

Interactive comment on “Isotopic evidence for biogenic molecular hydrogen production in the Atlantic Ocean” by S. Walter et al.

Anonymous Referee #1

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General comments

In view of the role of hydrogen (H₂) as potential new energy source, there is an increasing interest to identify its natural formation and consumption pathways as well as to quantify the atmospheric budget of H₂. Walter et al report a new data set of atmospheric and dissolved H₂ and its isotope signature from the Atlantic Ocean. Measurements of atm. H₂ from four cruises and diss. H₂ from two cruises are presented including a novel dataset of the isotope signature of diss. H₂ in seawater. Moreover, the atm. measurements are compared with model simulations. The manuscript is well written and concise. The results are well presented and discussed (but see my points below). The conclusions are mainly justified by the presented results (but see my points listed below).

Therefore, I recommend publication with a few (minor) corrections.

Major Comments:

Page 16440-42, Section 2.4.3, diss. H₂ measurements: Since the extraction method is new and presented for the first time, I would like to see a discussion about the overall measurement errors of both the diss H₂ conc. and its isotope signature in seawater.

p.16441, Eq.(6): H₂ solubility in seawater is also depending on the salinity, thus, I am wondering why the 'salt effect' is not considered in Eq.6.

p.16441, l. 16; Eq.(7); p.16449, l.6: The extraction efficiency is given as 92%. Is this a mean value? The extraction efficiency according to Eq.(7) is depending on the Ostwald solubility coefficient which, in turn, depends on the temperature. Was the temperature always the same? Please give details.

p.16441, l.16: Please cite a reference for the Ostwald coefficient.

p.16448, l.13-15: '... possibilities for improvements ...' are mentioned. However, what does that mean for the analytical error of the presented data? Or in other words are they just by chance in agreement with the literature data?

Table 4: I was surprised to see a positive (i.e. excess) H₂ concentration differences resulting in undersaturations of 0.35 and 0.94. That does not seem to be logical at all: How can a concentration excess of H₂ result in a H₂ undersaturation? Please explain.

Discussion of oceanic sources of H₂ (see Section 3.3.1; Table4; Figure7): It is obvious that the majority of H₂ data pool associated with the low temperatures has been measured in the coastal upwelling system off Mauritania. Upwelled waters off Mauritania, however, are originating from water depths down to 150-200m (i.e. upper oxygen minimum zone, OMZ). Thus, it is not that surprising to see a different H₂ signature because in OMZ waters there might different sources/sinks at work: E.g., the interplay between H₂ production (i.e. by N₂ fixation at depth) and associated H₂ oxidizing bacteria (which live at oxic/anoxic interfaces in particles and sediments).

Moreover, I am not really convinced by the argument against a H₂ sink (see p.16451, lines 1-5) since the observed undersaturations point towards a net H₂ sink (sink > source) they can, therefore, not result from a single source (see p.16451, l.17-20).

Technical Comments:

Mauretania should read Mauritania, please check spelling throughout the manuscript.

Table 4: X_h and D_a are mentioned in the table caption, but do not appear in the header of the table: I guess that X_m and D_m should read X_h and D_a (or the other way round).

p.16439/40; Figures 4 and 5: Please remove the CO data when they are not essential for the discussion of the H₂ data

Suppl. Material: There are citations in the text of the Supplement, but there is no list of references. Please add list with full references.

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