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Interactive comment on "Benthic biomass size spectra in shelf and deep-sea sediments" *by* B. A. Kelly-Gerreyn et al.

Anonymous Referee #2

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The manuscript by Kelly-Gerreyn et al. is first dealing with the descriptions of biomass size spectra of marine meio- and macrobenthos at three marine contrasted sites sampled in September 2000 (for two of them) and in December 2002 (for the last one). The authors then use a simple metabolic model based on 8 parameters at each of the 3 stations (6 consisting in three sets of two constants describing allometric changes in ingestion, respiration and mortality rates with body size, one dealing with assimilation efficiency and the last one with the flux of food available to the benthos in the considered size range). In each case, the model is considered to be at steady state and adjusted on measured biomasses in 16 size classes (with 5 replicates per class). The optimization of the model is based on : (1) a priori restrictions of possible parameter values, (2) the discretization of the so-obtained ranges, and (3) the repetition (10 times) of an iterative procedure derived from genetic studies until an equilibrium

is reached. This last procedure fails to produce a unique solution due to the fact that the number of equations constitutive of the model is insufficient relative to the number of unknown parameters. As pointed out by the authors themselves, the so-called optimized solutions (i.e., the ones leading to the smallest loss) of the three models should thus be considered with caution and attention should clearly be paid to the range of parameter values derived from the 10 repetitions mentioned above. The authors use their results to: (1) suggest/conclude that the model accurately reproduces observed benthos biomass size spectra in contrasted marine environments, thereby suggesting that benthos biomass size spectra are not significantly affected by other environmental parameters (e.g. hypoxia) than those involved in the model, and (2) the balance between the scaling coefficients of ingestion and mortality is a key factor in controlling benthos biomass size spectra.

The subject of the manuscript is clearly of interest for Biogeosciences. Its approach is original as well because of the comparison carried out between three contrasted areas. As it stands, the manuscript however still requires clarifications on several major points before I can recommend it for publication. My concerns are mostly dealing with data acquisition (observation and modelling) since potential flaws tend to weaken the (rather general) conclusions drawn by the authors.

1. Sampling. The authors apparently basically used 10cm diameter cores to sample macro-, meso- and meio-fauna. Irrespective of the later pooling of individual samples, this procedure is not necessarily appropriate for macrofauna, whereas it is probably OK for mesofauna and certainly OK for meiofauna. Bias in the sampling of large organisms were one of the main reasons for setting the upper range of body size considered in the study (39mgWW) however no convincing evidence is presented regarding the fact that organisms of this size were adequately quantitatively sampled. Along the same line, it is stated that macro- and meso-benthos were collected within the first 10cm of sediments, versus only 5cm for meiobenthos. In both cases, it is far from obvious that such horizons are adequate. In any case, evidence should be presented showing that

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such discrepancies do not affect benthos biomass size spectra.

2. Assessment of individual body weights. Body weights were derived from biovolumes, which were themselves assessed through the decomposition of individual body shapes in elementary geometric volumes that were measured. Neither reference nor qualitative assessment of this procedure is provided. Furthermore, it not stated whether it was carried out on fresh) or fixed organisms. In the latter case, a discussion on the occurrence of possible differential effects between taxa and possible consequences on biomass size spectra should be included.

3. Restriction of the range of body size. The lower limit of the considered body size range is set by the sieving mesh used to retain meiofauna. The upper limit is set by consideration regarding the sampling gear (see above). The truncation for small body sizes is taken into account in the model by reducing the amount of available food to the considered size range through the fbac parameter. The consequences of the truncation for large body sizes are less clear, since the model is strictly bottom up, and should be better discussed.

4. Optimization of the model. For all three considered sites, the 10 repetitions of the optimization procedure led to results almost similar in terms of adjustments (i.e., loss values) but rather different in terms of the values of the 8 adjusted parameters. The authors decided to interpret their results based on: (1) optimized values of each parameter, and (2) the ranges of the values of these parameters as derived from the 10 repetitions. This raises two key questions: (1) Do optimized values have any special validity (i.e., why not only base result interpretation on ranges?), and (2) why running (only) 10 repetitions of the optimization procedure? Regarding the second question, it could be interesting: (1) to assess changes in parameter ranges with the number of repetitions, and (2) to run multivariate analysis based on the values of the sets of adjusted parameters to assess whether the 10 (or more) derived solutions show different levels of similarity between each other (i.e., identification of “family of solutions”). This clearly constitutes a key point for the validity of the conclusions derived by the authors.

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5. Adjustments of the model. By looking at figure 3. It is clear that al all three sites, the increasing trend in the biomass size spectra is not linear in a log-log scale as these curves show marked secondary or even primary peaks. The affirmation that the model accurately reproduces observed benthos biomass size spectra at each considered site and furthermore in contrasted marine environments remains thus questionable. Quantitative data regarding the proportions of variances explained by the model could help in documenting this particular point, which is a key one for most, if not all, the conclusions derived by the authors.

6. The fact that the scaling coefficients seem to differ between the 3 considered sites should be discussed and possible causes identified.

7. Discussion on ingestion 4.3. First, it is not obvious that food selection correlates negatively with body size. Some large benthic invertebrates are microphageous and highly selective (let think of some tentaculate deposit-feeders for example). If they want to stick with this statement, the authors should clearly present better evidences supporting it, ... which apparently do not exist according to the authors themselves!. The use of stable isotopes does not allow for an assessment of food selection since: (1) their measure refers to assimilation and not to ingestion, and (2) in the case of deposit-feeders, it is almost impossible separating the pool of sedimentary organics into pure food sources that are submitted to selection. As far as nutrition is concerned, body size is probably more directly linked with trophic levels, which are not explicitly considered in the model. Second, in his 1980 paper, Cammen compared ingestion rates in various benthic invertebrates fed on different food sources. He (also used a log scale and) observed a “good” correlation between those rates and individual body size, thereby supporting the so-called “compensatory intake theory”. Other studies have been specifically dealing with the relationships between ingestion and food availability in single organisms fed on single food sources. I suggest that such studies may prove more relevant for this manuscript. Third, regarding ïĄą. the term absorption could be better suited than assimilation since it explicitly refers to the passage through the gut

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wall, whereas assimilation refers to incorporation in body tissues. The values of ïĄą could then be compared with reviews on absorption efficiencies of deposit-feeders (e.g. Lopez and Levinton 1987). The discussion on the relationships between ïĄą and body size mostly refers to absorption and is rather inconclusive. Besides gut size, which is likely related to body size, gut architecture, which is likely related to trophic levels, seems also important to take into consideration (see for example the body of literature by Penry and Jumars) as a controlling factor of gut residence time.

8. Discussion on predation 4.4. The concept of passive (versus) targeted predation should be better explained. The term deposit-feeders should not be used to qualify secondary consumers.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C1741/2014/bgd-11-C1741-2014-supplement.pdf

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