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Interactive comment on "Effects of dust additions on phytoplankton growth and DMS production in high CO_2 northeast Pacific HNLC waters" *by* J. Mélançon et al.

Anonymous Referee #2

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This manuscript contributes important empirical evidence and debate on the influence that ocean acidification may have on biogeochemistry in the oceans. Iron availability is episodic in the NE Pacific and plays an important part in determining pelagic community composition and levels of productivity. Investigating how an increase in pCO2/decrease in pH influences the response to episodic iron availability in natural oceanic communities is ambitious and unsurprisingly, not altogether easy to interpret. Nonetheless, the authors have designed an effective and achievable experiment, applied a suite of relevant measurements and provided, in general, a balanced interpretation of the results. Several points could be clarified to improve the manuscript:

1. Title: The title fails to capture the essence of the study, in my view. It puts a large

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emphasis on the influence of dust additions but the manuscript is presented very much as an ocean acidification+dust experiment. It also does not capture the impact on community composition, which is a much larger component of the paper than the DMS response.

2. Iron treatments and bioavailability: The application of two different versions of iron addition is useful and adds a great deal to the interest of the paper. However, this is not reflected in the stated objective of the research (L12286, L12+) and should be. How do differences in the form of iron used for previous comparable experiments influence those results (Introduction, L12285, L3+)? Although the reader is directed to Nishikawa et al (2000) and Hwang and Ro (2006) for more information on the specific dust added, it would be useful to present more details on the dust in this context. This should include total iron content and possibly the form of the iron present in the dust. If DFe is not a good measure of the bioavailability of iron (Section 4.3) what should be measured? Please discuss why the Fe contained in the dust is more available than the added FeSO4.

3. Phytoplankton taxonomy and pigments (Section 2.4.3): The study uses HPLC-based pigment analysis and Chemtax to assign class-specific contributions to total chlorophyll. This requires more explanation. In particular, how is the contribution of diatoms differentiated from that of haptophytes or pelagophytes? As Table 3 illustrates, these classes share several of the biomarker pigments and differentiating them is far from exact. At the last, the authors should make clear the uncertainty in the class-specific chlorophyll assignment and take it into account in their conclusions that increased pCO2 increased the fitness of diatoms over other taxa.

4. Increased respiration is provided as one explanation for why increased 14C assimilation is not reflected in increased biomass in Control+Acid (P12298, L23+) and Dust+Acid (P12301, L19+). However, using 24h 14C incubations to determine carbon assimilation is generally thought to measure something closer to net production than gross production; meaning that any increase in the rate of respiration would be

BGD
12, C6431–C6433, 2015

captured by that measurement.

5. DMSP and DMS. Given the generally high DMSP content of dinoflagellates, it is puzzling that the almost immediate loss of dinoflagellates is not reflected in the trends in DMSP? What/who were the main contributors to the DMSP pools? Are the high initial DMS:DMSP ratios (roughly 1:4) a product of the experimental set-up, i.e. cell disruption/negative impact on dinoflagellates, or were in situ DMS concentrations similar at the time the water was sampled?

6. Iron uptake rate. Figure 4C. I am not sure this shows Fe uptake rate. This was measured as the incorporation of added 55FeDFB. A clear explanation (Section 2.4.4) of how (and why) Fe uptake rates are calculated from the assimilation of the 55FeDFB complex is required.

7. In general, the table and figure legends would benefit from a greater level of explanation.

Minor points: P12287, L19, CO2SYS needs to be consistent.

P12291, L16. What size of filter was used?

P12294, L3 etc. the levels of precision, reflected in the number of decimal places, should be consistent.

P12295, L10. Maybe refer to the Control in this sentence.

Interactive comment on Biogeosciences Discuss., 12, 12281, 2015.