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Interactive comment on "Ice Acidification: A review of the effects of ocean acidification on sea ice microbial communities" *by* Andrew McMinn

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I thank the review for his/her constructive criticisms. I agree with everything suggested and have made changes accordingly.

Anonymous Referee #2

General comment Ocean acidification (OA) is a hot topic that received increasing attention during the last 10 years or so. Several experimental studies have been conducted to assess the sensitivity of phytoplankton and other marine organisms to the predicted changes in pH/CO2 concentrations. Bacteria and microalgae living in the sea ice will also be exposed to changes in pH. Few studies so far attended to determine the sensitivity of these microorganisms to changes in pH/CO2. This paper reviews their main

findings. The paper reads well and provides a good overview of our state of knowledge. Although bacteria are also considered here, the focus is clearly on sea ice algae. The paper offers a good balance between the factual review of the findings from the different papers and more personal viewpoints. Surprisingly for such specific and relatively recent topic, the number of published papers is large enough to justify a re- view. The main conclusion is that ice-related organisms are generally quite resistant

I would recommend to:

Provide an estimate of the relative importance of bottom, brines, and top ice assemblages to the annual primary productivity. In the present version of the paper, algae living in these different ice 'habitats' seem to be equally important in terms of PP which is not the case.

RESPONSE: I have modified this section to explain that each of these communities can be dominant in different seasons and different locations.

Present the information on bacteria and algae in different sections.

RESPONSE: Now broken into different sections, as suggested

Specific comments

P1, 10: . . .than marine phytoplankton. . .Note that coastal and even more estuarine phytoplankton are also subjected to large variations in pH taking place at different time scales.

RESPONSE: RESPONSE: Yes, I have added 'like some coastal and estuarine phytoplankton'

P1, 23: . . .on bacterial growth. . .

RESPONSE: Added 'bacterial'

P2, 20-21: Is this seasonality found all over the SO or only in the marginal ice zone?

The 2-3 examples provided in the paragraph are all from the near coastal waters.

RESPONSE: Added ‘in the seasonally ice-covered waters’

P2, 32: . . .CO2 concentration in. . ..?

RESPONSE: Added ‘in brine channels

P3, 14-19: This paragraph disrupts the flow of the paper.

RESPONSE: I have now removed this paragraph to the final section

P3, 14: . . .form later in the season and melt sooner. . .Yes but the extent of sea ice tends to decrease over most the Antarctica waters.

RESPONSE: Changed to ‘sea ice extent will inevitably decrease and the ice will form later in the season and melt sooner’.

P3, 30: . . .The biological communities can be. . .Here the author should refer to the previous studies describing these different assemblages (ex. Cota et al. 1991, Horner et al. 1992, and the more recent one by Bluhm et al. 2017). In Antarctica ice, infiltration assemblages are important at time. They are not mentioned in the review.

RESPONSE: Added Horner 1992, Bluhm et al 2017, Arrigo 2014. Infiltration communities are a type of surface community, which are discussed here extensively. Recent reviews (eg Arrigo 2014) refer to infiltration communities to as ‘surface communities’ and do not specifically emphasize the mechanism of their formation. I have added ‘infiltration to the first time I comment on surface communities in both the abstract and the section on biological communities (p1 ln 15; p4 ln 4).

P4, 9: ...communities (add Bluhm et al. 2017 in the list. . .

RESPONSE: Added Bluhm et al. 2017

P4, 28: . . .diatoms, which also show increased. . .I am not sure ‘also’ fits well here since there are several mentions before (and after. . .) of no or negative effects of high

CO2 on phytoplankton growth. This could be confusing.

RESPONSE: Deleted ‘also’

P5, 4: . . .which affects average. . .

RESPONSE: Changed to ‘affects’

P5, 32: Delete ‘at all’.

RESPONSE: Lannuzel et al. 2011 is correct

P6, 1: . . .the important. . .delete ‘important’ or explain why this species is important.

RESPONSE: Deleted

P6, 2: . . .Unlike most previous experiments, growth was not stimulated. . .Why ‘unlike’? You previously mentioned other studies showing no effect of high CO2 on phytoplank- ton growth.

RESPONSE: Deleted ‘unlike most previous studies’

P6, 9-12: This paragraph will better fit at the end in a ‘summary’ section. P6, 30: . . .demanding function for species with CCMs. . .

RESPONSE: I have moved to the final paragraph

P7, 3: . . .how these processes. . .Which processes? Please be more specific.

RESPONSE: Added ‘the presence or up/down regulation of CCMs’

P7, 10: . . .Likewise. . .The author should explain why bacterial growth increase with increasing CO2 concentrations.

RESPONSE: Added ‘because of increased DOC production’

P7, 14: . . .Sea ice ecosystems. . .The idea developed in this paragraph is interesting but is not well introduced. The paper needs a proper conclusion section starting with a short summary of the main findings, followed by the limitations identified (ex. age of the culture), and ending perhaps with the importance of considering the full life cycle of the species.

RESPONSE: I have added a new section '5. Discussion and summary' This section expands on the ideas suggested here and adds a final summary.

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