The manuscript evaluates the reliability of process-based ^{222}Rn emissions in Europe using atmospheric inversion modeling—an important and timely topic within the scope of Atmospheric Chemistry and Physics (ACP). By assessing different soil moisture-driven emission maps through an inversion framework constrained by ^{222}Rn observations, the study provides insights into limitations of current ^{222}Rn emission modeling approaches.

Major Concerns:

Writing and structure: The writing quality needs some improvement. Several section titles are overly informal, and some figure captions are too lengthy or imprecise. Enhancing the clarity, consistency, and scientific tone of the manuscript would significantly improve its accessibility and impact.

Methodological clarity: There are a number of unresolved issues in the methodological approach, particularly regarding the inversion framework. These include inconsistencies in particle counts across models, the interpretation of time-aggregated versus time-resolved footprints, the background subtraction procedure, and the handling of prior–posterior dependencies. Some aspects of the inversion should be more rigorously justified or supported with sensitivity analyses.

Limited broader context: The manuscript would benefit from a broader discussion of the implications of its findings—particularly regarding how the improved ^{222}Rn flux estimates might support applications beyond radon itself, such as in evaluating atmospheric transport schemes or constraining emissions of other trace gases.

Strengths:

Despite these concerns, the manuscript includes a number of thoughtful discussions.

The authors critically assess the data coverage of ^{222}Rn observations, highlighting how observational density shapes the inversion's dependence on prior information.

The manuscript provides a breakdown of uncertainty sources, including observational scaling, model assumptions, and soil parameter variability.

The soil moisture sensitivity analysis, particularly the exploration of top-10 cm versus deeper layer effects, is a valuable contribution and has potential implications for improving future radon flux parameterizations.

Line by line comments:

Line 17: In the abstract, “In this study, we evaluate two process-based 222Rn flux maps for Europe based on two different soil moisture reanalysis products (GLDAS-Noah and ERA5-Land) using the flux results obtained from a one-year 222Rn inversion performed with the CarboScope-Regional inversion system and 222Rn observations from 17 European sites.” The sentence is quite long, making it challenging to follow on a first read. My suggestion is but may not be accurate:

“This study evaluates two process-based ^{222}Rn flux maps for Europe. The maps are developed separately using different soil moisture reanalysis products, GLDAS-Noah and ERA5-Land. The evaluation is based on flux results from a one-year ^{222}Rn inversion, which was conducted using the CarboScope-Regional inversion system, and observational data from 17 European sites.”

What is the role of the inversion system? Does it used to estimate surface 222Rn concentrations using the two soil moisture reanalysis products?

Line 138: “Whilst the GLDAS-Noah and ERA5-Land models assume that the soil porosity is constant over the entire soil column, AMBAV uses vertically resolved soil porosity data (from Hartmann et al., 2024).”

What could be the magnitude of the impact from using constant soil porosity (as in GLDAS-Noah and ERA5-Land) versus vertically resolved porosity (as in AMBAV)? Can this help explain the comparisons later in the manuscript?

Line ~150, Table 1: It would be better to expand Table 1 to include additional information such as temporal resolution, type of soil model used, and porosity representation (e.g., 3-hourly, constant vs. vertically resolved). This would help readers understand their influence on the ^{222}Rn flux calculations.

Line 155:Title of Section 2.2 is the same as that of Section 2.1. It could be renamed “^{222}Rn Observations” or something similar.

Line ~215: The caption of Table 2 is not sufficiently concise. Some information currently in the caption could be moved to the main text or directly represented in the table layout.

Latitude coordinates should explicitly indicate N (North) or S (South) to avoid ambiguity.

Consider rephrasing the caption for clarity and consistency. For example:

"Table 2. Overview of the 17 European sites providing atmospheric ^{222}Rn measurements."

Also consider including symbol definitions as footnotes directly under the table, such as:

T – Tower sites

* – High-altitude sites

Line ~227: The manuscript states that particles are released at regular hourly intervals rather than matched to the exact ^{222}Rn measurement times. Any reason that the particles cannot be released at the exact measurement time?

Line ~230: It would be better to include an equation indicating how ^{222}Rn concentrations are calculated from the footprint and fluxes. This would clarify the convolution process and demonstrate how radioactive decay is incorporated.

Line 235–240:The manuscript notes that 100 particles are released in STILT, compared to 20,000 in FLEXPART. This discrepancy raises concern. After 10 days of backward transport, especially within the boundary layer, the number of STILT particles in the footprint layer may be very small, potentially causing biased flux estimates. Could the authors clarify why such a low number is used in STILT? would increasing the number of STILT particles (e.g., to match FLEXPART) improve consistency and reduce sampling error?

Line 235–245: The authors use 30-day back-trajectories for FLEXPART and NAME, but only 10 days for STILT. Given the ^{222}Rn half-life of 3.8 days, the contribution from emissions beyond ~10 days should be minimal. Could the authors clarify whether the 30-day simulations were necessary for resolving ^{222}Rn fluxes?

Line ~245–255: FLEXPART and NAME use ^{222}Rn-decay-corrected footprint products aggregated over 30 days, and that these footprints lack temporal resolution. Without knowing when the footprints are most sensitive, it is unclear how the daily fluxes can be meaningfully convolved with the aggregated footprints.

Line ~255–260: The authors justify their use of 3-day averaged fluxes with FLEXPART and NAME based on timing sensitivity inferred from STILT. However, STILT uses only 100 particles, which may not be sufficient to robustly resolve the timing distribution after 3–10 days of transport. Moreover, FLEXPART is capable of outputting time-resolved footprint information. There are critical compromises in the intercomparison of the three model products.

Line ~260–275: The manuscript applies a modeled ^{222}Rn background correction by subtracting TM3-simulated concentrations based on a globally uniform ^{222}Rn flux map. However, this approach raises several concerns. First, the short lifetime of ^{222}Rn means that long-range contributions to observed concentrations are inherently small. Subtracting a modeled background may introduce biases and artificially reduce the observational constraint. I do not think this is necessary. Second, the assumed constant fluxes outside Europe lack empirical justification and do not account for known variability in soil radium content, land cover, or meteorology. This can introduce noise in the seasonal/spatial variations.

Line ~280–285 (in the inversion system description): Including an equation of the inversion system would clarify how the posterior fluxes are derived and how prior information and observational constraints are weighted. A simple expression such as

$$J(x) = (x - x_{\text{prior}})^T \mathbf{B}^{-1}(x - x_{\text{prior}}) + (y - Hx)^T \mathbf{R}^{-1}(y - Hx)$$

along with brief definitions of terms.

x: unknown posterior ^{222}Rn flux field; x prior: prior flux map

y: observed ^{222}Rn concentrations; H: footprint-convolved fluxes

This may raise another critical issue. If my expression is correct, then H is NOT independent from x prior. It is calculated based on the flux map, which is x prior here. I am not sure if the inversion model is still valid if H is a dependent of x prior. Again, my expression can be wrong, so it is important for the authors to provide such an expression.

Line ~310, Section Title: The title “Temporal correlation between soil moisture and 222Rn flux variability” sounds like it will present a data-driven correlation study between two independently measured quantities: soil moisture and ^{222}Rn flux variability. In fact, it investigates the impact of using a flat prior compared to the soil moisture based prior. I believe that this involves not only temporal variations but also spatial variations. The title can be something like “2.4 The impact of using Soil Moisture based ^{222}Rn fluxes compared to a flat prior on the inversion system”. This is probably not good enough. I leave it here for the authors to polish more.

Line ~360, Figure 3: The maps present annual means. However, given the high temporal variability of ^{222}Rn fluxes driven by soil moisture and meteorology (as shown in the time series), the maps cannot provide much information. It would strengthen the evaluation to also show the innovations at monthly or seasonal resolution.

Line ~370: Figure 3 shows that posterior ^{222}Rn fluxes over the European domain exhibit a clear seasonal cycle, while the Germany-only domain shows much weaker seasonal variability, particularly in the flat-prior inversion. It would be better if the authors can discuss this difference and its implications for interpreting regional versus continental-scale flux patterns.

Line ~378: A more precise title could be “Sensitivity of Inversion Results to Model Configuration”

Line ~402: “The NAME forward simulations” have never been introduced before this point.

Line ~417: “However, there seems to be a slight seasonal cycle in the difference”. Again, it would be better to have some discussion about reasons for such seasonal cycles.

Line ~448: The title “Alternative ^{222}Rn flux maps” does not clearly reflect the scope of the section, which focuses on analyzing the temporal variability of posterior ^{222}Rn fluxes and their correlation with soil moisture at different depths. Try from “Evaluating Soil Depth Sensitivity in ^{222}Rn Flux”.

Line ~475: The authors state that the annual mean flux is only marginally affected (2–3%). It would be valuable to also assess the impact on daily or monthly mean fluxes. Such resolution is particularly relevant for interpreting short-term atmospheric ^{222}Rn observations and improving result fidelity.

Line ~516: Although I had a few concerns about the methodology, the discussions here are quite thoughtful. The sources of the big difference between process-based fluxes and the posterior fluxes are clearly articulated, and the uncertainty due to model assumptions versus prior biases is discussed carefully.