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Interactive Comment

Interactive comment on "Characterization of the ^{222}Rn family turbulent transport in the convective atmospheric boundary layer" *by* J.-F. Vinuesa and S. Galmarini

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Are radon and its decaying products influenced by the vertical turbulent transport due to their radioactive transformations? What is the role in the distribution of radon of the exchange between the free troposphere and the atmospheric boundary layer under convective diurnal conditions? These are the two main interesting questions posed in this research. In my opinion, they are partially answered and, consequently, the investigation can benefit by further and more complete analysis. Therefore, I will suggest to the authors to elaborate more and deeper in their results and the discussion before the paper is finally accepted. If they wish, I am willing to work with them in the processes of revision. Below, I shall give my suggestions. 1.- Are radon and its decaying products influence by the vertical turbulent transport? The research only shows that turbulent transport can locally influence the vertical distribution of radon and its decaying components. Other relevant information is missing in the discussion. To be more specific: (a) Are the time evolution of the mixed-layer concentrations also affected by this process or is it only a localized effect? and (b) How do these results compare with the evolution of inert species? The authors have the tools to answer these questions, but they seemed reluctant to do it (see my first review previous to the acceptance to ACPD). The application of a mixed-layer model with a radioactive module, where turbulence is not explicitly solved, and its comparison against the LES numerical results is a quite straightforward procedure to answer these questions. If the authors want to present a comprehensive characterization of radon in the boundary layer this information has to be included. Additionally, these results and discussion are very relevant for large scale modelers interested in using radon to evaluate their boundary-layer transport schemes. 2.- Figure 4 and 5 show basically the same: the concentration gradient and the flux depart from their inert profiles due to the contribution of the chemical term. However, and in my opinion, the relevant question is: How much (and where) the time variation of the concentration gradient departs from the quasi-stationary state condition. By investigating the departures from this state, they could also provide an estimation of how much the flux will depart from its linearity due to the radioactivity transformation. 3.- In connection with this last point, Could the authors discuss if the exchange coefficients of certain decaying species depart from the inert form (Galmarini et al., Quarterly Journal Royal Meteorological Society, 123, 223-242, 1997). This analysis in combination with the non-dimensionless flux Damkohler number can be very useful in the determination of specific exchange coefficient for reactive species. 4.- As this research shows, flux and gradients for certain decaying species (for instance S1 and S2) are largely modified near the surface. At this point my question is: Is still appropriate to make use of a sub-grid scale (SGS) model that is independent on the scales? One of the authors is specialist on sub-grid scale dependent models. However, in the manuscript nothing is mentioned respect the choice of the SGS model in the large-eddy simulation model. To my knowledge, it will be more appropriate to use a dynamic SGS model in regions characterized by strong gradients either created by turbulence or by the chemical sources and sinks, i.e. in the current case understudy near the surface. In consequence, one might wonder if the results presented in the paper are dependent on the choice of the SGS model. 5.- I still think it is necessary to add a sensitivity analysis study on the upper boundary condition. The case that they show is an extreme situation with a very rigid lid that can be very bias in the discussion of the role of ventilation on boundary layer concentrations. Just to put an example, the concentration gradients discussed at figure 4 can also be dependent on the exchange between the free troposphere and the atmospheric boundary layer. For instance, moisture, an inert scalar, shows also clearly gradients in the mixed layer. By adding a numerical simulation with a weak inversion jump, one can learn the dependence of the vertical distribution of radon to the inversion conditions. 6.- Section 5.3 requires a more thorough discussion. For instance at figure 11, it is rather confusing to show that in the first two hours the mixed layer concentration of radon approaches a value similar to the free troposphere value (I guess a decrease of the radon jump at the inversion and therefore a potentially less exchange flux at the top of the boundary layer). However, the flux at the top of the boundary layer still increases. Why? The authors should provide a clearer description of the situation understudy. 7.- Closely connected with points 4 and 5, it is the numerical set up of the unsteady case. Apart from the very large potential temperature jump at the inversion, they have prescribed a surface heat flux constant on time. The contribution of a relative strong forcing in the early morning hours could lead to erroneous interpretation of the role of ventilation. It seems to me that it is unbalanced to be very precise on the upper boundary conditions for the decaying species (section 3.1) and not so much with the dynamic governing forcing at the surface and at the inversion. I will appreciate very much if this point is clarified. A possible solution is to carry out a simulation where the surface flux is better characterized by a diurnal variation of the surface heat flux. 8.- This more physical set up can also be very beneficial for the interpretation of the re-

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sults in terms of the Damkohler number (Table 2). In my opinion, in the unsteady case, the Damkohler number should commence with lower values that the ones indicated at Table 2. 9.- At figure 11, the authors show a slight decrease of the radon concentration. They related to the dilution in the boundary layer growth. As mentioned the decrease is rather gentle, why do they mean by "collapse" in the mixed-layer concentration at the conclusions?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8917, 2006.