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Interactive comment on "Pore-water in marine sediments associated to gas hydrate dissociation offshore Lebu, Chile" *by* Carolina Cárcamo et al.

Anonymous Referee #1

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Carcamo et al in the manuscript "Pore-water in marine sediments associated to gas hydrate dissociation offshore Lebu, Chile" use bathymetric, sedimentological, foraminiferal and isotopic measurements to identify and explain the formation of a positive relief along the Chilean margin. The combination of the different approaches – including theoretical modeling - leads the authors to conclude that the positive relief identified is the result of mud growing processes associated with gas hydrate dissociation in a specific region where cold seeps occur in previously identified faults and fractures.

General comments

As a regular reader of HESS papers, I feel like the paper by Carcamo et al does not fall into the scope of HESS journal. Although occurring at a margin which is by definition at

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the interface between continents and oceans, the scope of the paper is in my opinion more related to marine sedimentology and/or marine geochemistry. This feeling is confirmed looking at the references used in the introduction that come mostly from journals that deals with geological, marine and solid earth issues. Maybe the paper should be submitted to a journal that deal with these topics rather than HESS. I let the editorial board and the associated editor handle this question. Should this paper be published to HESS, I would like to see the following points be addressed in a revised version of the paper.

My first concern is related to the introduction of the paper. The introduction is too short and does not state properly the general context of the study and the research questions tackled. As a continental hydrologist, I wonder what fluid escapes and positive relief are used for. For what reasons should these systems be studied and identified? These critical points should be clearly explained in the introduction so that the reader can figure out the novelty/added-value of the research presented in the paper. It is very hard to understand if the identification of a positive relief and the explanation of its formation is a major research challenge or not. Moreover, it seems that no new methodology regarding theses question is proposed and that rather classical approaches were used. In this context, it is hard to state if the research presented is worth being published as it is.

The overall quality of the paper should also be improved as the description of the methods and the results are too short, lack precision and are sometime of poor quality. For instance, foraminiferal information is used to better understand the processes producing the positive relief. In the methodological part, it is not explained to what extent and how foraminifers can be used to better interpret these processes. Moreover, the text and figures are not always in agreement (see specific comments below). The conclusions drawn from the measurements of the isotopic composition of pore-water is not clear enough and should be discussed in greater details (see specific comment below). The same goes for the foraminiferal part.

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I really like the fact that the authors have used a theoretical model to explain the processes responsible for the formation of the positive relief. I find this approach very valuable for the paper.

Specific comments:

- Line 41: “have been reported worldwide” - Line 82: “in the framework of a project entitled. . . sedimentological, geochemical and bathymetric studies . . . were performed”. Please end the sentence with a point. - Please define what mbsl means here. - Line 135: I don’t understand what the authors mean when they say that they measure pore water (w%). The unit is confusing. Even more confusing as the term water content is used when commenting Table 1 (line 162). - Line 164: TOC in the text and MOT in Table 1 and Figure 4. Please correct. - Line 174: this is not a sentence. . . - Line 193: pH is not shown Fig 5. - Comments on Figure 5 (line 186 to 189): There is indeed a trend to positive values but in my opinion, it is not as evident as the authors state. The authors also state that the delta 180 reaches a value of 6. This is more than questionable as (i) only the last point reaches this value and (ii) this point is clearly out of the trend. This point needs to be discussed thoroughly. - Line 237-239: Can this assertion be verified or confirmed by other measurements/approaches? - Line 261-271: These concluding remarks should be improved.

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