In this work, the authors aimed at refining gravity anomalies of coastal areas by combining a global geopotential model (GGM) and a residual terrain model (RTM). The 3” resolution SRTM v4.1 DEM and the 15” resolution GEBCO_2024 DBM were combined into a hybrid DEM/DBM that describes the topographic relief over the land and the seafloor undulation over the ocean. In order to avoid distinguishing the rock density and the seawater density, the rock-equivalent technique (RET) was applied to condense the seawater masses into equivalent rock masses, leading to the unified rock density within the RTM. Afterwards, the RTM gravity anomalies were estimated by the prismatic approach. The numerical results showed that improvements of gravity anomalies over both land and ocean areas could be achieved by adding RTM gravity anomalies to GGM gravity anomalies.

Refining gravity field of the GGM by incorporating high-frequency gravity field signals obtained from the RTM is widely used for modeling regional gravity field and eliminating the truncation error of the GGM, especially in the area with sparse gravity observations or even with data gaps. Obviously, the construction of an appropriate RTM is essential for this method. In the land area, since the topographic masses have the same rock density, it is straightforward to generate the RTM by subtracting a smooth reference topography from a detailed topography that is usually represented by a high-resolution DEM. However, when the research area is located nearshore, the construction of the RTM becomes more challenging as not only the topographic relief over the land should be considered but also the seafloor undulation as well as various mass densities should be taken into account. An appropriate RTM over the coastal area can also help to determine a precise land-sea (quasi)geoid model. Therefore, the motivation of this work is solid and such a work is worthy for investigation. In general, this manuscript is well organized and the English is readable. However, my primary feeling is that this work looks very similar to the work of Hirt (2013) after carefully reading through the manuscript. It seems that the novelty of this work is not obvious. Furthermore, I also have some puzzles on the procedure of constructing the RTM given in this manuscript, which are shown as follows.

Major comments

(1) In this manuscript, the detailed topography of the research area is described by a hybrid DEM/DBM obtained by combing the SRTM v4.1 DEM and the GEBCO_2024 DBM, while the reference topography is derived from the Earth2014. In fact, the RTM effects represent the high-frequency part of the topographic effects, requiring that the reference topography should be sourced from the detailed topography. Obviously, the hybrid DEM/DBM and the Earth2014 have different sources, especially in the ocean area because the GEBCO_2024 DBM is released in 2024 but the Earth2014 is released in 2014. This might introduce additional errors due to the source inconsistency. A more proper way is to generate the reference topography by applying a spatial filtering approach (e.g. the moving average) or the spherical harmonic approach to the hybrid DEM/DBM, as what have been done in most published works of using the RTM technique. Although the numerical results are positive in this manuscript, the modeling deficiency cannot be ignored from the theoretical point of view. I cannot say the current procedure is wrong, but it is flawed.

(2) The concept of the RET is not new, and similar works have been well done in Hirt (2013). In comparison with the inland area, the ocean and coastal areas encounter more complex environments, enhancing the difficulty of constructing a proper RTM in these areas. Within this background, the main purpose of the RET is to simplify the environments by condensing seawater masses into equivalent rock masses, with the cost of changing the geometry of masses but keeping the total masses unchanged. This results in a unified topography with the same rock density over the whole research area (including the land and ocean areas). From another perspective, the application of the RET over the ocean area is actually to transform the complex oceanic environment (with both seawater and rock masses) into the simpler “land” environment (only with rock masses but with negative heights). As a result, the construction of the RTM becomes the same as the case in the land area. For example, in the work of Hirt (2013), they firstly merged the DEM and DBM into a hybrid DEM/DBM, and then applied the RET to yield a unified DEM/DBM with the same rock density. On the basis of this unified DEM/DBM, the spherical harmonic approach was applied to generate the reference topography, and finally yielding the RTM. However, I find the procedure of constructing the RTM in this manuscript is different from that of Hirt (2013). According to the workflow shown in Figure 7, it seems that the authors firstly derive an initial RTM based on the original hybrid DEM/DBM and the Earth2014 without condensing the seawater masses, and then apply the RET and mass center offset correction to the initial RTM, yielding the final RTM. I am not sure such a different procedure is due to the fact that the authors misunderstand the work of Hirt (2013) or they intend to propose a new procedure but fail to highlight it in the manuscript. If the latter reason is, the authors must highlight its novelty and compare the current results to the ones computed by the procedure of Hirt (2013). Furthermore, according to my understanding on the RTM technique, when the initial RTM is obtained without condensing the seawater masses, its density should be the difference between the seawater density and the rock density with a positive or negative sign. Unfortunately, the authors do not give a deep and solid explanation on this issue in the manuscript. Finally, the description of the mass offset correction starting from page 9 and line 206 to Figure 6 is not clear to me. I do not understand what the authors mean and cannot follow Equations (9)-(10) as well as Figure 6. Please revise this part, at least convincing me that such a correction is reasonable and necessary.

Minor comments

(1) Figure 4: Is this RTM the one obtained after applying RET? If it is not, this RTM is not the exact model that is used for computing RTM gravity anomalies. Therefore, its presentation makes no sense.

(2) Page 7, line 170: "disturbance gravity" should be "gravity disturbance", "disturbance potential" should be "disturbing potential". Please revise similar descriptions in the other places of the manuscript.

(3) Page 7, line 173: I do not see any (x_i, y_i, z_i) in Equations (3) and (4). Please revise it to make it more readable.

(4) Figure 5: (x_2, y_1, z_1) should be (x_1, y_1, z_1), (x_1, y_2, z_2) should be (x_2, y_2, z_2).

(5) Equation (7): I suggest to revise "$\Delta g^{\mathrm{RTM}} = \sum_{i=1}^{k} \Delta g(i)$" to "$\Delta g^{\mathrm{RTM}} = \sum_{i=1}^{k} \Delta g^{\mathrm{RTM}}(i)$". My reason of doing so is that each prism represents the mass element of the residual terrain. So the corresponding effect is better to be marked as the RTM effect.

(6) Page 8, line 189: If my understanding is correct, "After incorporating Δg^{XGM}" should be "After incorporating Δg^{RTM}".

Upon the above comments, I recommend a major revision of this manuscript.