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Hydrology and Earth System Sciences Discussions

Interactive comment on "Origin and assessment of deep groundwater inflow in the Ca' Lita landslide using hydrochemistry and in situ monitoring" *by* F. Cervi et al.

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Answers to the editors questions.

1. Does the paper address relevant scientific questions within the scope of HESS?

Paper addresses relevant scientific questions.

2. Does the paper present novel concepts, ideas, tools, or data?

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Interactive Discussion
Discussion Paper

The idea of deep groundwater flow into the landslide authors are representing is very interesting and pertinent for the large landslides in Alps.

3. Are substantial conclusions reached?

Conclusions are not justified with data presented. For the rest see comments.

4. Are the scientific methods and assumptions valid and clearly outlined?

See comments.

5. Are the results sufficient to support the interpretations and conclusions?

See comments.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

See comments.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes.

8. Does the title clearly reflect the contents of the paper?

Yes.

9. Does the abstract provide a concise and complete summary?

First paragraph can be omitted.

10. Is the overall presentation well structured and clear?

See comments.

11. Is the language fluent and precise?

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I'm not qualified to judge language. However, it is clearly written and easy to read.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

See comments.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

No.

14. Are the number and quality of references appropriate?

Yes.

15. Is the amount and quality of supplementary material appropriate?

There is no supplementary material.

General comments to the paper

I agree with authors that deep originated flow of groundwater in Alps can be important in understanding of large landslides.

Regional hydrogeological conditions are precisely described and illustrated by the sketches of regional and local geology. I will expect that also conceptual hydrogeological model will be presented. In their interpretation deep water flow plays important role but they do not illustrate how this water is rising up. For the interpretation it is important to propose flow along the fault (which of them is behaving as a conduction path) or through the whole mass of bedrock under the landslide.

For reader not knowing local information there is also scarcity of information about landslide geometry. Information about the area, width, slope and aspect must be added at the beginning. Also some hydrogeological information of rocks and soil will be very valuable. These data will also enable better understanding of landslide water balance.

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I would also expect more easily scanned water balance of the landslide. I suggest representing water balance concept with the system of equations that are easily readable than plain text description.

However, based on the number of the analyses my opinion is that number of samples is too few to justify such strong conclusions as it is deep groundwater flow. In the area where they are working strong seasonality effects can be present in isotopic and chemical composition of water cycle. Only with few samples it is difficult to reliably interpret such influences. I suggest using multiple hypotheses approach.

In chapter 5.3. Groundwater isotopic analyses authors discuss results of isotope analyses. Based on the reported results I can only partly agree with their interpretation. Their data are nicely represented in the Fig. 6. Large part of the samples is positioned below the regional (Italian) meteoritic water line - RMWL. However, it is interesting that they are positioned on the same line with lesser slope than RMWL. This is typical situation of the evaporation effect. Probably such isotopic composition can be also related to the presence of connate waters in evaporates. However, such deviation from RMWL can be also the consequence of exchange of HCO3- but due to the fact that partial pressures of CO2 in the groundwater and soil at the site are rather low such mechanism is very unlikely. Authors should consult some of the classical text books of isotope geochemistry (e.g. Gat, Mook, Fritz & Fontes, Clark & Fritz, IAEA etc.). Effect of evaporation line presence must be explained and used in the interpretation of the results.

It will be also very helpful if they can use deuterium excess approach which can help a lot in the interpretation of isotope data.

For reported study I will expected some information on the isotope altitude effect which can help to discern recharge area of the water. There is no such information in the paper. Authors should stick more to the local information about isotopes in water than to regional interpretation of Longinelli and Selmo, 2003 that represent statistical (aver-

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aging) interpretation from data originating from earlier times than results representing in the paper. Authors should try to obtain results of precipitation sampling from some of the near station in the GNIP network of IAEA. Based on these data they should interpret what are expected fluctuations of isotopic composition in precipitation and consequently in local groundwater. If possible from these data they should tray to estimate also altitude effect that can help to understand recharge area.

Seasonality effect is probably present. From the Table 3 follows that range in DrA sampling point of δ18O is 1,6‰ and for δ2H is 5,6‰ that are rather high amplitudes. This can be the consequence of different end members mixing, however if such changes exist than landslide body is relatively highly permeable.

Authors also reporting tritium concentration that are not reported in the tables and they are not referenced. Data must be properly reference and analytical methods described in the chapter related to the isotopic methods. With the interpretation of tritium data one should be very careful, especially if only few data are available. In recent precipitation values bellow 5 T.U. are more probable than higher and concentrations around 10 T.U. are likely to coming from groundwater ages between 10 and 20 years, depending on the local conditions.

Samples on the evaporation line exhibit also high ion concentrations illustrating that "anomalous" isotopic composition is related to the chemical status of groundwater. These are probably interrelated. Authors should put more efforts to interpret the sources of Cl-, SO42- and B- ions. Their statement that former two are not justified with the local mineralogy is not acceptable. Based on my knowledge it seems that these ions are coming from evaporate minerals and if these hypothesis is valid the end member for mixing can be connate water from evaporate deposits. It is very unlikely that such concentrations will come form oxidation processes of sulphide minerals. Also some efforts should be put in their interpretation of the relation between Cl- and SO42- ions that will help to clear out their origin. At the same time authors should check hypothesis if some on-site concentrating processes are possible. This can happen as a

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consequence of evaporation that is unlikely here or with suction processes through the low permeable soil (sediment).

Among analyses at the disposal Cl- concentrations can be understood as more conservative than δ18O. Why is chloride not using for end member mixing model? With alternative use of δ18O and Cl- consistency of interpretation can be checked.

Some caution is needed in the interpretation of Btot concentrations. B species are not conservative as Cl- ion is. Their concentrations can be very much influenced by the mineralogy of the clay in the aquifer and consequently absorbed or desorbed.

At the end of the paper based on the mixing model conclusion is reached that deep water component is in the range of 7,800 to 17,500 m3/year. In fact this is small quantity between 2.5x10-4 to 5.5x10-4 m/s. Authors should somehow comment reliability of their water balance. The numbers of the deep water flow are much smaller than the error of the estimated evapotranspiration and surface water component that are very roughly estimated and not measured. For the latter they are even playing with estimated surface run-off coefficient.

Reviewer's conclusions. Paper represent very interesting case study important to understanding deep seated landslides in Alps. However, they have to improve the interpretation of their data in more consistent way.

Specific comments

Througout the paper all ions must be written in ionic form (e.g. Cl- and not Cl) and where concentrations measured as total reported as such (e.g. Btot). Writing without indexes for ions is not in accordance of IUPAC standards. The same is valid for the diagrams.

Through the paper in reporting isotope compositions unit for permile ‰ must be strictly used. According to the accepted professional standard in isotope geochemistry literature is not premited to write results of isotopic composition without unit. The same is

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valid for the diagrams.

Precision and accuracy of isotopic measurements should be reported. Based on these data numbers in Table 3 must be properly rounded and interpreted in the text.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7699, 2012.