: Please explain why the O₂ concentration was low and remained so for such a long time? I am surprised that it was not displaced by atmospheric O₂ through diffusion.

This is explained by the relations between CO₂ and O₂. Because the CO₂ was high and stable, the O₂ was also low and stable. The ratio between CO₂ and O₂ was ~0.8-1 (i.e., an increase in ~4% of CO₂ was synchronized with a decrease of ~4.6% in O₂). We note that the actual ratio was likely closer to 1, since the CO₂ sensor's range limit was 4%. The fact that both CO₂ and O₂ remained stable for a long time indicates that this is a signal from the source below, without active advective mixing.

- Line 400-405: In Figure 7c, the bottom node in the wet well shows very high CO₂ concentration (around 40000 ppm) that also fluctuates quite a lot in the winter. Please comment on that and explain if/how that is connected to recharge or thermal dynamics? The authors argue that this period is associated with thermally-induced mixing in the wet well, but I am not sure why O₂ and CO₂ concentrations fluctuate so much in this period.

In summer, ΔT_v is negative throughout (Fig. 7d), so the column is stably stratified and with a diffusion-controlled gradient (Fig. 4b). In winter, ΔT_v is mostly positive, reaching +10 to +20 °C. The deep-well air remains near the annual mean subsurface temperature, while the outside air is cold. TIC then mixes the upper part of the column (Fig. 4b, spring and winter profiles). The TIC-mixed layer does not reach a fixed depth. Its thickness depends on how large and how persistent ΔT_v is. The -30 m node sits near the lower edge of this layer, in the transition zone where concentration changes steeply with depth. Small shifts in that boundary move the node

between the mixed layer (close to atmospheric conditions of low CO_2 and high O_2) and the stagnant lower zone (high CO_2 and low O_2). This explains why this winter's fluctuations are mainly TIC-driven, as opposed to the smooth diffusive gradient in summer. Towards the end of winter (~start of March 2025 in Fig. 7d), when ΔT_V is approaching negative values, it is clear that the CO_2 and O_2 fluctuations decrease until complete and stable stratification.

We agree that the recharge events can also contribute to these fluctuations, as they also perturb the air column directly. Additionally, once TIC has flushed the well, the rebuild rate depends on this source strength. A weaker CO_2 source, due to dilution of CO_2 in groundwater from recharge events, leads to slower recovery and a lower plateau.

Unfortunately, the relative effect of each process (TIC vs. Recharge) cannot be validated from this dataset. We propose to acknowledge this by adding the line below after the discussion of the effects of the TIC and recharge processes during winter.

“TIC and recharge cannot be fully separated in this dataset, because both are active in winter”