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***Interactive comment on* “Surface-to-mountaintop transport characterised by radon observations at the Jungfrauoch” by A. D. Griffiths et al.**

Anonymous Referee #2

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Comments on “Surface-to-mountaintop transport characterised by radon observations at the Jungfrauoch” by Griffiths et al. In this paper authors described a radon-based approach to infer surface-to-mountaintop transport mechanism using meteorological and other in situ measurements at a ridge top site (Jungfrauoch, henceforth, JFJ) and at a nearest plateau site (Bern). The discussed a method to detect anabatic wind. Finally, they quantified the radon measurements performed at the saddle point JFJ with respect to the impact of vertically transported radon from the adjacent region pertaining to anabatic wind in different seasons (except winter). They found three components of the diurnal cycle of radon measured at JFJ: (1) baseline, (2) anabatic, and (3) non-anabatic. The authors seemed to have an adequate set of data to document the vertical transport mechanisms related to anabatic wind often observed in the experi-

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mental area. While I consider that such work is necessary given that mountaintop sites are frequently affected by pollutants from adjacent plains/valleys, I have some concerns about the method, analyses, interpretation of the results presented in the paper. They used numerous awkward phrases in the paper which are oftentimes impossible. I provided some examples. This paper cannot be accepted in its present form for publication in the journal Atmospheric Chemistry and Physics. Major issues discussed below need to be addressed before the paper is accepted. Major comments Method: Authors should elaborate their methodology. In its current version, it is not transparent enough to be reproduced by another researcher. In particular, step 6 needs to be discussed in detail and step-by-step. For instance, they should first discuss the general pattern of trace gas concentration variability and how is it affected by slope flows so that “sinusoidal” curve is often observed. They should also discuss the later part of diurnal cycle pattern with an aim to demonstrate the role of downslope flows and their effect on trace gas concentration via mass budget of tracers. Finally, they should explain why JFJ measurements in the nighttime contain signatures of free tropospheric values. In step 2, they mentioned 07:00 UTC to be the time when minimum radon concentration in the JFJ diurnal composite is observed. I wonder if this time is season-independent. Please explain. While separating different contributions, they discuss that the anabatic part is the mean of the running diurnal composite minus background. Is this basically the amplitude of the running diurnal composite? Above their threshold value (i.e. 220), resulting intra-day variability could be also due to the situations when Rn concentration at the saddle point is higher than the Rn concentration in the air mass arriving via anabatic wind. This is particularly true when JFJ remained within an elevated residual layer rather than FT since in the residual layers the mean state variables are the same, at least initially, as those of the boundary layers. Many previous studies showed the mechanisms and roles of elevated residual layers which form frequently in regions near mountains where terrain gradients exist. Additionally, they should discuss in detail how does the interaction between anabatic wind and mountain-top measurements affect the representivity of JFJ measurements to be considered as

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background (baseline) measurements. How does this factor vary w.r.t. different seasons? This will be important information for the researchers looking for different factors affecting the FT background values in this area. In summary, the method section lacks detailed scientific understanding on the impact of anabatic wind on mountaintop Rn concentrations although they have a nice data set for more than two years at their disposal confirming variability of Rn at both the saddle point and at the nearest plateau region. Additionally, they should clearly discuss how does the rank-based approach differentiate anabatic component and advected component since JFJ measurements are oftentimes influenced by transported contribution. P18092, L5: “intra-day variability combined with the method’s inability. . .” What makes the method imperfect? What are the most possible reasons for intra-day variability? Please discuss the site-specific meteorological conditions. Results and Discussion P18093, L10: “allowing 77 %” what explains the remaining 23 % of days to be non-adiabatic. Are these days affected by dynamically driven winds and/or synoptic scale advection? Please also explain the role of convective boundary layer along with overshooting thermals which oftentimes reach mountaintops although the CBL top height remains below the ridge height. P18093, L13-15: “Mean radon concentration” Do you mean daily average values? P18093, L13-15: “Strength of the diurnal cycle”: Do you mean peak to trough amplitude? This is another example of awkward phrase. P18094, L13: “different time-scales” what do you mean by different? Please clarify. P18094, L15: “diurnal cycles of similar strength” What do you mean here? Unclear to me. An important discussion about the results presented in Fig. 5 is missing: They should discuss the relationship between low/high rad days with the observed radon gradient (Bern and JFJ) in the daytime. How does that relationship relate to the gradient values reported in the literature? It is obvious that CBL air reaches from plateau to JFJ and similar concentration is achieved in the daytime when anabatic wind plays an important role. In many past papers, using other tracers like CO, O₃, aerosols, etc. have been found. Which kind of new information you get using radon as tracer and the method introduced in the manuscript. I would suggest to investigating these issues by comparing gradient values and ranks on day

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to day basis. From the figure, I find differences of 8 bq m⁻³, 6 bq m⁻³, 7.5 bq m⁻³, 6 bq m⁻³ from one set of rank to the other starting rank set 75-124. This is also related to the CBL development in the plateau. They may exclude days in winter when no influence of anabatic wind was found. Additionally, authors should discuss the CBL development in the area and consider the findings addressed in Ketterer et al. (2014). They already referred to this paper in their manuscript. Section 3.4: The discussion on the aerosol washout effect is not important to this manuscript. Additionally, without a discussion on rain rate and timing of rainfall at the site, this information is not useful. I suggest dropping the entire section and they should reserve this for more dedicated studies in future with more detailed information on precipitation. Figures: Authors should increase the font size of the Figs. 4, 9, and 10. Legends with units are not readable. I had to magnify the PDF three times than appears on a normal screen to visualize correctly. Additionally, they should put the y-axis title at proper place in all figures like done for x-axis title. Otherwise, it is very difficult to extract the information and oftentimes too confusing. A detailed explanation of Fig. 10 is missing. In Fig. 5, Bern and JFJ should be indicated with the figure legends at a proper place (e.g. top left). Fig. 12: why do you use median instead of mean? Other comments 1. About the references: Some pioneering works by Whiteman (2000) related to thermally-driven anabatic winds and their effect on transport of pollutants is unfortunately missing. Additionally, authors should refer to Brooks et al. (2012) who discussed a spectrum of techniques in their paper to separate influence of transported contributions on mountaintop trace gas concentrations. [Whiteman CD. Mountain Meteorology: Fundamentals and Applications. Oxford University Press, 2000; 355 pp], [Brooks et al., 2012. Assessing filtering of mountaintop CO₂ mole fractions for application to inverse models of biosphere-atmosphere carbon exchange, Atmos. Chem. Phys.] 2. P 18086, L12: “a factor of three”. Please mention that this factor is site specific. 3. P18086, L19: CO: VOC? 4. P18087, L4: Syntax 5. P18095, L5, “with little dilution”: What do you mean by “little”. This is an awkward phrase for a scientific paper. 6. P18095, L5-6: “not by solar forcing”. Is it related to advection? 7. P18098, L24: “abatic”

SEE the PDF file attached with the same information above.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/14/C5530/2014/acpd-14-C5530-2014-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 18083, 2014.

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