

Interactive comment on “Time scales of regional circulation of saline fluids in continental aquifers (Armorican massif, Western France)” by A. Armandine Les Landes et al.

Anonymous Referee #2

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The Armandine Les Landes et al. paper has some interest in the area of displacement of connate waters from groundwater basins. Its greatest interest lies in its fitting of an empirical equation (2) to average basin chloride concentration related to time since marine transgression, and then exploration of implications of tau ($\bar{i}A_t$), labeled by the authors as residence time. While this is the most interesting part of the paper, it could be more strongly evaluated. A lot more evaluation and interpretation of this equation, consideration of sensitivity and error in fitting the curve to the 3 data points, discussion of the meaning of tau ($\bar{i}A_t$), and comparison to analog solutions, would make this a very interesting paper. As it is, a long and thorough effort is made to quantify and

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justify the three average values for Cl(time) plotted in figure 7 to which equation 2 is fitted. The derivation and documentation of the three Cl(time) data points could be condensed. More discussion is needed of how equation 2 was fitted to the 3 points, and the meaning of the parameters. Without such a change in balance, this reviewer finds the paper interesting but weak, more descriptive than interpretive.

Most of my technical suggestions pertain to the issue about the meaning of equation 2, its fitting to the data, and the meaning of its parameters. I accept the derivation of the Cl(time) data from the basin analysis as given.

1. More explanation is needed to justify and explain the value of C_{in} parameter. It seems that was determined from the fitting of the equation to the empirical data (3 points). If that is correct, the “fitted” C_{in} value seems poorly constrained. What range of values might a sensitivity analysis have determined to be likely? Could a probability distribution be roughly assigned to C_{in} value. Do the authors have an a priori reason to believe that C_{in} should be close to 100 mg/L?

2. An interesting implication of the reasoning is what would have been the Cl concentration in the aquifer at the end of the transgression. I believe there is little in the literature about this question; did the authors’ literature review find anything regarding seawater mass emplacement? When there has been a major marine transgression lasting XX millions of years, would the subsea aquifer have been flooded by seawater? If so, would not the C_{in} value be Cl= 19,000 mg/L? The authors should expand their reasoning for why the initial post-transgression aquifer Cl would have been two orders of magnitude less than seawater salinity. Do they have a quantitative explanation for why emplacement of seawater under prolonged transgression would have been incomplete?

3. The authors mostly call tau ($\bar{i}A_t$) “residence time.” I am not sure it is residence time (or storage time), as usually defined [(total mass of solute in basin)/mass flux]. tau ($\bar{i}A_t$) seems more like a half-life, a rate constant, but not exactly analogous. This

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meaning needs to be more carefully and thoroughly considered. It is the crux and most interesting part of this paper! Implications: how might tau (τ) differ between basins? What physical parameters and evolution of paleohydrologic boundary conditions might affect the distribution of tau (τ) among aquifers?

4. Three field-based data points makes for a weak fit of an exponential equation. In my comment 1 above I hint that additional discussion on uncertainty in the fitted equation is needed and appropriate. But there is another source of information that the authors could use, which comes from analytical (easily accessed or redone) and numerical models (no so available). For example, Domenico and Robbins (1985. The displacement of water from connate aquifers. GSA Bulletin 96:328) and surely others since define analytical solutions for a similar problem. The authors could adapt such an approach, and then "sample" the domain through time and generate many more than three data points. Wouldn't that comparison be worthwhile (and relatively easy)?

Other comments:

1. Is leaching of marine waters best term? Displacement? Does leaching have specific content not relevant to this hydrologic context?

2. P. 6601 versus p. 6605. Is there any reason to assume that there were no transgressions older than Mio-Pliocene that could have emplaced seawater in these ancient rocks. No sedimentary record of older transgressions. Or only the most recent ones count (p. 6605). Might be worth addressing more clearly.

3. P. 6607 (6.1). Both 'salinities' (60 to 1400 mg/L) and 'chloride concentration' are used but it is not clear that the authors are not treating them as synonymous. Is the meaning of salinity = total dissolved solids as used?

4. P. 6608. There are many other sources of chloride than the 3 listed. The 3 might be the only ones relevant to this study, but the others should be recognized. The most obvious are solution of halite (there are no evaporates in the basins?) and evaporatively

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concentrated brines or other formational brines at depth.

5. P. 6612 "All previous studies" is a vague reference. Is that all as in ALL, including the whole literature cited and not cited in this paper? Or does it refer to just the papers cited in this paper? Could be made explicit by reciting the relevant papers.

6. Figure 6. I don't believe the first derivative adds anything to the paper and that part of the figure should be deleted.

7. Figure 3. Is there something mislabeled? Caption cites 'characteristic sediments' but the explanation and map do not appear to indicate sediment information. Do the three symbols (triangle circle square) represent age of sediment? If so, caption should state 'age of sediment' and not 'characteristic sediments.' The latter would suggest shale, sandstone, etc.

8. Figure 5. Because there is a 3D distribution of chloride concentration, I am not sure the value of displaying this information on a 2D maps. I suggest deleting this figure; coastlines are in figure 3. Authors should explain to the editor justification for keeping figure 5.

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