Biogeosciences Discuss., 10, C2475–C2477, 2013
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Biogeosciences Discussions
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Interactive comment on "Respiration of Mediterranean cold-water corals is not affected by ocean acidification as projected for the end of the century" *by* C. Maier et al.

Anonymous Referee #1

Received and published: 4 June 2013

It is really a shame the Author did not include these data on the last recently published paper concerning the effects of acidification on Mediterranean cold water corals (Maier et al 2013). Together these new measurements would have a major value than presented in a separate study. Actually, although the respiration rates of these corals have never been measured under acidified conditions, as several times repeated through the ms, this paper does not add new relevant insights. The tested hypothesis that the ability to maintain positive rates of calcification (Maier et al 2013) at high pCO2 is accompanied by higher energy requirements could not been fully proved by measuring only the respiration rates as here presented. Other measurements such as food ingestion rates, and the lipid reserves production and deployment should be investigated in

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order to support the main hypothesis. In addition, there are two major factors which might have masked the response of corals to the acidified treatments: i) as clearly showed by the high deviation of data, the parameter used to normalise the respiration rates (skeleton weight) is not the best one, as also recognised during the discussion; I know that measuring the protein content required the sacrifice of the precious samples. However, normalising data to the protein content or tissue surface area would be more informative than the skeletal weight; ii) the high feeding regime used during this experiment certainly contributed to keep the corals happy, leading to more energy available for their growth. Actually we ignore the amount and quality of food for cold water corals in their natural environment but it is likely that feeding in aquaria does not mimic a real, ecological relevant situation. This crucial aspect has not been extensively discussed so far, especially in the conclusions raised by the Authors, except in the last sentence of the section 3.3 which, in my opinion, is very dangerous because it invalids the conclusions and the message given in present study and the previous Maier et al 2013.

Minor comments Introduction. The saturation state of calcium carbonate is no more a good indicator of whether calcifying organisms can build and maintain their exoskeletons, as suggested by the Authors. There are several examples of corals able to calcify and growth without any signs of dissolution at undersaturated conditions (e.g. for DSC, Thresher et al 2011; Jantzen et al 2013 Mar Biol; Lunden et al 2013 L & O).

I found the incubations in the respiration chambers too long. Leaving corals for two hours and half in a closed vial means that most of the oxygen was consumed by the animals, likely becoming a toxic environment, and that the seawater carbonate chemistry of the medium greatly changed during the incubation, including the tested level of pCO2, therefore confounding the effect of treatments. Why the Authors used such long-term incubation?

In section 2.3 there is confusion with the previous publication, including G, calcification rates, in the calculation. It is not clear from this section how the Authors measured it

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during the present experiment.

Regarding the hypothesis that more energy must be allocated to up-regulate internal pH, McCulloch et al 2012 calculated for cold water corals a growth-rate cost of ca. 10 % per 0.1 pH unit decreases in seawater pH. This means a 20-40% increase in the present experiment, which likely was masked by the elevated feeding regime used and/or the large deviations of data.

The Authors did not really discuss their results with previous studies on other Mediterranean cold water corals such as Naumann et al 2013, which showed high thermal tolerance of these corals.

Interactive comment on Biogeosciences Discuss., 10, 7617, 2013.