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Interactive comment on "A global diatom database – abundance, biovolume and biomass in the world ocean" *by* K. Leblanc et al.

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This manuscript represents a tremendous amount of work to compile diatom abundance data and to convert them into diatom biomass using a single standardized method. The authors objectively present uncertainties and biases inherent to such huge dataset and present a first order estimate of global diatom biomass. However, I think that the spatial distribution of the data points does hinder the calculation of a really convincing estimate of global diatom biomass. The North Atlantic and GIN seas, where diatom abundance and biomass are not so high, are well covered from the coastal to the ocean realms. Other oceans are not adequately covered and the specific distribution may overestimate calculation of oceans' diatom biomass. Data points in the Indian Ocean and the Pacific Ocean are from the upwelling systems where diatom biomass

is much higher than in the nearby Indian and Pacific tropical gyres. Globalization of upwelling estimates to the whole Indian and Pacific oceans may lead to the enormous diatom biomass in these oceans (Table 4). Conversely, absence of coastal data from the Southern Ocean may strongly underestimate diatom biomass there (table 4). I wonder whether studies by Hasle (1969) in the Pacific sector, Ligowsky (1982, 1983, 1986, etc...) and Beans (2008) from coastal Antarctic Froneman (1995) from the Atlantic sector or Annett (2010) from the Antarctic Peninsula (to cite a few publications from the SO) were used in this study. Many studies of seasonal diatom abundances based on sediment traps from locations not covered here also exist (see review by Romero and Armand, 2010). It seems that the present study mainly used large datasets but disregarded single localization studies; at least I could not find the reference citations in the main paper or the supplementary material; which could have greatly improved quantitative estimate of diatom biomass.

Si biomass is calculated from the conversion of C biomass using Brzezinski empirical Si/C ratio. This conversion presents a two-step uncertainty. First, as depicted in the manuscript, the diatom C biomass calculated here presents huge uncertainties. Second, recent studies have shown the diatom Si/C ratio is highly variable depending on the biological and physiological state of the diatoms. I think it would be more accurate to estimate Si biomass from the diatom surface area by assessing a Si quantity by surface unit but taking into account species specific frustule ornamentation and thickness. Smetacek's group may have worked on this issue.

One of the motivations of the present manuscript is 'to help validate biogeochemical ocean models by using carbon (C) biomass derived from abundance data'. Buried diatoms are also very important in biogeochemical cycles as they represent a large sink of Si and C. A comparison to diatoms in surface sediments would be extremely interesting and could represent a second step in another paper. Large surface sediment diatom dataset with good coverage exist that could be readily used (Sancetta, 1982; Zielinski et al., 1997; Romero et al., 2000, 2003, 2005, 2009; Jiang et al. 2001; An-

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dersen et al.2004 ; Armand et al., 2005; Crsota et al., 2005; Abrantes et al., 2007; etc . . .).

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