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Interactive Comment

Interactive comment on "Evaluation of the vertical diffusion coefficients from ERA-40 with ^{222}Rn simulations" *by* D. J. L. Olivié et al.

Anonymous Referee #1

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This is an excellent paper that provides interesting insights in the use of Rn222 to evaluate vertical diffusion parameterizations, and clearly the authors have done a great job in bringing different datasets together. Having said this I think there are several improvement to the paper possible. The most important ones are:

1. A clearer statement of the objectives,

2. an improved organization of the paper (this should not be so difficult)

3. the conclusions (+abstract) better reflecting what we have actually learned.

Further I would appreciate a widening of the scope of the paper as to what the use of these different assumptions on vertical diffusion coefficients means for other tracers (e.g. NOx, hours to 1 day lifetime; O3/CO 20 days, CH4 years), and other regions (eg

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the tropics). I will suggest some simple diagnostics that could do this job.

Ad 1.: the paper is clearly not only evaluating ERA-40; but the real issue seems to be the use of "off-line" and "on-line" diagnosed diffusion coefficients, two/three different sets of parameterization, and the required time resolution. As such the title does not completely capture the contents of the paper.

Ad 2. The organization of the introduction is a bit messy, Section 2 should be called methodology (TM3 is part of it). 2.4 should be in results section; try to have the section header cover what is discussed.

Detailed comments: p. 4131 l.7: more generally Rn222 is referred to as a radionuclide.

p. 4131 l.21: does the off-line Kzz calculation reproduce the diffusion coefficients or rather the effects on Rn?

p. 4135: I would list towards the end of the introduction the objectives; and then systematically come back to them in the text and conclusions.

p. 4136 l.22 6-hourly or 3 hourly; this is confusing I think that if you store 3 hourly values automatically you have also 6 hourly values?

p. 4137 l. 5 mention units for the excess value (K?)

p. 4140 l. 10 Again confusing 3 and 6 hours.

p. 4143 l. 2 Should be a separate section. I wonder how the averaging of non-linear properties such as diffusion coefficients to TM3 grid has been done? What ECMWF resolution is used for the "off-line" diagnosis of diffusion coefficients

p. 4144 section 2.4 should move to results

p. 4144 l.12 values below: $P>600$ Hpa. How much is 2 or three layers?

p. 4144 l. 20 This is an important finding, which should make it to the conclusions. I think we should acknowledge (and if possible evaluate) the fact that free tropospheric

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diffusion coefficients are poorly evaluated (as are the length scales used in the parametersiations).

p. 4145 Why only land profiles? What about the oceanic profiles are they more similar among schemes? Further, I am wondering if you could better not evaluate the 10 percentile, median and 90 percentile values, since the mean can be so much distorted by extreme values.

p. 4145 boundary layer height is interesting. However I think we should realize that the evalution is merely based on mid-latitude stations- for which ECMWF is supposed to work better anyhow. Do you see some possibility to give the reader some feeling for how the model is performing in tropics? Perhaps using Lidar data, or ozone profiles as proxies for the boundary layer height? p. 4146 Figure 6 give corresponding numerical values (perhaps in the plot).

p. 4147 l. 20 Cincinatti and Socorro are done later.

p. 4148 Table 4 is poorly explained. For instance what are we supposed to learn from case (c)?

p. 4149 The main difficulty with the use of the Schauinsland station is that it is fairly difficult to assign a specified model height to the station. Sometimes the station will be well separated from the lower Rhine Valley; at other times up-slope conditions me prevail, and the model should be sampled as if it where a surface station. The truth is probably somewhere in between.

p. 4150 The issue of time-staggering is probably somewhat separate from the the previous discussion; maybe it should be brought later. I think you should point out that this ŚtimeŠ correction will probably be strongly dependent on the geographical location and cannot be generalized. It is probably worth to have it in the conclusions; since it is a motivation for having 3 hourly (or even better) time resolution for diffusion coefficient calculations.

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p. 4151 section 3.2.5 could be combined with 3.2.3

p. 4153 it is nice to see these old measurements used, and a good example of use of ERA40 data.

p. 4153 A main reason for discrepancy is that the morning measurements represent more local emission conditions, which may not fulfill the ubiquitous 1 #/cm2/s.

p. 4154 section 3.4 Here I would appreciate horizontal plots of field ratios evaluating the effect of the different mixing schemes 1) for Radon 2) for a tracer with a lifetime of 6 hours 3) for a lifetime of 1 month.

p. 4156 make sure all the important findings are in the conclusions.

p. 4172 Fig 2. As said I have the feeling that the graphical presentation of mean and +/- 1 sigma is a bit misleading. Better percentiles and median. I think it is also good to discuss somewhere in the paper the relationship between diffusion coefficient and timescale, defined as t=dH*dH/Kzz, also in relationship to other model timescales.

Fig 4-6; the plots are nice, but I feel the need to quantify the agreements and mismatch.

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