

Interactive comment on “SELEN⁴ (SELEN version 4.0): a Fortran program for solving the gravitationally and topographically self-consistent Sea Level Equation in Glacial Isostatic Adjustment modeling” by Giorgio Spada and Daniele Melini

Geruo A (Referee)

geruoa@uci.edu

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The manuscript describes an updated numerical model that solves the sea level equation. Compared with its previous iterations, the new model now accounts for shoreline migration and rotational feedback, and it features enhanced portability and computational efficiency. The theory session is generally easy to follow. The result session provides a clear overview of the model configuration (i.e. ice and Earth model inputs) and highlights the newly implemented features (i.e. shoreline migration and rotational effects). The manuscript fits the scope of the journal and it is generally well written. I

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Discussion paper



have a few minor comments listed as follows.

The authors first define the sea level as $B = -T$ (Eq. 3 and Page 3, Line 21), and later express it in Eq. 16 as the difference between the sea surface height and the height of the solid Earth surface. I think Eq. 16 is a more intuitive definition of the sea level. Based on this equation, it is also straightforward to define topography as $T = -B$. I suggest the authors introducing Eq. 16 before Eq. 2. Following this definition, the ocean function can also be defined immediately ($O = 1$ when $B > 0$ with no grounded ice).

Figure 8. Please consider increasing the range of the color scale to make the plot less saturated.

Page 15, Line 15 to the end of section 3.3.2. It would be helpful to clarify the typical range of errors of tide gauge measurement. I think this would help the readers understand the significance of the difference among the reported model runs.

Table 5. The label and the caption of Column (e) should be ICE-6G instead of ICE-5G.

Page 16, Line 1. Please clarify how the coherence between the two predictions is quantified.

Page 16, Line 20. It would be helpful to clarify that the “direct effect” is associated with the change in centrifugal potential. This would help the readers follow the discussion at Lines 21-25.

Page 17, Lines 19-25. This part of the conclusion focuses on the computational aspect of the model while the current result session is not organized in a way to highlight this aspect. It would be helpful to include a brief summary in the result session to justify this part of the conclusion, especially regarding the second point at Line 21.

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