

## ***Interactive comment on “Remobilisation of uranium from contaminated freshwater sediments by bioturbation” by S. Lagauzère et al.***

### **Anonymous Referee #2**

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General comments: The present study underlines the complex interplay between geochemical cycles and bioturbation activity in freshwater sediments contaminated or not by uranium. The experimental approach is very competent and the results obtained clearly demonstrated the complex interactions among geochemical cycles (of N, S and Fe) and the role of worms on the biogeochemical processes. Moreover, the present paper showed both the pollutant impact on bioturbation process but also the inverse -role of bioturbation on pollutant dynamics (uranium release from sediment to overlying water in the present study)-. So, I think that this paper is a very original contribution to the field of bioturbation and biogeochemistry. Nevertheless, I have 3 main remarks and comments concerning the MS:

1- I am not usual with the DET probes (Part 2.4.2.) and I think it would be helpful to

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have a better description of the system. For example, it is indicated in the text that “gel probes consisted of plastic holders . . . with an open window... From the aperture, a series of parallel...” (Page 17008, lines 24-25). Do “the aperture” and “open window” mean the same thing? If so, it is really confusing to not using the same term in the two sentences. I think that a scheme (figure) of DET probes could be useful to understand how they work (for a reader like me).

2- The section on statistical analyses is very succinct compared with other sections. Actually, there is no precision about the design: How many times were included in RM-ANOVAs? Which treatments were compared of data by the Student’s t test on U in the water column? I suppose that the treatments with and without worms were compared but it should be indicated in the materials and methods. I have also a major comment on the use of post-hoc Fisher’s LSD tests. This test is the worst to use as post-hoc because it does not take into account the increased probability to obtain significant results when multiple tests were done on the same data set. A Bonferroni correction is often used for post-hoc tests. It is also possible to use Tukey’s HSD tests for post-hoc comparisons following an analysis of variance. So, I recommend to re-run the analyses by using a better post-hoc test than the Fisher’s LSD.

3- For the N-cycle, I regret the lack of data concerning ammonium. At water-sediment interfaces, the release of N from low-oxygenated sediment to water column often resulted from a release of ammonium. Ammonium is produced during the degradation of particulate organic matter and is not nitrified under low-oxygenated conditions. So, ammonium could be very important to explain the dynamics of N in the system. If the authors have data, it would be very interesting to have information about ammonium concentrations in the MS.

Specific comments: I also have several minor comments concerning the MS:

- Abstract, Page 17002, Line 2: “Previous studies have demonstrated that” could be suppressed from the first sentence of the abstract.

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- Page 17006, Lines 3-4: I do not think that “dead arm” can be applied to a lake. I suppose that you talk about a dead arm of the Verdon River located in an artificial lake upstream of a man-made dam.
- Page 17006, Line 7: Please replace “grosser” with “coarse”
- Page 17006, Line 23-25: This sentence is not clear. What does “a bioturbation activity though diminished that generates...” mean?
- Page 17007, Line 15: A density of 60,000 individuals per m<sup>2</sup> seems quite high for sediments with a moderate organic matter content of 2.4%. Have you references showing such kind of density in “low” organic sediments?
- Page 17007, Line 23: “any organism” rather than “any organisms”.
- Pages 17008-17009, Paragraph 2.4.2.: This paragraph could be re-worked (see my general comment 1). The last sentence of this paragraph (calculations were made based on the assumption of gel strip dimension homogeneity) is not clear. Did you use a diffusive model between gel and pore water to calculate the concentrations of ions in pore water? It should be indicated in this part.
- Page 17010, Lines 14-15: How did you evaluate the subsurface porosity with and without worms? Did you make measurements or did you use literature data? -Page 17012, Statistical analyses: See my general comment 2.
- Results: At several places, it is indicated that you obtained P-values of 0.000. I think it means that you had P-values lower than 0.0001. If yes, I would prefer to have  $P < 0.0001$  in the text. This is more correct statistically.
- Page 17014, Line 22: Please indicate if your results concern “dissolved uranium profiles” or “total uranium profiles”.
- Results: In this section, you used both past and present tenses. Please use the same tense in all this section to be consistent.

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- Page 17016, Lines 12-13: The term "undisturbed" aquaria means aquaria without bioturbation. It would be probably clearer to use the term "non-bioturbated aquaria" in opposition with bioturbated aquaria (used on Line 13).
- Page 17017, paragraph 3.3: You calculated a bioconcentration factor (BCF). However, BCF apparently does not depend on exposure time? Will BCF be comparable if the duration of the experiment was longer?
- Page 17018, Line 7: The sentence "It is thus important to rapidly discuss these results." could be deleted.
- Page 17018, Line 18: "In accordance with previous works reported in the literature,". Please add references here.
- Page 17020, Line 14: Please replace "higher" with "high".
- Page 17020, Line 23: Please replace "enhanced of 10%" with "enhanced by 10%".
- Page 17022, Lines 13-14: "Nevertheless ... denitrification." This sentence needs more explanation to be understandable.
- Page 17022, Line 25: What does "capable of conserve energy" mean?
- Page 17023, Lines 21-22: "depth of maximal ingestion rate (2 cm)"How did you determine this depth of 2cm?
- Page 17024, Line 15: DOU? Please, replace with "diffusive oxygen uptake".
- Page 17026, Conclusions: You indicated that worms influenced U fluxes through upward bioconveying of sediment particles. Is it possible that the biogenic structures produced by worms in sediments and the worm displacements could also have influenced water exchanges between pore water and overlying water, leading to U fluxes similar to those observed?

References: Please check the reference list because I found an error for Krantzberg

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(1985). This paper has been published in Aquatic Sciences and not in Environmental Pollution.

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**BGD**

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