Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-289-AC3, 2018

Geoscientific Model Development Discussions

Interactive comment on "A multilayer approach and its application in modeling QGNSea V1.0: a local gravmetric quasi-geoid model over the North Sea" *by* Yihao Wu et al.

Yihao Wu et al.

yihaowu@hust.edu.cn

Received and published: 24 June 2018

Interactive comment on "A multilayer approach and its application in modeling QGNSea V1.0: a local gravmetric quasi-geoid model over the North Sea" by Yihao Wu et al.

With great interest, I read the article of Wu et al. "A multilayer approach and its application in modeling QGNSea V1.0: a local gravmetric quasi-geoid model over the North Sea". Unfortunately, the paper lacks many details which makes it hard to assess the results. My main concern is, however, that the authors are not very consistent compared to their previous study presented in Wu et al. 2017b. In that study, they used beside shipboard and terrestrial gravity anomalies also airborne gravity distur-

bances, multi-satellite altimetry measurements, and GOCE gravity gradients to compute a quasi-geoid model. To validate that model, the same GPS/leveling datasets are used as the ones used in this study. If we compare the statistics of solution A (obtained with the single-scale approach) in Wu et al. 2017b (solution computed without the use of GOCE gravity gradients) they obtained in terms of standard deviation 1.8, 1.8, and 1.6 cm for the Netherlands, Belgium, and Germany respectively. In this paper, they obtained using the single-scale approach 1.2, 2.8, and 2.9 cm for the Netherlands, Belgium, and Germany respectively. These differences are huge! Using their multi-scale approach, they obtained 0.9, 2.2, and 2.1 cm for the Netherlands, Belgium, and Germany respectively. Hence, except for the Netherland this solution still has a lower quality compared to what the authors presented in Wu et al. 2017b. The differences become even larger in case I compare their solutions obtained including GOCE gravity gradients data. To me, this shows that apparently the use of different layers of SRBFs is not the main issue in obtaining a better quasi-geoid model. Below, I provide some other concerns.

Response: The authors thank the reviewer for these beneficial comments. To our knowledge, the solutions in this study are indeed inconsistent with ones shown in Wu et al. (2017b), and should not be made simply comparison with each other. There are several reasons that you find the accuracy of solution modeled with the single-layer approach in this study is different from the one displayed in Wu et al. (2017b). First, in this study we only use terrestrial and shipboard gravity data, no airborne or radar altimetry data are incorporated. While, for the solution A (without GOCE data) in Wu et al. (2017b), we used terrestrial, shipboard, and airborne gravity data, and radar altimetry data. Thus, even we use the same GPS/leveling data for validation, we observe the different statistics for accuracy assessment. Second, the target area in this study and the one in Wu et al. (2017b) are not consistent. The area in the study of Wu et al. (2017b) extends from 49.5°N to 56°N latitude and 0.25°E to 8.25°E longitude (see page 6 in Wu et al., 2017b); While, in this study we choose a much larger area, which covers an area of 49°N-61°N latitude and -6°E-10°E (see page 3 in the original manuscript). And, when we choose a larger region, more data in UK, Norway, and the North Sea are incorporated. However, we notice that the data in Norway are sparsely distributed, especially in the mountainous regions; and this situation also occurs in the north parts of the North Sea, see Fig.2 in Wu et al. (2017b). Consequently, the quality of the solution may be affected if different gravity data are introduced, even when we validate the solution only use the GPS/leveling data in the Netherlands, Belgium, and Germany. We should not directly compare these statistics if these solutions are modeled under different conditions. For the similar reasons, we can't simply compare the solutions computed in this study with the ones in Wu et al. (2017b).

pp 2: In the first paragraph the authors state (pp 2: 4-5): "However, one layer of SRBF's parameterization may be only sensitive to parts of signals' spectrum and reduce the quality of the solution." –> This may seems so if you look to the spectrum of the SRBFs being used. However, several authors (e.g., Slobbe 2013) have successfully computed quasi-geoid solutions using one or two layer(s) of SRBFs that have an accuracy comparable or even better than the authors present in this paper. The only prerequisite is that the energy in the data at the lowest and highest frequencies is reduced by using a reference GGM and a digital terrain model, respectively. (Slobbe, D. C. (2013), Roadmap to a mutually consistent set of offshore vertical reference frames, Ph.D. thesis, Delft University of Technology.).

Response: The authors thank the reviewer for these comments. Yes, we believe the reviewer's statement is right regarding this multilayers approach may work fine when only the residual gravity field is modeled from the ground-based data, i.e., the long- and short-wavelength parts have been removed. In this study, we also model the regional gravity field within the framework of remove-compute-restore method, and only the residual signals are parameterized, we emphasize this in the revised manuscript according to the reviewer's comments, see pp 2 in the updated manuscript. We also see the (one) two layers of SRBFs works fine, i.e., see Slobbe (2013) and Wittwer (2009). However, we should not compare the accuracies of the solutions if they are modeled

under different solutions, see our detailed response to Q1. We also cite the contributions of the existing literatures regarding the modeling with single-layer approach, i.e., Wittwer (2009), Slobbe (2013). Moreover, we remove the "However, one layer of SRBF's parameterization may be only sensitive to parts of signals' spectrum and reduce the quality of the solution.", since we believe this is too absolute to some extent, which may lead to the wrong understanding. Based on the reviewer's comments, we modify and restructure the relevant contents, please see pp 2 in the updated version.

pp 2: I somehow have difficulties in understanding the main objective of this paper. The authors state without motivation (pp 2: 23-26): "However, differing from these methods mentioned above, we propose a multilayer approach, inspired by the power spectral analysis of local gravity observations, which indicates the gravity signals are the sum of the contributions generated from the anomaly sources that locate at different depths." In my opinion, a proper motivation is required. It should become clear what are the limitations in existing multi-resolution representation/multi-scale approaches and how the approach proposed by the authors is going to tackle these. Definitely, the authors are not the first ones that utilize a multi-scale approach as they mention themselves.

Response: The authors thank the reviewer for these beneficial comments. Yes, we think the reviewer's comments are right. In our opinion, there are two limitations for the existing studies. First, to our knowledge, no direct comparisons have been made between the single-layer approach and multi-scale one regarding the performances in local gravity field recovery. Besides, the existing multi-scale methods mainly construct the multi-scale framework in a mathematical way, where no explicit geophysical meanings are investigated. Thus, the main contributions of this study are twofold. First, to develop a new parameterization of SRBFs network in the framework of the MRR idea, i.e., the so-called multilayer approach; and the multiply layers are linked to the anomaly sources at different depths beneath the topography, which aim at recovering the signals at different levels. To our knowledge, no existing literatures studied this issue. Moreover, we assess the performances of the multilayer approach and traditionally-used single-layer one in this study, where the advantages and disadvantages of different methods are analyzed. According to the reviewer's comments, we modify the relevant part the updated manuscript and make the motivation more clearly, please see pp 2-3 in the revised version.

Section 2.1: It is not entirely clear to me whether or not the authors used GOCE gravity gradients as an additional datasets as they did in Wu et al. 2017b? The confusion is introduced by their sentence (pp 19: 6-8): "Moreover, the improvements in the frequency bands that GOCE data contribute may be also the reasons, since EGM2008/EGG08 was developed without GOCE data." This suggests that they used it. However, the dataset is not mentioned in Section 2.1. And what about the radar altimeter data and airborne gravity data the authors used in Wu et al. 2017b? If, indeed, these datasets are not used. What is the reason for that? In the abstract the authors mention that "A multilayer approach is set up for local gravity field modeling based on the idea of multi-resolution representation merging heterogeneous gravity data." What they do understand by "heterogeneous"? With their approach, can they not handle different data types?

Response: The authors thank the reviewer for these beneficial comments. We didn't directly use the along-track GOCE gradients as the additional groups as we did in Wu et al. (2017b). In fact, only the terrestrial and shipboard gravity data are introduced as the observation groups, Section 2.1 give the details regarding the data sets we use here. Although we didn't directly GOCE gradients, we used the GOCO05S as the reference model, which was computed with GOCE data. However, for the development of EGM2008/EGG08, no GOCE data were used. Thus, in the bandwidth that GOCE data contribute, i.e., in frequencies from 0.005 to 0.1 Hz, we believe our model may outperform EGM2008/EGG08. In this sense, we say "Moreover, the improvements in the frequency bands that GOCE data contribute may be also the reasons, since EGM2008/EGG08 was developed without GOCE data.", it doesn't not mean we directly combine the GOCE data as additional observation groups for modeling, but just use

a more accurate reference model in the measurement bandwidth (MBW) of GOCE mission. The motivation of this study is to develop a new parameterization of SRBFs network in the framework of the MRR idea, i.e., the so-called multilayer approach, and compare it with the traditionally-used single-layer approach for the performances in regional gravity field recovery. For a case study, we only use the terrestrial and shipboard gravity data, and the results in case derive reasonable solutions, which can be used for supporting the conclusions of this study. The "heterogeneous" here not only means the different types of observations, but also refer to the data sets with different spatial resolutions/coverage, different noise levels, see Wu et al. (2017c) in the updated version regarding the details of heterogeneous data sets. The different types of observations groups can be combined through the multilayer approach just similar as the way the researchers did for in the single-layer approach, e.g., see Klees et al. (2008), and Slobbe (2013).

pp 6: From Figure 1, the authors conclude that "the gravity signals are the superstition (should be "superposition" I guess) of the contributions generated from the anomaly sources at different depths; and the signals originated from different anomaly sources have heterogeneous spectral contents". I have strong doubts. In Figure 1, I observe a quite smooth spectrum (no distinct peaks or whatsoever). The red lines are to me somewhat artificial.

Response: The authors thank the reviewer for this comment. First, we only model the residual gravity signals in this study, and the power spectrum showed in Figure 1 is based on the residual gravity data in Sect 2.1, the short- and long-wavelength signals are removed. Moreover, the local gravity signals are the sum of the contributions of different anomaly sources, i.e., the contributions from different anomaly sources have been separated, and the spectrum here shows the one for the mixed signals. After we separate the different signals with wavelet decomposition, and more distinguished spectrums occur, see Figure 3 in the revised manuscript. We also want to mention that Figure 1 is just an example support the statement that the gravity signals are the sum of the contributions of different sources, and red lines are also the illustrations show that slopes of the spectrum are different in different frequency bands, and please see our response to the question below regarding how we estimate the slopes (i.e., the red lines) of the spectrum. However, we also think this figure is confusing to some extent, and we remove this figure and restructure the relevant part based on the reviewer's comments, please see pp 6 in the updated version.

pp 6: It is not clear how the authors estimated/obtained A_W (first term of Eq. 4)? Given Eqs. 6-7, I suppose A_W is not estimated...?

Response: The authors thank the reviewer for this comment. Based on Eq.4, the gravity anomaly can be decomposed into a number of wavelet details and a wavelet approximation. Thus, the difference between the gravity anomaly and the sum of wavelet details is the wavelet approximation A_W, similar information can be found in Xu et al. (2017, 2018). The target for the wavelet decomposition is to design the parameterizations of multilayer approach, and for modeling purpose, the point-wise gravity data are combined just as we do in the single-layer approach.

pp 8: To compute their solutions, the authors applied variance component estimation and regularization. However, nowhere the regularization parameter is given, neither the estimated weights.

Response: The authors thank the reviewer for this beneficial comment. Yes, we believe the reviewer is right, and the variance factors for different types of observations are important, indicate their relative contributions, and play a key role in data combination. According to the reviewer's comments, we add the information of estimated variance factors of different observations groups and regularization parameter in the updated version, please see pp 17.

pp 8: It is not clear why the authors used 10 as the "preliminary maximum order for decomposition"? Why not 20 or 5?

Response: The authors thank the reviewer for this beneficial comment. This is a good question. To some extent, the original maximum order is arbitrarily chosen. However, wavelet analysis has a number of nice properties, for instance, the low-order details are invariant with the increase of decomposition order, and only the high-order details and wavelet approximation change. Thus, we can preliminarily choose a predefined order for decomposition, and analyze the derived details as we do in Section 3.1. If there are still details that are useful for constructing the multilayer model haven't been separated, we need to increase the decomposition order until all the useful details have been extracted; otherwise, we can truncated to a specific order as we do in this study, and compute the corresponding the necessary details and approximation for constructing the multiply layer's network. According to the reviewer's comment, we add and enhance this information in the updated version, please see pp 9.

pp 10: In the manuscript, the authors suggest that the wavelet details (D_W) have a kind of geophysical interpretation; for example D_W is explained as "the local anomaly originated from shallow and small-scale heterogeneous substances." If so, can the authors comment on the maps shown in Figure 2? To me, these are very peculiar. In particular D_5, D_6, and D_7 show strange stripy patterns...

Response: The authors thank the reviewer for these beneficial comments. We think the reviewer's concern is right regarding these strange stripe like signals, since we carefully check the source code for wavelet decomposition, and find bugs that may derive incorrect wavelet details. Based on the reviewer's comments, we redo the wavelet decomposition based on errors corrected source code, and compute the updated wavelet details and approximation, please refer to Figure 1 (in pp 11) in the updated version, and no strange stripy patterns occur. Moreover, we provide the geophysical evidences for the different patterns of various wavelet details. More specifically, D_1 and D_2 are seems dominated by the high-frequency signals correlate strongly with the local topography, which are mainly due to the uncorrected topographical signals in RTM corrections. D_3 and D_4 with respective average source depths 4.5 km and 9.2 km primarily reflect the density distribution of the upper crust. The distribution of D_5 and D_6 is in agreement with the tectonic structure of the middle crust. D_7 is consistent with the Moho undulation. D_8 and A_8 represent density distribution of the upper mantle. Overall, these decomposed gravity anomalies can reveal the tectonic structure of study area at different depths. Based on the reviewer's comments, we add the detailed comments related to the different patterns of Figure 1 in the revised manuscript, please see the information in pp13-14. We also notice that the wavelet details and approximation change after we implement the wavelet decomposition with the errors corrected source code, and we redo the whole procedure for the multiply layers' network design, i.e., estimating the depths of different layers and the number of Poisson wavelets in each layer. Then, we recompute the solution based on the multilayer approach with the updated parameters of multiply layers (i.e., the depths of different layers and the number of Poisson wavelets in each layer), and redo the comparisons with existing models based on the updated solution. Following, the geodetic MDT (called MDTNS_QGNSea) based on the updated model derived from the multilayer approach is computed. Please refer to pp 13-29 in the revised manuscript.

pp 13: With Figure 4, I have the same problem as I have with Figure 1. How they came up with the red lines?

Response: The authors thank the reviewer for this comment. The average depths for the power spectrum of wavelet details are estimated from the eq.(5). Actually, a number of literatures showed how to estimate the depths from these spectrums, e.g., see Figure 4 in Xu et al. (2018). More specifically, the red lines represent rates of change for logarithmic power relative to wave number, which are estimated by autoregressive method. The starting point and terminal point of the red lines are inflection points of the curves (green lines in Figure 3), recognized by us according to the trend of the curves. Based on the reviewer's comment, we also add this information in the revised manuscript, see pp 13 in the updated version.

pp 15: The authors mention without any motivation that "Point-wise terrestrial and

shipboard gravity anomalies are merged for modeling." Why, these datasets usually have different accuracies...

Response: The authors thank the reviewer for this comment. For modeling purpose, point-wise terrestrial and shipboard data are combined. These data have different accuracies, and this is also one of the reasons why we need the MCVCE method for estimating the variance factors for different observation groups. The gridded gravity data is only used for wavelet decomposition, i.e., for designing the multiply layers' network, since this wavelet decomposition method needs the regularly distributed data. While, for modeling purpose, the point-wise data are directly used just the same as the single-layer approach. We also enhance this part for avoid confusing based on the reviewer's comment, please refer to pp 17 in the updated version.

pp 15-16: "These results demonstrate that the multilayer approach can more accurately recovers the local high-frequency signals than the single-layer one." –> Of course, the least-squares residuals are lower! In the multilayer approach you locate the SRBFs much shallower!

Response: The authors thank the reviewer for this enlightening comment. Yes, we believe the lower residuals may be attributed to the shallower SRBFs. Shallower SRBFs are more sensitive to the local high-frequency signals, and the corresponding spectrum also shifts to high-frequency bands, which may lead to a better fit to the data. However, there are still two aspects may be of concern. First, we parameterized the local gravity field by 7 layers with different depths, where the layer7 are still deeper than 40 km (where we locate the single-layer of SRBFs' grid), see Table 3 in the revised manuscript, thus not all the layers are shallower than 40 km. In addition, to our experience with the single-layer approach, the shallower SRBFs' grid may lead to a reduction of least square residuals, but not guarantee a better solution, i.e., the better fit to the independent control data for external validation, please refer to Figure 2, 3 in Wu et al. (2016), which clearly shows a shallower grid than 40 km may not derive a better solution. However, in this study, the multilayer approach not only derives a better fit to the data, but also obtains better solution validated by the control data. This can't acquire by solely putting the SRBFs' grid shallower. According to the reviewer's comments, and we restructure and enhance the relevant parts in the updated version, please refer to pp 18-19.

Table 6: The authors have used GNSS/leveling data to validate their quasi-geoid model. What is not clear to me at all is why the statistics presented in Table 6 for the single-layer approach are so different from the values they presented in Wu 2017b (solution A). In that paper, they obtained in terms of standard deviation 1.8, 1.8, and 1.6 cm for the Netherlands, Belgium, and Germany respectively. The parametrization they have used is the same. In this paper, they obtain 1.2, 2.8, and 2.9 cm for the Netherlands, Belgium, and Germany respectively. These differences are enormous! Can the authors explain what happened? Is that due to the fact that you did not use radar altimeter and airborne gravity data, and merged shipboard and terrestrial data sets. Anyway, it seems that compared to their work presented in Wu 2017b, their multi-scale approach performs still worser (except for the Netherlands)!

Response: The authors thank the reviewer for this comment. In our opinion, we should not directly compare these statistics if these solutions are modeled under different conditions. The solution derived from single-layer/multi-layer approach should be different from the solution A in Wu et al. (2017b), since the inputs for these solutions are inconsistent. Thus, even we use the same GPS/leveling data for validation, the derived statistics are heterogeneous. Please see our detailed response to the first question.

pp 19: "Apart from the application of different techniques for modeling, these differences are partly interpreted as the additional signals introduced by QGNSea V1.0, stemming from the incorporation of more high-quality gravimetry". This maybe applies to EGM2008 and EIGEN-6C4, but not to EGG2008.

Response: The authors thank the reviewer for the comment. Yes, we believe the reviewer is right. We also refer to EGM2008/EIGEN-6C4 when we say the additional

signals introduced by QGNSea V1.0 are stemmed from the incorporation of more high-quality gravimetry. And, the sentence “Apart from the application of different techniques for modeling, these differences are partly interpreted as the additional signals introduced by QGNSea V1.0, stemming from the incorporation of more high-quality gravimetry” further explains “For EGM2008/EIGEN-6C4, remarkable differences show in south of Norway and northwest of Germany”. However, according to the reviewer's comments, we modify this part slightly to eliminate misunderstanding, see pp 24 in the updated version.

Figure 8, the analysis is hampered by edge effects in QGNSea V1.0. The authors should exclude the edges of the area over which they computed QGNSea V1.0.

Response: The authors thank the reviewer for this comment. Yes, we believe the reviewer is right that the edge effects should be excluded. In fact, for plotting Figure 8 in the original manuscript (Figure 7 in the revised version), we have excluded the edge effects by contracted by 0.5 degree in all the directions. For modeling purpose, the boundary limits for the target area is chosen as 49 N-61 N latitude and -6 E-10 E longitude, see sect 2.1. While, for displaying the differences between different models, the signals only inside 49.5 N-60.5 N latitude and -5.5 E-9.5 E longitude have been extracted and compared. We also add this information in the updated version, please refer to pp 24.

The derived MDT models are not realistic. Please use DTU13MSS and EGG2008 to compute a MDT model and compare that to the one obtained using DTU13MSS and QGNSea V1.0. Prominent signals, like the Norwegian coastal current are not visible at all (e.g., Idžanovi ÌAËŻc 2017)! (Idžanovi ÌAËŻc, M., V. Ophaug, and O. B. Andersen (2017), The coastal mean dynamic topography in Norway observed by CryoSat-2 and GOCE, Geophys. Res. Lett., 44, 5609–5617, doi:10.1002/ 2017GL073777.)

Response: The authors thank the reviewer for these beneficial comments. Yes, we agree with the reviewer's comments, and the geodetic MDTs in the original manuscript are not realistic. The problem is seems due to the implementation of too strong filtering on the raw MDTs. In the original manuscript, we compared the MDT derived from QGNSea V1.0 with the existing global model called DTU13MDT. DTU13MDT was computed in a purely geodetic way, where the difference between DTU13MSS and the quasi-geoid derived from EGM2008 was used to estimate the raw MDT, and the derived MDT was further smoothed by a Guassian filter with a correlation length of 75 km to suppress the small-scale signals (Andersen et al., 2013). To make these comparisons consistently, in the original manuscript, the computed raw MDT (the difference between the DTUMSS13 and QGNSea V1.0) was also filtered by a Guassian filter with a correlation length of 75 km. However, based on the reviewer's comments, we believe this filter may be too strong since the prominent signals have been filtered out. According to the reviewer's comments, in the revised manuscript, we compute the raw MDT by computing the difference between DTUMSS13 and QGNSea V1.0/EGG08, and filter the raw MDT by a Gaussian filter to further smooth the derived MDT, which is called as MDTNS_QGNSea/MDTNS_EGG08. Considering the small-scale signals that have the wavelengths shorter than several kilometers can't be recovered from the local gravity data, since the mean distance between gravity data is approximately at 6$\sim$7 km level, the correlation length of Gaussian filter is chosen as 6 km instead of 75 km in the revised manuscript. This time, the derived MDTs show more realistic patterns, although MDTNS_QGNSea don't provide a full picture of Norwegian coastal currents due to the limited data coverage in Norway and its neighbouring ocean areas, please see Figure 9 in the updated version. According to the reviewer's comments, we restructure and modify the part for MDT comparison, please refer to pp 27-29 in the new version.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-289/gmd-2017-289-AC3-supplement.zip

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-289, 2018.