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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Predicting terrestrial ^{222}Rn flux using gamma dose rate as a proxy" *by* T. Szegvary et al.

Anonymous Referee #2

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General Comments:

I think this is a valuable paper worthy of publication. The radon flux from the earth's land surface is an important subject that is sorely in need of more information related to spatial and temporal variation and overall quantification. Especially needed are field measurements of radon flux for diverse regions and conditions – both to be used directly, and for calibration of models. An important aspect of this paper is its presentation of valuable new field measurements of radon flux for central Europe. Although the subject of a correlation between radon flux and gamma dose rate is not new, this paper provides new data and appropriate evaluation for the selected European countries. The authors do a good job of discussing some of the sampling issues related to more localized radon flux measurements versus gamma dose rate measurements that

is probably average over a larger region of soil.

The paper could be strengthened by some revision. Radon flux measurements using the closed chamber method are a tricky business. Although the authors offer a fair amount of discussion and references, I'm still worried they may have missed some things. Careful calibration of their chamber technique may not be so important for conclusions related to the correlation with gamma dose rate, but for archival purposes and future use by other scientists, the subjects of calibration and accuracy are worthy of careful attention. Since the authors have gone to a lot of trouble making measurements at diverse locations, it would be a shame not to have as much information as available on accuracy and calibration. Similarly, it would be helpful to have a little more background information on the location (regional map?) and conditions of flux measurements (soil properties? meteorology? climate?) and a more quantified breakdown of results by location and/or measurement conditions. In other words, if a new researcher wishes to apply the same methodology at some new location or time, provide him/her with as much information as possible for projection of expected results for the new situation.

Specific Comments:

Let me start with some comments on the closed can technique. Since the authors have gone to a lot a trouble to make measurements at diverse locations, it would be nice if we could have as much confidence as possible in the accuracy of the results for use by others in perhaps other contexts. One of the best ways to establish accuracy in this field is to do intercomparison measurements with other research groups. If something like this has been done, it would be helpful if the authors specifically referenced it and gave a one sentence summary. Unfortunately, radon flux standards usually do not exist in this field, so lacking intercomparison measurements, the authors are then left to try to estimate errors by analysis. In section 4.1 the authors mention a +/- 15% error. Where did this come from? I can name two potential sources of error that especially worry me and I could not find addressed (they might be buried in a listed reference I

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do not have). One is the subject of “back diffusion”: it does not take much build up of radon in a chamber for that radon to reduce the gradient from soil in a very tiny surface soil layer and reduce the apparent flux density of radon. Have the authors considered this effect? This subject has been around for a long time. Some of the more recent references related to it are Y. S. Mayya, “Theory of Radon Exhalation into Accumulators Placed at the Soil-Atmosphere Interface”, Radiation Protection Dosimetry, 2004, v. 111, #3, p. 305-318. Another more general reference is Hutchinson et al., “Chamber Measurement . . .”, Journal of Geophysical Research/ Atmospheres, April 16, 2000, v. 105, #D7, p. 8865 – 8875. Another source of error that can be important is a difference in pressure between the inside and outside of the chamber. A pressure difference as small as a fraction of one Pascal can be important. Now pressure differences much larger than this are possible due to the pumps and lines sampling the radon from a chamber, depending on how they are connected. Sometimes two pumps are required, one at the inlet and one at the exhaust to avoid impedance-caused pressure drops. Sometimes a Bernoulli effect from wind can cause a pressure difference. The only way to be certain is to check the pressure difference under field conditions. Have the authors considered this issue?

The authors discuss some other factors affecting radon flux such as local variability in soil properties and rain (soil moisture). There are other possible factors. Did they see any correlation with other factors such as temperature or atmospheric pressure? For example, there seem to be some systematic patterns in figure 3 top that are not explained by rain. Do they have ideas about the explanation? Any chance of an effect due to diurnal atmospheric pressure variation?

On the subject of more complete, and quantitative, reporting of results here are some thoughts. I have in mind trying to have a fuller understanding of the background conditions for the results reported by Table 1, Figure 1c, Figure 2, and Figure 3(top), but only to the extent that such information is easily available. Would a map help for better referencing the location of measurements? Can the authors supply any other sup-

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porting information for measurements conditions? If not, say, averages for detail like radium content of soil and soil moisture, maybe more generic information such as soil type, climate classification, etc. If grass or small plants were present at a sampling site, were chamber measurements made over them or were measurements restricted to bare soil? Maybe indicate whether numbers such as given in Table 1 are averaged over all times of days or seasons, or some specific time of day or season. Would it possibly help to provide both median and means for certain tabulated results? The authors should be able to see the gist of what I have in mind. In other words, if I wanted to go back to your sites and replicate your results, what would be helpful to know. The authors do not need to go overboard here, only give what could be helpful and easily provided in a short paper.

A major conclusion of the paper is establishing the correlation between radon flux density and gamma dose rate. The authors make a very convincing case. It might be worth giving more quantitative detail such as the actual factor to multiply gamma dose rate at some height to get average radon flux density. Possibly include an estimated error in these factors.

One of the authors' main conclusions was that the relation between radon flux and gamma dose rate was fairly robust and constant from one region to the next. The authors commented about a correction for anthropogenic cesium. How about the following issue. Generally, outdoor gamma dose rate comes from the soil uranium series, the soil thorium series, soil potassium40, and cosmic rays. There are geochemical reasons why uranium and thorium are often correlated in soils. However, gamma dose rate varies with altitude, and potassium geochemically is somewhat independent of uranium and thorium. I have seen radiation survey maps for the entire United States, and noted there are regions where the soil potassium is high without a proportional increase in the uranium/thorium series. Keeping in mind issues such as potassium variation and a cosmic ray altitude effect, do the authors care to speculate on how robust their conclusions will remain for regions beyond the area of Europe they studied?

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Technical Corrections:

I saw some minor errors in standard usage of English grammar and syntax but not significant enough to list line by line.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1877, 2007.