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Interactive comment on “The low temperature hyperalkaline hydrothermal system of the Prony bay (New Caledonia)” by C. Monnin et al.

Anonymous Referee #1

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Evaluation of manuscript “The low temperature hyperalkaline hydrothermal system of the Prony bay (New Caledonia)” by C. Monnin and others

The paper reports new and interesting fluid compositional data for the Bay of Prony alkaline springs, which are of considerable interest to a growing group of geochemists and geobiologists. The main conclusions of the paper are:

1) fluids reflect mixing between seawater and alkaline groundwater 2) brucite precipitation starts at $\text{pH} > 9.5$, when a critical supersaturation threshold is reached 3) carbonate supersaturation peaks at $\text{pH} 10$ and fluid mixing plays a key role in carbonate formation

Conclusions 2 and 3 are not supported very well by the way the data is reported. Brucite saturation state is not plotted (see below), so it is difficult to verify what is

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stated in the text. Brucite precipitation suggested to occur at $\text{pH} > 9.5$ would result in non-conservative behavior of Mg. However, Mg looks like it behaves conservatively in Figure 5. This needs to be investigated in greater detail, perhaps in a blow up diagram that examines the relation of Mg vs. Cl at low concentrations. Likewise, sulfate appears to behave conservatively. Does this mean that all the mixing takes place during sampling? Or is sulfate indeed not consumed? Is there any evidence for increased sulfide? There is certainly enough hydrogen dissolved in the alkaline waters to drive sulfate reduction. Also, from the published work (Pisapia et al.) what metabolic reactions would one expect to take place and are these reactions reflected in the composition of the vent waters?

No attempt is being made to match the fluid chemistry with precipitate mineralogy in order to test the hypothesis that the highest supersaturation in the mixed fluids drives carbonate precipitation. It would be interesting to learn more about the carbonate mineralogy, which apparently has been worked up in related studies.

Gas concentrations should be reported for the individual fluids. Just reporting averages in Table 3 is not very helpful. The authors state that Eh is controlled by hydrogen. I don't think this is correct, because the Eh-sensor is fairly insensitive to dissolved hydrogen. It would actually be interesting to see $\text{H}_2(\text{aq})$ concentrations computed from the partial pressures of H_2 in the gas phase. I am pretty sure that those H_2 concentrations would correspond to Eh-values that are much lower than the measured ones. The slope in the Eh-pH diagram is also too steep of $\text{H}_2(\text{aq})$ control. The authors may want to speculate about which redox reaction would correspond to the trend displayed in Figure 4.

A more detailed comparison between the endmember compositions of the waters with those from CaOH-type water from other sites would be helpful.

Figure 3 shows a positive correlation between temperature and pH. I don't think the two usually correlate with each other. Mixing it proposed to be responsible for the correlation, but I would not expect a straight mixing diagram in a log-lin diagram.

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In general, I think it would be really helpful (in particular when looking at Fig. 3 and, more so, Figs. 6 and 7) if the authors computed conservative mixing lines between seawater and the BdJ spring endmember.

I think that the authors could improve the significance of this communication a lot if these comments were addressed in a revised version

Other quibbles:

The term “Ca-OH type water” is used in abstract but nowhere else in the manuscript. Please explain the term in the introduction or omit using it in the abstract.

Figure 7: What do the dashed lines represent?

Figure 8 and 9 obviously plot the same data. I suspect both show calcite saturation state. It is hard to imagine that brucite is oversaturated in pH 8.5 fluids.

Figure 11: Why do the flow paths for the ST11 fluids cross the basal thrust? Is there any evidence for water interaction with continental crustal rocks?

Table 2: Sample W8 from ST12 is obviously seawater. How can it have only 0.4 mmol/kg DIC. Is this a typo?

Neal and Shand (2002) and Fujii et al. (2010) references Boschetti and Toscani (2008) in Table 1 are missing from list of references

Quéméneur et al. (2014) is also not in the list of references

1. Does the paper address relevant scientific questions within the scope of BG? yes 2. Does the paper present novel concepts, ideas, tools, or data? Novel: no, interesting: yes 3. Are substantial conclusions reached? Perhaps after paper has been revised 4. Are the scientific methods and assumptions valid and clearly outlined? Yes 5. Are the results sufficient to support the interpretations and conclusions? Partly 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Not clear what complexes

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were omitted in the computations of saturation state: only organic complexes? 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes, mostly. 8. Does the title clearly reflect the contents of the paper? Yes 9. Does the abstract provide a concise and complete summary? Yes 10. Is the overall presentation well structured and clear? Yes 11. Is the language fluent and precise? The manuscript would benefit from editing to improve clarity. 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? yes 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See comments 14. Are the number and quality of references appropriate? yes 15. Is the amount and quality of supplementary material appropriate? N/A

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