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***Interactive comment on* “Technical Note: Determining the size-normalised weight of planktic foraminifera” by C. J. Beer et al.**

Anonymous Referee #3

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I provided a preliminary review of this contribution, and many of my criticisms of that initial submission still stand. On a procedural level I am critical of labelling this contribution as a “Technical Note.” It should be evaluated, and accepted or rejected, as a full-fledged paper or not at all. The figures in the Supplementary Information absolutely should be moved in to the main paper, not left in an appendix (see my comment below), as they contain information important for evaluating the authors’ thesis. The paper still comes across as a confirmation of previous studies which have used sieve-based weights to infer changes in foraminiferal calcification. Indeed Supplementary Fig. 1a shows a high correlation ($r^2 = 0.91$) between SBW and MBW. By the way the y axis on the left-hand graph in Suppl. Fig. 1 appears to be mislabelled – it should read “(μg)” not “(μm)”. In particular the paper is not as thorough as it could be in fully comparing the proposed method of size-normalisation to samples whose weights have

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been estimated within sieve fraction, within the same sieve-size intervals. However, the manuscript shows there is within-sieve-fraction variability, which I don't think anyone denies, and which is a contribution. Indeed, a more thorough way to evaluate this source of variability would be to go beyond aggregate measures of mean shell weight, and evaluate the distribution of individual shell weights and shell areas within given sieve-fraction-constrained samples. An example of this type of analysis can be seen in Moy et al. (2009), Figure 3. Nowhere do I see SNW plotted against SBW to really see the correlation or lack thereof. This comparison would help resolve the essential questions here: which is more "precise," SBW or SNW, and is precision gained (or lost) by adopting a SNW approach? There's an implicit assumption in the paper that errors cited here would apply to the other data sets cited. Looking at both de Moel et al. (2009) and Moy et al. (2009) it appears both studies used far greater number of tests than the mean 19 test used here for their comparisons of mean shell weights so you'd have to go to similar numbers for a valid comparison. Without actually applying this suggested method to the other data sets cited, it's not clear how it would actually improve upon the reported error bars in those studies. So really the way to test this idea is to apply it to the samples analysed by Barker and Elderfield (2002), Moy, deMoel, and others. They would also have to use to same size fractions used by these other studies (e.g. 300-355 and 355-425 micron in the case of Moy et al.) or make clear the inherent assumption that the error within the 200-250 micron fraction applies to these other sieve fractions (who knows, it may be greater). The authors assert that "a recent analysis of the SNW-[CO₃] correlations suggests that SNW is a function of multiple, as yet undetermined, environmental controls and not [CO₃] exclusively (Beer et al., 2010)." But if this paper is going to explore the "multiple controls" idea (which may well be valid – I wouldn't be surprised) why not use environmental data from the area of the tows to examine those relationships? Indeed, a key problem for this paper is the implicit assumption that all the shell weight variability arises from with-sieve-fraction variability in shell diameter and thickness, with no accounting for environmental drivers. Yet the samples were taken from a range of latitudes (and depths?) and there's no indication of

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the between-site variability in environmental variables previously cited by these same authors as drivers of foraminiferal shell calcification.

The contribution claims to improve upon methods in which sieve-constrained shell weights are used, and the sieve fraction is taken as a means to control for shell size as a covariate of shell weight. Perhaps a fair criticism of the previous studies, but these authors admit “area and volume are assumed to remain proportional to one another over the size range of interest (Barker [and Elderfield], 2002). Although proportionality cannot be demonstrated (because of the absence of volume measurements) the strong correlation between area and diameter ($r^2 = 0.87$) lends support to the assumption of proportionality between the three measurements.” So it seems what they have done is transfer an assumption that shell size can be controlled for by sieve fraction to an assumption that shell size can be controlled by area. And for the taxa they are examining shell volume may well vary considerably even for a given area within a (difficult to achieve) homologous umbilical “view” of the test. Furthermore, even for perfectly uniform umbilical orientations, none of the taxa analysed are round in plan view, so “diameter” is not a meaningful dimension for these shells. The diameter was estimated as “the mean of the diameters which bisect the centre of the foraminiferal test, as observed in 2-dimensions.” But what are the statistical spreads of the means of these diameters, and when accounted, how much variability do they “add” to the normalisation? Finally all these taxa vary in thickness between spiral and umbilical sides so that there is another source of shell-weight variability not captured by this analysis.

Another potential flaw of the study is that in only using a 50-micron size window, it immediately adopts one of the assumptions it claims to challenge (that sieving doesn't do a good job of restricting size), unless they selected specimens between 200 and 250 microns by some means other than sieving. It's not made clear how they selected specimens from the 200-250 micron fraction, so if it was by some method other than sieving they should be explicit about that and I'd be happy to stand corrected on this point.

The shells were not treated for organic matter, and it's not clear they were fully dried so

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moisture content could add a great deal of variability. So it's not clear how comparable these results are to the studies these authors are addressing.

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