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Interactive comment on "One year of ^{222}Rn concentration in the atmospheric surface layer" *by* S. Galmarini

Anonymous Referee #2

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This paper analyzes a 1-year time series of surface radon-222 measurements made in Northern Italy during 1997. The paper uses wavelet analysis to investigate the influence of several meteorological parameters on radon-222 concentrations (primarily wind speed and precipitation). The authors argue that the analysis provides strong evidence that precipitation reduces surface radon emissions, and that global model parameterizations of radon emissions need to include this effect to be valid. It also identifies several periods where a secular increase in radon-222 is superposed on a diurnal cycle and uses these periods to estimate a surface emission rate for radon at the measurement site. I think that the attempt to understand the influence of meteorological variability on the variability of a time series of surface radon measurements is

a scientifically useful and interesting exercise. I appreciate the author's attempt to add some sophistication to the analysis through the use of wavelets. However, I think there are a number of problems with this paper which should be addressed.

First, I find the author's analysis of the relationship between precipitation and surface radon emissions to be unconvincing, particularly the analysis of precipitation events shown in Figure 4. Figure 4 clearly shows that the reduction in peak radon concentrations and the disruption of its diurnal cycle occurred prior to the precipitation event that the author argues is the cause of the observed change in radon. I do not see how radon could be responding to an event that has not yet occurred. In this instance, I think that the wavelet analysis is confusing because the amplitude of the power at longer periodicities rises prior to the precipitation event itself. This cannot mean however, that an event can influence the past. A second problem with the author's argument is the fact that Table 1 indicates that precipitation and radon are virtually uncorrelated with each other. The paper never explains the lack of a strong correlation. I think the small correlation shown in the table is probably due to the fact that no precipitation occurs on many days even though radon varies during those days. A better evaluation of the true effect of precipitation on radon concentrations might be obtained by calculating the correlation of radon and precipitation only for the subset of days during the year on which significant precipitation occurred.

A second problem with the analysis is its attribution of short time scale features seen in the wavelet transforms to various physical processes without actually demonstrating a connection between the selected physical processes and changes in radon concentrations. For instance, on page 12904, an 8-hour timescale is identified in the January scalogram (Figure 3a). A claim is made that this timescale results from the formation and evolution of the nighttime boundary layer. However, this claim is never supported with any real evidence. The paper does not explicitly show that the boundary layer evolution at the time of the measurements actually occurred as described in the paper. The reader thus is left with only a plausible explanation, rather than one whose validity

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is actually demonstrated.

Third, I was confused as to why the red noise spectrum shown in Figure 2 is an appropriate "null hypothesis," and would appreciate a few sentences of explanation here.

Fourth, on page 12901, six energy peaks are identified in Figure 2b. I don't see the meaningfulness of the significant figures when the radon time series consists of 1 hour resolution data.

Fifth, I am skeptical of the claim made on page 12910 that the exhalation rate of radon from the ground can be assumed to be constant because of the lack of precipitation events. It is my understanding that pressure variations can also produce variations in radon emission (e.g., Schery et al., "Factors affecting exhalation of radon from a gravelly sandy loam," J. Geophys. Res., 89, 7299-7309, 1984).

A final criticism is that Table 1 shows us the correlation between radon and various meteorological parameters that occurred during the year, but does not indicate the probability that the observed correlation occurred by chance. Without this number it is impossible to determine the significance of the presented correlations.

To improve this paper I think the author should reexamine his contention that precipitation has a strong impact on surface radon fluctuations. More convincing evidence of a strong connection should be provided. I also think the paper should provide solid evidence that the physical processes which are identified as being responsible for the features shown in the wavelet scalograms are in fact responsible. Finally I think that Table 1 should be modified to include the probability that the observed correlations occurred by chance.

Typographical Errors:

Page 12897: "... where not investigated before." should be "... were not investigated before." Page 12913: "... where precisely connected..." should be "... were precisely connected..."

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Interactive Comment