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Interactive comment on "Technical Note: Weight approximation of single coccoliths inferred from retardation estimates using a light microscope equipped with a circular polariser – (the CPR Method)" *by* J. Bollmann

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This is a very interesting contribution to the field of morphometry of coccolithophores. The use of circular polarization will help many researchers working in our field. It is however not a revolution since the use of polarization to measure the mass of coccoliths has been described in 2005 (Beaufort, 2005) and now it is commonly used. The title is ambiguous because it gives a name, the CPR method, which would suggest that it is a new method, although it is based on the same principle which is polarization. Since a few years I work with an equivalent system, which does not use circular polarization,

but uses a set of rotating polarizers (Beaufort et al., 2011) and gives the same results as proposed here.

A thing which bothers me more is the following: The relation given between the thickness of a calcite crystal and interference color (lightness received by the camera in cross-polarized light) is not precise enough to provide a good estimate of the thickness. The given estimate is not perfect for several reasons:

1- Here, Bollmann bases his calibration on the excellent paper of Sorensen 2013. In his paper, Sorensen fits the theoretical Âńspectral transmission matrixÂż (equation 3 in Sorensen 2013 - which gives the relation for every wave length of visible light with the CIE colour matching function (RGB for Red Green Blue)). Although this fits quite well, it is not perfect and explains the Âńsmall bumpsÂż in the black line of Fig. 1 of Bollmann. Bollmann should have used not the fit but the original matrix.

2- This curve is then fitted with a quadratic polynomial (4 orders) which cannot fit well the extremes of a sigmoidal function such as the relation between lightness and thickness.

These two problems result in a near maximal lightness at about 1.37 μm. The equation 3 of Sorensen gives for thickness=1.36 μm a value of I = 0.847 (average of all L values found at a wave length between 360 and 830 nm) when the I maximum is found for 1.555 μm with an average value of I = 0.847. This thickness = 1.555 μm corresponds to a retardation of 267 nm. This is the classical limit between maximum white and pale yellow in the Michel-Levy chart? (Michel-Levy and Lacroix, 1888). Therefore, using Sorensen's equation, the thickness will be underestimated by 13%. For example a disc of 3 μm in diameter seen by the camera at max light would then be inferred to have a thickness of 1.36 μm (instead of 1.55 μm) and have a volume of 9.6 μm3 which corresponds to a mass of 26 pg, when it should be 11.0 μm3 and having a mass of 30 pg. Therefore this error on the thickness results in a significant and systematic underestimation of the mass.

Bollmann heavily critiques the method I proposed in 2005: 1- Quote: ÂńMost weight estimates reported by Beaufort (2005) appear to be higher than the values reported here even if the maximum coccolith length for a given species is assumed.Âż This is not true. The table 1 in Beaufort (2005) shows good agreement between the different methods. The differences are in the order of intraspecific variation observed since then. 2- The transfer function would not be valid because I used Âńparticles that are outside the valid range of 0 – 1.56μm thickness (please note that a maximum particle thickness of 1.56μm was given by Beaufort, 2005)Âż. The particles used for calibration are longer but not thicker, and are flat-lying on the slides. There are many other critiques that I do not share but it would be too long to discuss here (in particular the orientation of the particles which is one of the strengths of this method rather than, as he describes it, a flaw).

What is important is that I used a maximal thickness value of 1.56 μm and he uses a 1.36 μm as a maximum.

In the case I am wrong and he is right (I doubt about that) all the published data with my method should be reduced by a certain factor. The relative variation of mass in and between samples, however, would not be affected. Then the method of Beaufort (2005) should not be as bad as invalidating the result that it produces: Recently two papers in BGD got very severe critiques because they used my method (Bauke et al., 2013; Horigome et al., 2013) and at least one of those came from Bollmann. This is not acceptable especially when we know that the error is in the present manuscript, which is still under peer review.

Finally and less importantly, the author does not give any clue how he tunes the light of his microscope... how does he know when the bulb is aging and how it effects the results?

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