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Discussions



# *Interactive comment on* "Sinking rates of particles in biogenic silica- and carbonate-dominated production systems of the Atlantic Ocean: implications for the organic carbon fluxes to the deep ocean" *by* G. Fischer and G. Karakas

**Anonymous Referee #1**

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### General comments

This manuscript presents an interesting compilation of sediment trap data collected in the Atlantic ocean, to derive information on particle sinking rates, discussing the relative role of siliceous and carbonate producers as carriers of carbon to the deep. Although the approach (estimation of sinking rates with different methods, combination of satellite data, sediment traps and model results) is very interesting, I do not believe the conclusions derived by the authors are fully supported by their analysis. I try to explain



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why below. I believe this manuscript deserves another chance after major revisions. Revisions should include (1) a more neutral presentation of topics which are presently debated in the literature (faecal pellets versus aggregates, carbonate versus siliceous organisms), (2) statistics to fully support their first conclusion of "highest sinking rates in carbonate-rich systems" and (3) a better use of the sediment trap data base and modelling, with sensitivity analysis conducted on the different parameters controlling carbon transfer efficiency and not only, fitting of sediment trap data by playing only with sinking rates (and deriving conclusions only on sinking rates, and not on remineralization for example). Below, I start with these three general comments, before providing additional, less important, comments.

1. It seems to me the authors have chosen to present faecal pellets as sinking more rapidly than aggregates. They cite studies with very high sinking rates for faecal pellets and lower ones for aggregates. Although this may be correct, I think it would be more appropriate to recognize that we simply don't know or more positively, that this is still a matter of debate in the literature. Deposition of phytodetritus, mostly aggregates, has been observed on the seafloor of all major ocean basins (Billett, 1983 for the Atlantic; Smith et al., 1996 for the Pacific etc) and they have been estimated to reach the seafloor sometimes probably within a few days (in the Southern Ocean, see Riaux-Gobin et al., 1997). Similarly, not all pellets do sink rapidly and for example, there is debate about the sinking rate of copepod faecal pellets. And these pellets are subject to numerous processes (coprophagy. . . ) which may also decrease their sinking rate during sinking and participate in the overall retention of particles in the top 100 m. All I mean is that care should be taken to present the complexity of these mechanisms, rather than going straight in one direction only, right from the beginning.
2. I would say the same concerning the debate as weather diatoms or coccolithophorids transport carbon to the deep. Again, right from the beginning (lines 10-15 of p 2544), but also several times during the reading (e.g. p 2550, lines 20-



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25; see also below, my comment below on Figure 4d), coccolithophorids appear as the main carrier, based on studies from François et al. (2002) or Klaas and Archer (2002), in line with the discussion on faecal pellets versus aggregates. But this is also debated in the literature, and not really supported in this study. It is well-known that most phytodetritus aggregates mentioned above are mostly constituted of fresh diatoms, that the role of diatoms in carbon transport has been demonstrated recently in the Pacific (Buesseler et al., 2007). The use of global data sets to derive global algorithms (François et al., 2002), without looking carefully at temporal or spatial variations, has been questioned as well (Ragueneau et al., 2006). None of these studies are being cited, giving this impression (which is probably not intended) that the authors are already convinced that carbonate rather than diatoms are the key players. Again, I think that to present the interest of this manuscript, it would be more useful and more accurate to stress that we want to understand better the regional and temporal variations in the efficiency of the biological pump, which sometimes or somewhere, seems to be mediated more by siliceous organisms and some(other)times and somewhere else, more by carbonate organisms.

This is important I think because it seems to me that the results and conclusions of this paper are extrapolated too far. I will have a word below concerning this problem in relation with the comparison between model results and observations and the conclusions that are derived concerning sinking rates. But in relation to this debate concerning diatoms and carbonate (the authors are correct in stressing that one should not mix all carbonate organisms), the results from Figure 4d are far from being convincing. From this figure, one can not say that sinking rates are lower in the diatom-dominated area. First, there is only one data point in this area (although there are three traps, why?) and second, it falls in the middle of the data points appearing at lower latitudes. Third, I could easily find examples where much higher sinking rates have been reported in the Southern Ocean, with diatoms as the main carrier (again, Riaux-Gobin et al., 1997, but see also more recent results from the AESOPS transect), so that these data points could be way above the sinking rates presented as higher in the carbonate-dominated region.

3. Perhaps, my major comment is linked to the comparison between model results and deep water fluxes measurements (Fig. 9) and the general philosophy of this comparison. As I understand it, model results match reasonably well measured fluxes with a sinking rate of 75 m $d^{-1}$ during winter and spring and 150 m $d^{-1}$ during summer. Conclusions are then made on the importance of seasonal variations in sinking rates, which seems logical. However, as stressed by the authors themselves (p 2543), carbon transfer efficiency depends upon the production in surface waters, the sinking rate of the particles relative to their decomposition rate. In this study, they have played with sinking rates, keeping the remineralization constant. Had they been playing with remineralization rates, keeping sinking rates constant, they may have also been able to reproduce sinking fluxes reasonably well and would have discussed the importance of seasonal variations in the quality of the sinking particles (change in phytoplankton composition, change in carbon lability or silica dissolution rates etc. . . ). I will come back on this important point at the end of the following section.

## Detailed comments

P 2542.The ranges of atmospheric CO2 concentrations and the impact of the biological pump on these concentrations may be confusing. Without mentioning the range given which does not seem realistic with today's human influence, it also suggests when talking about "future climate scenarios" (line 25) that the biological pump may have an influence on short time scales, which is not the case.

P 2543. The range of e-ratios (0.05-0.25) given line 3 is not correct. Boyd and Trull (2007) typically report values that can be lower or much higher, especially during bloom periods (see also Buesseler, 1998).

P 2543. On the same line, ranges given for sinking rates are not presented in a consistent manner. Line 13, a mix of faecal pellets and aggregates sinks with velocities typically between 50 and 250 m $d^{-1}$. Line 10-15 of p 2544, much wider general ranges are given for both faecal pellets and aggregates.

P 2546. It is not mentioned weather flux calculations have accounted for degradation or dissolution in the cups of the traps (see Ragueneau et al., 2001; Antia et al., 2005).

P 2546. In the paper by Yu et al. (2001), it is shown that trapping efficiency can be quite low below 1000 m and even 1500 m. Why looking at traps between 700 (and even lower, see Table 1) and 1000 m ?

P 2548. For a study looking at the relative role of diatoms and coccolithophorids in the biological pump, how to justify using only one phytoplankton in the model and no influence of silicic acid on diatoms for example?

P 2548. Remineralization rates are chosen but no reference is provided. Same for coagulation. The authors make a difference between remineralization rates for small and large particles, why? Such a difference, also by a factor 3, has been observed for silica dissolution rates in single cells versus aggregates so it might be correct but is this choice based on that study (Moriceau et al., 2007) or any other?

P 2550-2553, section 3.1. This section is rather long and the message is not so clear. Higher sinking rates are found in carbonate-dominated regions (from Fig. 4d, which can be questioned, see general comment), but no relation was found between sinking rates and carbonate content (line 23 p 2551). It is not clear as well, p 2552, weather lithogenic contents influence or not sinking rates. We understand that slow sinking of diatom aggregates in the Southern Ocean may be more due to lateral transport than to intrinsic properties of the aggregates (lines 10-15, p 2553). But we also see that low rates can also be encountered for coccolithophorids, for similar reasons (following



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paragraph). Finally, the authors acknowledge the fact that coccolithophorids rather than total carbonate should be used in global relationships between carbonate and organic carbon (a distinction not done in the study of François et al., 2002) and in fact, sinking rates of the primary producers appear to be lower. At the end of the paragraph, we are left with the impression that yes, sinking rates estimated are in the order of values found in the literature but literature values cover all ranges from 1 m $d^{-1}$ to $>$ 1 km $d^{-1}$. And we do not know if the wide range of values estimated is due to the approach of estimating them, or to the high variability, spatial and temporal, that exists out there. Maybe, by describing first the results obtained and only then, discussing these rates with clear concluding remarks, would help us understand better the message of this section.

P 2558, lines 15-18. The authors stress the fact that despite the fact that the model has only one phytoplankton and particles sinking in spring and summer have different origin, the model is able to reproduce flux patterns reasonably well, simply by playing with seasonal changes in sinking rates. This brings me back to my last and perhaps most important general comment because I would worry about such a result. Also, one can not apply a model at a given site and fit the data by changing one parameter, without testing the effects of changing the other parameters (such as the remineralization rate, see general comment). They would do the same at another site, find a different sinking rate. Then what? I believe that what is most needed in this case, would be an empirical relationship linking sinking rates to some environmental variable (be it the temperature, PP, carbonate content or whatever) and testing such a relationship with the same model, at different sites. Can such a relationship be derived from this study? Perhaps, statistics could be applied to the data set to derive such a relationship, and then include it in ROMS and test it at one or two sites?

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Interactive comment on Biogeosciences Discuss., 5, 2541, 2008.