

## ***Interactive comment on “The rarefied (non-continuum) conditions of tracer particle transport in soils, with implications for assessing the intensity and depth dependence of mixing from geochronology” by David Jon Furbish et al.***

### **Anonymous Referee #2**

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In this manuscript, Furbish, Schumer and Keen-Zebert investigate theoretically and numerically the evolution of tracer particles in a soil mobilized by all sorts of perturbations. More specifically, they address the concentration of cosmogenic isotopes such as  $^{10}\text{Be}$  and of quartz grains sensitive to Optically Stimulated Luminescence (OSL). To do so they derive, in much details, the evolution equations for the probability distribution of these particles, using the formalism of the Fokker-Planck equations. They also run numerical simulations which reproduce the erratic motions of these particles. Finally, the authors comment on how their results could help the interpretation of field

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measurements.

The manuscript is carefully written, and the mathematical derivations seem correct. I also believe that the authors' endeavor is needed, as more and more field data are collected, and often interpreted in a much wanting theoretical framework—in the literature, the  $^{10}\text{Be}$  concentration is often reported directly in terms of erosion rate, as if the two quantities were unequivocally related. I am therefore much supportive of the publication of this necessary work in E-surf.

The manuscript, however, is an arduous read. This is partly due to the subject itself, of course, but also, to a large extent, to the way the authors chose to present their results. Apart from the minor points listed below, I would like to encourage the authors to edit their manuscript to clarify their views.

Most of the difficulties stem from three decisions by the authors, namely (i) to present the mathematical derivations in full, even when they are very similar to each other, (ii) to treat the problems of OSL and of cosmogenic isotopes in parallel and (iii) to present their derivations and results first, and comment about them in the last section only, when the reader is most likely to have mixed them all together already.

I believe the authors could make the reading easier by presenting first the derivation of the Fokker-Planck equation for a cosmogenic isotope, and sending some intermediary steps of the derivations to the appendix. Indeed, these derivations are cumbersome, but not really difficult, and they often land on unsurprising expressions (equation (13) is a typical example). In most cases, the initial step of the derivation, and the necessary hypotheses, should suffice in the main document, before showing the final form of the expression. Then the authors could introduce their analytical solutions, and comment on them, before introducing the numerical simulations, and then comment on these new results.

Once the case of cosmogenic isotopes is clear, that of OSL particles would be easy to follow, if the authors content themselves with pointing at where the two derivations

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differ from each other, and follow the order proposed above.

Regarding the interpretation of the results, I was also surprised to find a mixture of crucial statements, such as the one regarding the age of a fluid parcel (page 25), with merely tautological ones: “*particle properties are not in themselves subject to advection and diffusion, but rather, are merely carried with the particles*” (page 31)—how could it be otherwise?

Regarding the former, I would suggest making them more explicit, by writing in full what previous theories say, and compare it to the authors’ results, ideally with a dedicated figure. As for the latter, I suspect the authors make these obvious claims because previous theories were ad odds with them. If it is so, I would recommend mentioning these theories explicitly, and point at where they might be flawed.

Overall, I would recommend spending more time on the points where the authors’ theory differ from previous ones, especially on the ones that are accessible to measurements, and drop all secondary points, or send them to the appendix. I believe this would result in a clearer paper, thus doing justice to the authors’ remarkable work.

#### Minor points

- Please write OSL in full in the abstract.
- Page 5, the discussion about the Knudsen number and the mean free path of particles is, at best, confusing. The Knudsen number compares the size of the system of interest to the mean free path of the particles, because the latter is the distance over which their trajectory loses its self-correlation. The value of the Knudsen number therefore tells us whether which macroscopic equations we can use, such as the Navier-Stokes ones. The density of passive tracers has nothing to do with this. A tracer particle interacts with many other particles before it encounters another tracer particle. That we follow a small number of

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particles (large “*geometrical mean free path*”) says nothing about the validity of the macroscopic equations. It affects the statistics of measurements only, which I believe is the point of the manuscript.

- Page 5, please define  $V_p$  carefully. It took me some time before realizing what it is.
- Page 10, I found that calling  $P$  an “advection speed” is confusing, and unnecessary. This is also true page 13.
- Page 20, are you sure the Fokker-Planck equations cannot tell us about the “*variability in <sup>10</sup>Be concentration of individual particles*”?
- Page 24, the discussion starting line 10 is confusing. Do you mean Lagrangian vs. Eulerian? This point might be related to the one above. Please clarify.
- Page 29, the discussion about the disturbances starting line 10 seems interesting, but I could not really understand it. Would a simple example help?
- Page 35, the statement starting line 10 seems tautological to me. Please see my remark above.

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