The manuscript describes a set of experiments with enforced removal of the grounded ice performed using the PISM ice-sheet model for the Amundsen Sea Embayment region in West Antarctica. The first author appears to be a graduate student and I have sympathy to them. However, I have no sympathy for the senior coauthors who did not spend time and effort required to conduct a meaningful study and produce a good manuscript. The manuscript has a number of misconceptions, unsupported claims, self-inconsistencies, and conclusions that do not appear to be supported by the study results. The study itself appears to be based on ad-hoc parameterizations, which are not physically justified.

The Marine Ice Sheet Instability is valid for to steady-state conditions only, i.e. all internal and external parameters do not change in time. In the described experiments, the ice shelves were artificially removed, additionally the grounded ice is artificially removed according to an ad-hoc "calving" parameterization. The evolving glacier configurations are due to this enforced removal of ice and have little to do with concepts of Marine Ice Sheet and Ice Cliff Instabilities that are instabilities of steady state configurations. The experiments use parameterizations, which main property is simplicity, however, the manuscript does not provide physical justification for their use. The interpretation of the results ignores a large body of studies of marine outlet glacier dynamics (a non-exhaustive list is below).

It is difficult to assess the model results because the results of individual experiments are highly sensitive to the parameters used in parameterizations, and differences in "sea-level contributions" between different scenarios are primarily determined by the parameter choice. The manuscript describes only "sea-level contributions", which is an integral metric and the grounding line positions, which are difficult to interpret on their own. In the absence of ice-dynamics related parameters such as ice velocities, thickness, stresses or strain rates, it is impossible to assess the reasonableness of the obtained solutions. With the used model resolution of 4 km, the simulated grounding line dynamics is questionable, as the required spatial resolution should be 2 km or higher (Seroussi et al., 2014).

In terms of the manuscript presentation, the manuscript should be self-sufficient and have enough information to follow it without in-depth reading of referenced publications. A brief description, and a physical justification of chosen parameterizations would be sufficient. The manuscript has contradicting statements. The title of the section 3.4 is "Bed
topography controls rate of grounding line retreat", the line 31 of page 14 states "In summary, we find no clear statistical correlation between the bed topography and the grounding line retreat rate."

As apparent from above, the manuscript cannot be published in its present form.


