Ocean Science Discussions

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Reply on RC1

Knut Ola Dølven et al.

Author comment on "Autonomous methane seep site monitoring offshore western Svalbard: hourly to seasonal variability and associated oceanographic parameters" by Knut Ola Dølven et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-85-AC1, 2021

Thank you for your positive comments and for acknowledging the importance of this study. Please see below our replies regarding the suggested changes (reviewer comments in italic).

- *The salinity used in this study is obviously practical salinity (no unit), not absolute salinity (g kg^{-1}). This fact (the name "practical salinity") should be mentioned somewhere (something like "here and elsewhere in the papers the salinity values are practical salinity").*

We agree, this should be mentioned and we will include this in line 95.

- *Are the CH4 and CO2 trends described in lines 110 and 115 statistically significant, considering the high variability of the measurement values? The former (methane) probably is due to the large difference at the start and end of the time series, but I would not be sure about the latter (CO_2). Also, generally no tests of statistical significance or*

In lines 110-115 we describe a 2-week median value including both 24-hour and daily 1-hour data, which prevents from calculating meaningful statistical significance due to persistence and an uneven sampling scheme. However, the trend described in the time period (up until January 2016) is statistically significant, with a p-value <0.001 for an F-test with N-2 degrees of freedom (see e.g. James et al., 2013) and a Durbin-Watson statistic of 1.78 indicating no autocorrelation (upper limit, dU=1.72, see Durbin and Watson, 1971), using the averaged 1-hour data, including the first hour of the 24-hour weekly data for a total of 190 days/datapoints from July to January, assuming a linear trend. A caveat with relying on the daily 1-hour data in this way is the 23.93 hour periodicity in the current velocity data, which gradually changes the most probable origin of the measured water. This effect is described in Sect. 4.1 ln 175. Using only averaged 24-hour data (avoiding the problem of daily periodicity in current direction), i.e. 1 value every 21 day to a total of 9 values (up to 28 December), gives a non-significant trend (p=0.09). It should, however, also be noted that we have assumed a linear trend, which is probably not the best regression model in this case.

Due to the faulty pump and inherent uncertain response time for the CO_2 sensor, we could not use the daily 1-hour data. Therefore, the only (if any) meaningful way to report significance for this data would be to average the data collected in the 24-hour measurements. In that case, the trend from July to November is not significant ($p=0.39$), but the decreasing trend from November to May is ($p=0.0001$). But as described in the manuscript, this data should be interpreted with caution due to the faulty pump.

In general, we decided not to focus on reporting statistical significance for the different trends and correlations due to the uneven sampling scheme, wide range of characteristic time-scales in the chemical/physical mechanisms we investigated, nonlinearity, as well as persistence at different time-scales in the different datasets. For instance, temperature autocorrelates on time-scales of weeks, while methane concentration autocorrelates on time-scales less than a few hours. Indeed, the correlation matrices in the Results section are meant as a first order overview of potential relationships. While there are ways to circumvent problems with persistence in time-series, e.g. by using autoregressive models or estimating the effective degrees of freedom (Bretherton et al., 1999), we would in any case have to individually adapt methodology for each time-series to obtain meaningful significance estimates. We therefore instead chose to focus on identifying the principal features, phenomenon, and hypothesis that should be further investigated and provide individual, tailored analysis of these (the Discussion section).

- *A minor nitpick. The statement in line 115 can't be right as it is written: "CO_2 averaged 403 µatm with a decrease from mid-November 2015 (□400 µatm) until 6 May (□391 µatm) in 2016 (Figure 2a)". Looking at the figure I know what you mean but still something that decreases from 400 to 391 should not have on average 403. Please rephrase.*

We agree that there is missing information, as CO2 indeed increased towards mid November when the concentration was around 410. We will clarify this.

- *The cited literature is very rich and generally well chosen but I am surprised by the lack of a citation of the review paper James et al. 2016, https://doi.org/10.1002/lno.10307. It could be cited in multiple places in the manuscript as it covers many of its threads. For full disclosure I am one of its co-authors so please treat this as non-obligatory but I honestly think it's lack is puzzling.*

We completely agree that James et al., 2016 should have been cited - it is indeed one of the most substantial review pieces done on the overarching topic of our study. The manuscript went through a substantial cut in content prior submission and it must have slipped then, but we will make sure to include this citation in line 34 and/or 37, where it should fit well.

References

Bretherton, C. S., Widmann, M., Dymnikov, V., Wallace, J. M., Bladé, I.: The effective number of spatial degrees of freedom of a time-varying field. Journal of Climate, 12(7):1990-2009., https://doi.org/10.1175/1520-0442(1999)012<1990:TENOSD>2.0.CO;2, 1999.

Durbin, J., Watson, G. S.: Testing for Serial Correlation in Least Squares Regression. III. Biometrica, 58(1), 1–19. https://doi.org/10.2307/2334313, 1971.

James, G., Witten, D., Hastie T., Tibshirani R.: An Introduction to Statistical Learning: with Applications in R. New York: Springer, 2013.