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Interactive comment on "Modelling global-scale climate impacts of the late Miocene Messinian Salinity Crisis" *by* R. F. Ivanovic et al.

Anonymous Referee #1

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The Messinian Salinity crisis is a very peculiar event in the Earth's history. It has been largely studied over the last 3 decades but how this multi-phase event impacted Late Miocene global climate remains to be studied. This paper attempts to clarify this question.

This modelling exercise consists of the use of the fully coupled AOGCM HadCM3 to simulate the response of atmosphere (in term of air surface temperature mostly) and ocean (in term of surface temperature, potential temperature at depth, salinity, meridional overturning) to different scenarios combining the salinity of the Mediterranean Sea (from fresh water to hyper-saline water) and the intensity of the Meridional Outflow water (from a no-exchange to a twice stronger exchange). The paper is clear and well written.

However I think that some points need to be clarified.

The authors use a "Messinian control" as a reference simulate for that period. In this run, the model is forced with boundary conditions representing the Early Pliocene (EP) (PRISM2 dataset). It includes EP topography, EP vegetation, reduced ice sheets and a pC02 fixed at 400 ppm. The authors give some references (Haywood and Valdes, 2004; Lunt et al., 2008a, 2008) in which the reader will find a full description of the simulated EP climate. However these papers did not give a full description of the simulated EP climate simulated with HadCM3. For instance, the air surface temperature shown in the paper (fig.4a) seems to be different from the figure 1 (top left) in Haywood and Valdes (2004). Concerning the two other papers (Lunt et al., 2008a and 2008b), they do not provide any further information how the AOGCM HadCM3 simulates the "Messinian" control (or Early Pliocene) at global scale since they consists mainly of sensitivity experiments to pCO2, topography, closure of seaway. I think that a more detailed description of the EP simulations will be useful. For instance, the authors should explain by how much the simulated EP climate is so warm, especially at low latitudes. The figure 4a does not permit to distinguish the respective effect of the rise of pCO2 (400 ppm vs 280 ppm), topography changes (fig.4d), and/or albedo changes. Changes in surface albedo can be shown. The sea surface temperature anomaly (fig.4b) should be discussed. The figure 4c (precipitation minus evaporation anomaly given in percentage) does not permit to observe the wetter and drier zones. These anomalies should not be explained in %. The authors must quantify the global mean difference in air surface temperature, precipitation, the change in NADW.

The sensitivity experiments to changes in MOW exchange and Mediterranean salinity are interesting. The authors have selected the three most pertinent experiments. However the authors must better explain how the "most pertinent" simulations were selected. In the discussion, they indicate that the chosen simulations can't be associated with a peculiar stage of the Messinian Salinity crisis. Thus the choice of the runs is not clear. The full data can not be accessed on the website.

I suggest that the authors should discuss the role of salinity changes and MOW exchanges separately. The figure 8 displays the impact on annual mean surface air temperatures but it would be interesting to display the changes in sea ice and/or surface albedo. A seasonal approach may be useful. Moreover the authors should add a sentence about possible feedbacks due to vegetation changes.

In the last part of the discussion, the authors concluded that air temperature is very sensitive to MSC. However the impact on precipitation is not shown and/or discussed. Finally the authors suggest that regions can be defined as key zones. It is not so obvious according to figure 8.

The red shades used in the colour scales of different plots (figures 4a, 4b, 8a etc) are hard to distinguish.

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