

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

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Answers Reviewer 2

1) While this manuscript intends to provide evidence supporting the assumption that the relief along the Chilean margin (as shown in Figure 2) is related to gas hydrate dissociation, the data presented essentially fails to do so. Data and information on pore water, sediment and foraminifera do not support the assumption that gas hydrate or methane emissions may actually explain the formation of such a relief. The main conclusions, therefore, seem to be rather far-fetched – at least in the current form of the manuscript.

Thank you for your comment. We had better support our hypothesis providing more

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details and better underlying that different pieces of evidence like geochemical, biological and geophysical arguments point out to gas hydrate dissociation. Our hypothesis about mud growing related to gas hydrate dissociation is strongly supported by: a) The benthic foraminifera presence that has proved to be a good indicator of methane releases (Sen Gupta and Aharon., 1994; Rathburn et al., 2000; Hill et al., 2003; Torres et al., 2003) b) The enrichment isotopic composition of O18 and D. c) The theoretical modelling. d) The gas hydrate presence in the study area, as indicated by the BSR presence reported in Vargas-Cordero et al., (2010a, 2011).

2) Several aspects, such as the use of foraminifera to explain a relief);

We include new sentences related to Foraminifera in the Introduction and discussion in order to explain its relation with our hypothesis.

3) the integration of water samples data with sedimentological data, need further clarification and proper interpretation.

Regarding the water and sedimentological samples were analysed in order to evaluate the gas hydrate presence. In fact, sediments in presence of gas hydrate affect the physical-chemical properties at the interface seawater-subbottom. We improve our Methods and discussion including more detail.

4) Other open questions relate to the location of the sulphate-methane-transition zone (SMTZ);

Thank you for rising this point. We are aware that it might be possible to associate the process occurring inside the SMTZ with mud growing, but our measurements and analyses do not support us to confirm this. It means, we did not measure sulfate, methane, sulfate reduction or AOM (anaerobic oxidation of methane). After saying that, our hypothesis is strongly supported by BSR (Fig. 7), isotopic composition (O18 and D), foraminifera taxa, and static modelling. To highlight the gas hydrate presence in the study area, we added a new figure (Fig. 7) where the BSR was identified.

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5) when was the relief actually formed (if really related to gas hydrate)

Radiometric dating analyses were not part of our goals; however, our hypothesis is strongly supported as previously explained.

6) note that the sediment core used to conclude on the presence of gas hydrate appears to be located (too) far off the relief to be really considered as a relevant and reliable indicator

Our sediment core is located near (100 m) to relief, also the study reported by Rivero (2018) shows sediment cores on the relief with similar results. We include new sentences in the Discussion in which the data of Rivero is referenced.

7) are there any indications of a gas hydrate reservoir in the seismic data?

We included a new Figure (Fig. 7), in which we reported the bottom simulating reflector (BSR) as identified in a seismic line (SO161-44) analysed by Vargas-Cordero et al., (2010a, 2011). In fact, the BSR indicates the base of the gas hydrate zone and the top of the below free gas zone. So, Fig. 8 underlines the presence of gas hydrate in the study area, as already detected and analysed; consequently, the Discussion was coherently modified and updated.

8) Some of the aspects and open questions might benefit from further information in the methodology section of the manuscript. For example, there is a lack of information on the micropaleontological methods used and the exact origin of the results/data used in this study.

Thank you for your comment. We included in Methods and Discussion information about foraminifera analysis.

9) For stable isotope measurements, no errors are given, nor the standards used.

Additional information was added to the reviewed version. Nonetheless to clarify this point, We will add the following information “Oxygen and deuterium water isotope anal-

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yses were evaluated using LIMS (Coplen and Wassenaar, 2015) and normalized to the VSMOW-SLAP scale and reported as δ -values for oxygen ($\delta^{18}\text{O}$) and deuterium (δD). Each sample was measured at least twice in different days. For each measurement, samples were analysed for five consecutive times. Results are accepted if the standard deviation of every single run (composed of five repetitions) is $<1\text{‰}$ for δD and $<0.1\text{‰}$ for $\delta^{18}\text{O}$. Thereafter, the accepted stable water isotope value of a sample will be the average of the (at least) two different valid measurements within the range of the previously explained standard deviation.”

10) There is also needed further information on the theoretical model used in this study.

We included details about the method used to evaluate the theoretical BSR depth, as required.

11) The references used in the discussion chapter should be updated with more recent and relevant publications.

We included new references and the reference list has been updated as required.

12) In its current form, this contribution has major shortcomings that would have to be addressed before any further re-consideration for publication in this journal. The topic, data presented, results and conclusions furthermore are actually quite far from the actual main scope of the HESS journal. The authors would have to do an additional substantial effort to have their contribution really target the audience of HESS.

We improved our article, modifying Introduction, Methods and Discussions in order to make our manuscript more attractive to the HESS audience.

13) On the technical side, the manuscript also suffers from quite substantial shortcomings. There is quite a large amount of typing errors and poor English language that need to be improved. The quality of some figures (especially Figure 6) also needs to be improved.

We improved the text correcting shortcomings and the figures as suggested.

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14) Specific comments:

Line 44: when referring to biogenic and thermogenic methane gas, please note that thermogenic is also biogenic, as it stems from organic matter degradation. An alternative would be to use the term 'microbial' rather than 'biogenic'. We edited as suggested.

Line 45: Conditions proper for gas hydrate formation need to be further explained in this part of the manuscript. We added a new sentence in Introduction as suggested.

Lines 54-56: Note that the organisms listed here cannot be considered as biological indicators of fluid escape per se. We included more information regarding foraminifera taxa as an indicator of methane release.

Lines 56-59: Please specify in which stable isotopes the pore water has been enriched. We included more details as well as the values of O18 and D.

Line 89: "located around a positive relief" is not clear. Please be more specific. We modified the sentence by giving more details.

Lines 93-99: Please develop on how the water samples collected in the water column are related to sediment features? We improved our discussion including information about relationships between the water column and sediments. Also, we included a new sentence in Methods in order to clarify relationships between seawater and sea bottom.

Line 175: Foraminifera does not have gender (genus instead?). We changed as suggested.

Line 185-186: The data shown here do not support gas hydrate dissociation. The analysed core is located too far from the relief. Our sediment cores are located near (100 m) the relief. We included details about a recent study reported by Rivero (2018) that reports sediment cores on the relief with similar results. We included new sentences in the Discussion in order to underline this point.

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Line 188. In Fig. 5a there are not values up to 6%. We specified the values in the Results and Discussion.

Lines 194-202. In this section, statements are rather confusing and need to be reformulated. We completely modified the text as suggested.

Lines 203-208: Since methane is not metabolized by foraminifera, they cannot be used for discriminating between seeping and non-seeping sites. We specified the foraminifera taxa reported as an indicator of gas release.

Line 220: geothermal gradient to be expressed in 0C/km not km/0C. We corrected the mistake.

Lines 240-247: From the current developments, it is not clear how grain size data fits into this paper and how it can serve to sustain the main hypothesis. The grain size is related to the geological context that promote the hydrate formation. Gas hydrate is believed to exist in various forms within muddy layers or embedded within the pores of sandy layers (i.e., Waite et al., 2009; Hyodo et al., 2013). A comprehensive database of measurements for hydrate-bearing clay, silt, and sand at different effective stress and hydrate saturation levels is documented by Lee (2007). Moreover, the grain size can be associated with flow hydrodynamic conditions, in which mud and sand could be related to coastal and beach systems, fluvial or deltaic deposits, confirmed by the TOC value. So, the presence of the gas hydrate is compatible with the geological context.

Lines 248-260: Similar comment than here above, but for seawater data. The seawater data were analysed in order to evaluate the salinity and temperature at the interface seawater and sub-bottom. For example, in the case of freshwater spring from gas hydrate dissociation, the salinity would decrease.

Lines 261-264: It is not clear how the data used in this paper supports the main hypothesis (i.e. the observed relief is related to 'fluid flux channelized by faults and fractures'. Nor do the data provide evidence for gas hydrate dissociation. We better explain that

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all data available (including data in literature) suggests that fractures and faults should be present and related to the fluid flux that could generate the relief.

Line 537: The term ‘co-isotope distribution’ is confusing. Rather use ‘stable isotopic composition’ instead. In figure 5b and 5c we refer to the linear relationship between δD and $\delta^{18}O$, and not only to the stable water isotope composition. It’s important to remark characteristics like slope and intercept of the line. We changed this to: “b. co-isotope linear regression of pore water samples and c. Co-isotope relationship of pore water samples against the global meteoric water (GMWL)”.

Fig. 6. The pictures are rather blurry. Moreover, those are common species that should be classified. The name of the genus is not enough. The pictures were improved and it was possible to identify some specimens at the species level, according to the bibliography of the study area.

Fig. 7. Seismic data would be needed to really assess this schematic profile. We included information about the BSR reported in this zone adding new figure (Fig. 7) modified from Vargas-Cordero et al. (2011).

Please also note the supplement to this comment:

<https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-362/hess-2018-362-AC2-supplement.pdf>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2018-362>, 2018.

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