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Interactive comment on “Effect of ocean acidification on the fatty acid composition of a natural plankton community” by E. Leu et al.

Anonymous Referee #2

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This article describes the results of a mesocosm experiment to determine the effects of pCO₂ levels on the fatty acid composition of natural plankton communities in a northern fjord. I found the aims and results to be interesting, but I also have a number of reservations/concerns on the approach taken and the presentation of the work. One aspect that worries me is the lack of replication in the experiment, as only two of the 9 mesocosms had the same pCO₂ treatments whereas the rest all varied. Furthermore, the control treatment requires clarity, as I remain unclear about where/how the controls are presented in the results (and their relative significance overall). Some key methodological details are also missing, which makes it difficult to properly evaluate the manuscript.

Introduction: Page 8175, line 17: EFAs can be produced in small quantities by some

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heterotrophs, so some caution should be used here.

Experimental setup: What is the reasoning behind the times starting at -4 (and deployment at -7: what does this mean)? This time scale was never explained in the manuscript, and some of the plots begin at +4, some at -4 clarity is needed. How was CO₂ saturation accomplished on the seawater added to the mesocosms? What constitutes a sample? Volume filtered? Filters used? Replication? There does not appear to be any replication in this design (either within a mesocosm or among mesocosms). What is meant by 'most parameters were sampled daily'? We have no idea of what this means or how these 'parameters' were measured (or why). Nor are we provided with any 'parameter' data in the results section (other than chl a). How were the mesocosms closed? How do you account for/measure any enclosure effects on the experiment?

Analyses: Why was 23:0 used as an internal standard? This fatty acid has been found by others in natural water samples. Specify what the standard was used for (i.e. specify that it was used to create the quantitative measures. . .). Statistical analyses: Linear regressions are described here, but the results text is all about correlations. Regressions and correlations are different procedures. The three phases should be explained here (how are they distinguished and why). Which fatty acids were used to run the PCA – just those in Fig. 4 (should be specified here).

Results: I have issues with the results in that the authors bring in results from other studies (e.g. page 8180, line 18 and down: discussion of nanoplankton dominating the community, etc.). Relation of the study results with literature sources must be done in the discussion, they cannot be presented as though they are new data in this paper. Some of the statements made (e.g. 16:0 and lack of changes over time) are questionable and don't appear supported by the figures. The scales of the different figures also vary greatly, which makes them difficult to compare. Table 1: which mesocosms are controls? Why no replication? Table 2: data were averaged over the time period of three phases – what exactly does this mean? In this table, Rsquare and

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slopes/intercepts are meaningless for non-sig regressions, those details should be removed. There should be a sample size included for each line. Fig. 2: all of the abbreviations must be defined in legend. I also don't see the point of including PUFA and n-3 PUFA and other similar pairs. Discussion: There is methods information buried within the discussion: for example on page 8183, line 22 – information about the biomass measures must be presented in the methods section, not here. How was particulate carbon biomass measured (missing from methods)? Statements made about C:P and C:N ratios in the POM being low (obviously again from previous studies), but what does 'low' mean exactly? Many of the statements made on page 8184 are obviously referring to previous studies, but references are not specified in many cases. Page 8185, line 14 and down: discussion around CHEMTAX analysis, but not enough detailed comparisons made and related with the study aims, and again reference sources are not specified – the reader is in the dark about these data, especially since the two papers are apparently 'in prep', which is extremely unhelpful. The reader cannot accept these statements as well argued unless he/she has access to these other papers. I have issues with the cirripedia larvae data – the methods for this aspect were not described at all (in the methods section), and if it is true that the larvae are too small to be separated from the POM (line3, page 8186), then how could this procedure have been done at all? Again, there are methods missing and results placed in the discussion. Overall, the discussion does not address the key questions of the study, and I am not convinced by the data presented that the trends as stated can really be justified. The lack of replication, the vagueness about the controls, and the lack of methods details all contribute to a non-convincing story for me.

General writing: Some of the sentences need improvements: the phrase 'it has been shown' should never be used, and there are many instances of indefinite 'this' used throughout – one must always specify what 'this' is. Formatting of fatty acid nomenclature must be consistent – omega is used in one place, and a n format is used elsewhere (which should be n-3, not n3). 'between' is used instead of 'among' throughout the manuscript (when comparing more than 2 things, must use among).

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