

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
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Referee Comment on hess-2021-287

Anonymous Referee #1

Referee comment on "Evidence for high-elevation salar recharge and interbasin groundwater flow in the Western Cordillera of the Peruvian Andes" by Odiney Alvarez-Campos et al., Hydrol. Earth Syst. Sci. Discuss.,
<https://doi.org/10.5194/hess-2021-287-RC1>, 2021

General Comments

This is an interesting study that helps inform our understanding of recharge, groundwater flow, and spring origins. However, it needs attention before it is suitable for publication.

Firstly, the paper needs better focus. Some sections (e.g. the discussion of geochemical processes) do not seem to inform the overall story and are a distraction. The description of the study area is long and some of the information does not seem to be relevant. There are also descriptive sections in the discussion and a tendency to repeat information. A more succinct tightly-written paper would be easier to follow and have more impact.

More importantly, in several places the discussion of processes is not convincing (inc. the geochemical processes, groundwater residence times, and lapse rates). The explanations tend to be long and are sometimes not consistent or are overly speculative. The central point that the springs are recharged at altitude and the water follow different flowpaths appears reasonable, but this sometimes is lost.

Finally, the Conclusions are mainly parochial . Some idea of the broader significance of this study or comparisons with similar environments would give the paper a broader appeal.

I hope that these comments are useful.

Specific Comments

Abstract

The abstract gives a good idea of the major conclusions of the paper. However, it needs more focus on the results and conclusions rather than the aims. You should report a few key values in the Abstract as qualitative descriptors ("stable", "higher" etc) do not convey much specific information.

Introduction

The introduction is clear and well structured. The first paragraph (lines 40-55) would benefit from a few extra details if these are available, specifically:

- Are the impacts on groundwater resources quantified?
- Likewise are there estimates for how much the recharge may decline?

This would help with understanding the context of this study.

Lines 57-65. Provide some references for this material.

Lines 73-79. This is only true if there is no other mechanism for exporting Cl. In some saline lakes Cl is lost via salt deposits being eroded by the wind (deflation). Is that the case here? If the lakes do recharge the local groundwater system, I would expect there to be some evidence of that (shallow high salinity groundwater around the lakes) – any evidence of that?

Study Area

This is comprehensive, but in places the descriptions are lengthy.

- There is some repetition with the introduction (eg lines 119-129).
- Some of the details of the geological history (lines 137-148) seem superfluous
- The climate description (Section 2.3) could also be more succinct – the important details are a bit lost in the narrative
- Again with Section 2.4, how much of this detail is really necessary

The lengthy descriptions mean that it is not always clear what the important points are. Try to give more focus to what is important for the study rather than trying to cover everything.

The order is also not intuitive – Sections 2.2 and 2.4 both deal in part with the Geology and Geomorphology. It would be better to merge these two (or at least make them sequential) and to present the material in a large- to smaller scale order. So the descriptions of the large-scale geology (lines 200-207) would be better presented before the description of the basin (Section 2.2).

Figures 2 & 3 can be merged – they are maps of the same area. That way it would be easier to see the relationships between the samples and the faults. Also, it is not currently clear from Fig. 2 what is sampled (springs, surface water etc) without referring to the Table – make the symbol for each different.

Methods

Here also there are some diversions (eg Lines 261-265) that are distracting and do not belong in the methods. If this information is important, then it belongs in the study area section.

Lines 270-272. Really it is only SpC that is measured, the TDS is just calculated from the SpC using an assumed conversion. If you have a full suite of ions (which it looks like you do), you can calculate TDS as the sum of the ions. If you stick with the TDS estimated from SpC, you need to note that and provide the conversion.

Section 3.2. A few more details on the Tritium procedure (enrichment and equipment) needed

Section 3.3. Need to report precision for the major ions

Results

As explained below, this section loses focus in places and it is not always clear what the important points are. You need to be clearer as to what information is important for this study and concentrate on that. The figures need improvements -duplicating the figures just to show the two Salinas samples is excessive. It is clear that these are evaporated surface waters and you do not say much more about them than that. In that case just omit them from the figures and note that in the caption.,

H isotope ratios should have no decimal places to be consistent with the quoted precision (O is fine with one decimal place)

Lines 314-317. Report the low ^3H activities as bd without the +/- (which have no meaning for ^3H below the lower detection limit). If the precision is 0.1 TU (Section 3.2) then the +/- of 0.04 for BED is overoptimistic. Given that this is very close to the lower detection limit, how sure are you that this is real. Given that you sampled the springs at their surface outlets, could it just be a small amount of modern water (eg recent rainfall) mixed in with the spring water?

Figure 4. The two diagrams are a little confusing as they present the same data. If you feel that you need 4b to show the detail, you could make 4a in inset with just the Salinas surface water on it. Also need to reference the LMWL in the caption.

Figure 5. Again, you do not need both figures – you can just use Fig. 5b and explain that you have omitted the Salinas waters. It would be useful to have Fig. 4 & 5 as a single figure as it would show the relevant stable isotope data in one place.

Section 4.3

This section lacks focus. What is important here (the processes or the differences in chemistry). The discussion of the processes using the Gibbs Diagram is not very convincing and could be done better. however, consider whether that is important. If the important point is that the waters have different geochemistry and so follow different flow paths, then just show that (the Piper and/or a couple of bivariate plots like Fig. 10, and a brief description would suffice). The discussion of processes is not very clear and may not be necessary.

The Gibbs Diagram in particular is not very informative in determining processes and you have the data to do that more rigorously. Waters dominated by evapotranspiration have Cl/Br and Na/Cl ratios close to those of rainfall (which you can probably estimate).

Extensive rock weathering produces high cation/Cl ratios while halite dissolution produces very high Cl/Br ratios. A few bivariate plots (eg Na/Cl vs. TDS and Cl/Br vs. TDS) would show that much better than the approach that you are currently using. There is extensive literature on this (eg numerous groundwater papers by Mike Edmunds).

Lines 386-394. I presume that this also shows up in the other parameters? I'm not sure that you have enough data to do a PCA or cluster analysis but you should make the point with the other parameters.

Discussion

The Discussion covers a range of topics but there are a number of potential inconsistencies and unclear explanations. This is the most important part of the paper, so more clarity would help. I also suggest that you add a couple of sentences to the start of the Discussion as a guide to what you will be dealing with.

Sections 5.1.1 & 2.

The discussion regarding the lapse rate (lines 415-434) needs to be clearer. While it is true that springs can have stable isotope ratios that vary with altitude that is mainly the case where they are recharged close to where they discharge. In the case of your springs, you make the case (Section 5.1.2) that they are recharged at high altitudes. That interpretation is reasonable. However, the way that this discussion is presented is to set up the idea that the springs should vary with altitude (line 416) and then point out that that is not the case and then interpret the data in terms of recharge altitude in Section 5.1.2.

Much of Section 5.1.1 (the correlation with altitude etc) contains observations that should be part of Section 4 – some of it is in there already and it is just repeated here.

The magnitude of the lapse rate. If the springs are recharged at higher altitudes then you can't use them to estimate the lapse rate. In that case, your lapse rate should be based on the surface water samples. However, those data may not be suitable, specifically:

- Rainfall sampling is referenced to an unpublished study. It is not clear how many samples this represents and the duration of the rainfall record. Given the likely variability of rainfall isotope values, you ideally would have a multi-year weighted average value, but is this the case?
- Snow is probably mainly winter precipitation and is difficult to use with samples that

represent long-term averages.

- The surface water samples seem to be partially fed by spring water (Section 5.2). If those springs were recharged at high altitudes then using river water to calculate lapse rate is possibly not valid as it is not capturing only rainfall at the altitude where you sample it.
- A similar concern would apply to any rivers that flow from high to low altitudes and thus mix rainfall from a variety of altitudes
- Even if the rivers are mainly fed by local rainfall, their stable isotope values are likely to vary seasonally (as you discuss in Section 5.2) and so again are difficult to use in this way.

I'm not convinced that you can determine the lapse rate with the data that you have. If you are going to include this discussion, it needs to be more convincing. Otherwise you may be able to estimate it using other studies?

Spring recharge elevation. This seems broadly correct; however, it becomes more doubtful if there are palaeowaters in the basin (lines 463-465). Discharge of paleowaters into surface water bodies also complicates the lapse rate calculations. Is there anyway to test this idea? While you do not have radiocarbon data are there examples of palaeowaters in analogous settings or examples of nearby springs for which there are better residence time calculations?

Section 5.3 also needs attention.

- It is not clear where the ^3H activity of "young aquifers" of 2.9 TU comes from
- The definition of fossil water as being >60 years is largely a northern hemisphere viewpoint as the higher ^3H bomb pulse waters are still detectable in groundwater. This is not the case in the southern hemisphere where the bomb-pulse tritium has decayed back to natural levels (e.g., Morgenstern, U., Stewart, M.K., Stenger, R., 2010. Dating of streamwater using tritium in a post nuclear bomb pulse world: Continuous variation of mean transit time with streamflow. *Hydrology and Earth System Sciences*, 14, 2289-2301; Tadros, C.V., Hughes, C.E., Crawford, J., Hollins, S.E., Chisari, R., 2014. Tritium in Australian precipitation: A 50 year record. *Journal of Hydrology*, 513, 262-273).
- The residence time of 300 years seems arbitrary. Presumably it is based on mixing at the top of the aquifer but you have a fractured flow system that is likely to behave very differently.

As you have only three tritium measurements (all of which are close to or below detection) and you do not have a good idea of the rainfall values (2.5 to 10 TU is a large range), there is little quantitative that you can say here and this section is not that informative. I'd just make a case for the water being at least a few decades old in Section 5.4.

Section 5.4

Lines 517-520. I'm not sure that I'd expect Cl to increase along flow paths. To do so implies that Cl needs to be added from the rock matrix as evapotranspiration is a surface process. That will only occur if there is halite in the rocks (which is not that common). This concept does appear in many textbooks but the supposed process is never really explained.

Lines 517-525. You look to have measured Br. Cl/Br ratios will readily determine whether you have halite dissolution (Cartwright, I., Weaver, T.R., Fifield, L.K., 2006. Cl/Br ratios and environmental isotopes as indicators of recharge variability and groundwater flow: An example from the southeast Murray Basin, Australia. *Chemical Geology* 231, 38-56). You may not need to speculate here.

Section 5.5.

The first paragraph (lines 569-574) repeats the previous section and is not needed.

Figure 11 only needs one panel as you can show all three flow paths without confusion.

Conclusions

Again, there is some repetition here. Instead of repeating the specific findings, which you cover in Section 5, try to outline the general points. However, you should explain the general importance of the study or compare it with similar studies elsewhere. This will make the paper appeal to a wider readership.

