

## ***Interactive comment on “Global database of surface ocean particulate organic carbon export fluxes diagnosed from the $^{234}\text{Th}$ technique” by F. A. C. Le Moigne et al.***

**M. Rutgers van der Loeff (Referee)**

mloeff@awi.de

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This paper does exactly what the title tells: it presents published POC export data based on the  $^{234}\text{Th}$  technique. It is very good to have all these data collected and in this way obtain an impression of the geographical distribution of POC export rates measured with this technique and also of areas where data are lacking. The paper follows the Longhurst biogeochemical provinces, which is a good approach. However, the discussion on the global distribution of fluxes is rather superficial. The paper mentions “global patterns . . . with highest daily POC export rate occurring in the high latitude North Atlantic, the Arctic and the Southern Ocean”, giving the impression that POC ex-

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port is just a matter of latitude. It then discusses two exceptions (one of them a study with just one station) with high export at low latitude. But it fails to mention that the Boreal Polar province is now represented by 72 stations, including high-export shelf seas and extremely low export areas in the central Arctic Ocean. This contrast within one province should at least be mentioned. It is one more example showing that it is not possible to explain export or export ratio (see comment page 170 line 7 and Fig. 1 in Henson et al., 2011) from latitude or temperature alone.

I wonder why the authors did not include in this dataset the 234Th export rates and the POC/234Th values used. That would make the dataset much more useful. The 234Th export data are more robust than the POC export data because of the large uncertainty in the POC/234Th ratio (an uncertainty clearly mentioned in the paper). If future studies would find new ways to estimate distributions of POC/234Th ratios in sinking particles, they would have to repeat the data mining performed by the present authors.

Technical issues: 166 last line. P is once expressed per m<sup>2</sup>, the second time incorrectly per m<sup>3</sup>. 167 line 5 “a non steady state (NSS) model can be applied. The NSS model can be useful. . .”. I think it is the other way round: if we only have one profile, we must give arguments that it is valid to use a steady state assumption (see Savoye et al., 2006). 168, line 20. The figure is not cumulative. References: The Longhurst 1991 reference appears not to be the appropriate reference for the definition of the provinces. Buesseler 1998a and b are identical. The Rodriguez and Baena (2008) study deals with the Antarctic, not with the Arctic as listed in Table 1.

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