This paper uses five different primary marine organic aerosol emissions parameterizations in the GEOS-Chem global model and compares output to monthly, weekly, and hourly observations at two coastal sampling locations. Since several recent studies have looked at the impact of marine primary organic aerosol on cloud condensation nuclei and have mostly used different emissions parameterizations, this paper is important to understanding the strengths and shortcomings of the various schemes. I recommend that the paper be published in ACP as long as the following minor comments are addressed.

Abstract, **2nd sentence**:
The word "dependences" should really be "dependencies".

Introduction, 1st paragraph, last sentence:
The authors say here that marine POA has been shown to increase modeled CCN concentrations by 20%. Is that globally or regionally averaged? There should be a caveat with this saying it depends on your assumptions about sea-salt replacement or addition, internal/external mixing, and the emissions parameterization. For example, in Westervelt et al. (2012), we find that in the case where the sea-salt is replaced by the marine organics, CCN concentrations are actually decreased.

Introduction, 2nd paragraph, last sentence:
It is claimed that "all the available" marine POA emissions parameterizations are used in the study. The O'Dowd et al. (2008) parameterization is not included. I am aware that it is similar to Vignati et al. (2010), but this is still not all of the available parameterizations.

Section 2.1
Are you using RPMARES or ISORROPIA for the aerosol thermodynamics in GEOS-Chem? Mention and cite.

Section 2.2
Here I think you should mention explicitly whether or not your marine organic mass and number add to or replace the existing sea spray mass and number for the parameterizations that are linked to a sea spray scheme (everything but S08?). As mentioned before, that assumption is important.

Section 2.3
Are you comparing the observed WSOM+WIOM to the modeled hydrophobic + hydrophilic OM? This should be mentioned somewhere, perhaps in the results section, though.

Section 3.1/Table 2:
It is a little counter-intuitive to me that for all of the different parameterizations, the 31 S – 31N region has the highest contribution to global emissions. Is this just because it's an annual average so the northern and southern hemisphere's biologically active summers are cancelled out by the winters?

Section 3.3.2, Paragraph 2, Final sentence:
Regarding the time lag between offshore Chl-a and organic mass fraction, are you using the grid cell directly above Mace Head as your point of comparison? One quick and dirty way to explore the time lag would be to look at the OM concentrations at some of the grid cells just upwind of Mace Head (offshore).

Figure 3
The data from Rinaldi et al. (2010) in the upper right panel is supposed to be weekly averages, but the data points are so sparse it looks more like monthly or bi-weekly at best. Is this just missing data? Might be worth mentioning.

Section 3.3.2, Paragraph 3, and Figure 5:
Could the low wind speeds coinciding with low predictions of OM in G11 be quantified somehow? The reason being is in Figure 5b, it looks as if the highest windspeeds (red dots) are actually underpredicting OM just as much as overpredicting.