Review of the manuscript "Distribution and host diversity of *Amoebophrydae* parasites across oligotrophic waters of the Meediterranean Sea" by Siano et al.

This is an interesting manuscript that gives novel information on the abundance and relevance of Amoebophryidae parasites in oligotrophic Mediterranean waters. The paper is well written and discussed, and of great interest for improving our knowledge on the role of parasites in marine ecosystems. The methods used are in general appropriate, although some of the presented conclusions are not clear given the limitations that they present.

Abstract:

I think that the abstract is a little bit misleading. It is said that prevalences varied between 2-10%. However, an important number of species were infected with relatively low prevalence's (1-3%) and in all but one of the studied stations dinospores accounted for a very small proportion of the total eukaryotic cells (0.4-3.1%). Therefore, it is true that dinospores are infecting populations in oligotrophic waters, but the control on host populations is not clear yet. It would be important to rewrite the abstract somehow including this information.

Line 22 in abstract- with a notable exception for Blepharocysta paulsenii for which 25% of cells were infected at one of the studied stations (Station C means nothing in the abstract).

Results and Discussion:

Line 7 (7395): The life-cycle is completed within 2-3 days with the death of the host cell- Is there any information about if this period is changed by environmental conditions?

Lines 14-15 (7397) Concentrations of NO3+NO2 along the first 50m of the water column were notably higher…. How much is notably higher? Nothing is said in results about nutrient concentrations or if the NO3 values are significantly higher in station 27.

It is stated several times that the abundance of dinospores at station 27 could not be associated with dinoflagellate abundance or a particular species presence. However we do not know if the data on dinoflagellates (on a personal communication) include also only thecated, larger than 60 μm dinoflagellates or include all. Could small or naked dinoflagellates explain the pattern in station 27? This information can be very important given the limitation of the method to detect dinoflagellate infections when dinoflagellates lack these characteristics. Indeed, the explanations for the higher abundance of dinospores at station 27 are given in a quite confusing way in the discussion. If there are three possible explanations, they should be enumerated first and then discussed. 1) Presence of other potential hosts for dinospores, overlooked in the study; 2) Nutrient concentration or other chemical substances affecting infectivity success. 3) Physical factors (light, turbulence). In this sense, in lines 23-24 (7405) Llavería et al. 2010 should be cited, given that these authors show an interesting model of how turbulence can affect parasite infection. Additionally, the discussion on the possible existence of differences in humic substances content in station 27 does

not seem to be very relevant, given that the abundance of dinoflagellates was not affected (as it is expected in case this was the case).

Because no temporal data is available, could the not higher abundance of dinoflagellates in this station be only a consequence of the higher infection?

Figures:

Figure 1. It could be interesting to see also NO3+NO2 along the first 50m together with prevalence levels in another figure.

Figure 2. Lack of units in the right Y axis. The information on the legend about the Ocean Data View Software is unnecessary, as it is on M&M.

References:

Anderson 2006 is missing in the reference list.