**Biogeosciences
Discussions**

# *Interactive comment on* "Groundwater and nutrient discharge through karstic coastal springs (Castelló, Spain)" *by* E. Garcia-Solsona et al.

**Anonymous Referee #1**

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This paper deals with a timely topic that is clearly suitable for BG. The authors report an interesting dataset for a groundwater dominated coastal system. I have no issues with their sampling strategy and analytical approaches, but I feel that their discussion can be significantly improved and their calculations have to be revised (or clarified) before publication. Below, I describe my major concerns and later I list some minor points:

1) My main problem with many recent SGD papers is that the assumptions are not discussed, validated, or checked. For example, on page 640 (line 26) and page 641, the authors justify an important assumption (constant groundwater endmember concentration) by simply saying that this is a "...a common assumption...". Recent papers by Matt Charette's group show that radium isotopes in groundwaters can be highly variable. The authors have performed seasonal groundwater samples and it seems

that they might be able to use a temporally-variable groundwater endmember in their calculations.

2) What about the assumptions about coastal water residence times? After examining the data in detail, I find difficult to agree that equation 5 can be useful in the system under investigation. As acknowledged by the authors on page 642, the residence time equation relies on the more rapid decay of radium-224 relative to the other isotopes. The authors estimate a residence time of about 2 days – if this is correct, one should be able to see a curvature in the scatter plots between radium-224 and the conservative variables. However, the scatter plots in the paper show perfectly linear relationships between radium-224 and all the other variables (salinity, radium-228, radium-223, silicate). These linear correlations imply that mixing is much faster than radium-224 decay. In other words, no detectable radium-224 decay occurs within the plume under investigation, which would prevent equation 5 from being applied. The use of an equation that relies on decay in a system where radium-224 does not seem to decay has to be reconciled.

3) Still related to the comment above, Equation 5 was used only for Ra-224: Ra-223 ratios. What would happen when applying the equation to Ra-224 : Ra-228 and Ra-224 : Ra-226 ratios? The analytical uncertainties in the measurements of radium-223 are much higher than the uncertainties associated with measuring the long-lived radium isotopes, so I do not understand why the authors chose to use only 224:223 ratios. Is that why a >2-day residence time was estimated? From the linear trends in the data, I suspect that residence times would approach zero if using ratios between radium-224 and the long lived isotopes.

4) I also believe that the authors could have explored more the recent SGD literature. For example, they are dealing with a site dominated by point groundwater sources, while most of the recent literature is based on sites dominated by diffuse groundwater sources. How point and diffuse groundwater sources determine the relative contribution of fresh and saline SGD? What about conservative versus non-conservative

nutrient behaviour? And radium sources? Matt Charette's (I am not part of his group!) recent papers showed that radium is originated primarily from saline SGD, while the authors found a strong radium signal in a system dominated by fresh SGD. I found that remarkable. Bill Burnett's FSU group also has some recent papers on fresh versus saline SGD and non-conservative nutrient behaviour in systems dominated by diffuse groundwater sources. These papers have to be explored.

Minor comments: 1) I suggest using "saline" rather than "salty" throughout the paper.

2) Page 633, line 21: "hydrological modification...". I found this statement confusing. What is meant by hydrological modification and how nutrients can interfere with local hydrology?

3) Page 634, line 8: "important reverse" – please clarify or rewrite.

4) Page 634, line 11: "subterranean aquifers". Redundant. All aquifers are subterranean.

5) Page 634, line 25: "...can be only...". I understand what the authors want to say here, but there are a number of other ways to estimate residence times (models, tidal prism, etc). Please revise. Radium is probably one of the most unconventional (though powerful!) ways of estimating residence times.

6) Page 637, last paragraph: the authors compare a range in groundwater concentrations to an average in surface waters. Please be consistent (show both averages) so that the reader can have a better feeling about the real radium enrichment in groundwater.

7) Page 638; line 7: Delete "but also"

8) Page 638: The authors make some comparisons to other sites. While this is useful information, this probably belongs to the discussion section.

9) Page 639, line 19: there is no "bad" or "good" correlation. Use "not significant" or

"significant" correlation.

10) Equation 3, 4 and throughout the paper: Why use the subscript "std" to refer to coastal waters? Probably better to use a more obvious abbreviation such as "cw".

11) Page 640, line 24: here the authors use average concentrations; later, in order to calculate residence times, they use equation slopes rather than averages. Please be consistent or justify why making different choices.

12) Page 641, first paragraph: Hard to follow. Are you using a constant groundwater endmember or not? This comment is related to my first major comment.

13) Equation 5: The authors cite Moore, 2006b, but the equation does not appear in this paper. I feel they refer to Moore, W. S. 2000. Ages of continental shelf waters determined from 223Ra and 224Ra. Journal of Geophysical Research 105: 117-122.

14) Section 5.3: This section (and the SGD rates derived from here) needs to be revised as the equation results do not seem to be internally consistent with the diagrams (see major comment above).

15) Section 5.4: This section could be explored further using the literature (see major comment 4 above). I believe that just reporting numbers without discussing uncertainties and putting the results into perspective has limited usefulness.

16) Page 643 shows a very interesting analysis.

17) Page 644, line 8: reads a little confusing, perhaps because the sentence is too long. Please revise.

18) Page 644, line 13: "...strictly correct...". Their approach will be valid if nutrients are conservative, not necessarily because samples were "located close to the water edge".

19) Page 644, line 16: "new + recirculated" . Isn't the paper dealing only with springs? If so, all nutrients should be new. If recirculated seawater is a source of nutrients, one

would not expect to observe clear linear mixing trends. Or am I misunderstanding the flow of information?

20) Page 645, line 8: Denitrification in groundwater or seawater? This passage may have to be revised. The linear relationships imply no nitrogen transformations (such as denitrification) along the transport pathway.

21) Language: in general fine (compliments to the authors), but some typos and inaccuracies can be revised and the text can be polished. Some paragraphs are very long and can be shortened.