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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.

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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 disturbances, multi-satellite altimetry measurements, and GOCE gravity gradients to compute a quasi-geoid model. To validate that model, the same GPS/leveling datasets are

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Discussion paper

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.

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.)

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.

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?

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.

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...?

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.

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

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...

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

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...

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!

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)!

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.

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.

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 ÌĄc 2017)! (Idžanovi ÌĄc, 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.)

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