Reply to editor,
May 8, 2018

Dear Editor,

We received comments from two anonymous reviewers about our BGD submission "Inferring particle dynamics in the Mediterranean Sea from in-situ pump POC and chloropigment data using Bayesian statistics (BG-2018-6)". The first reviewer was concerned that "the whole text is largely driven by methodological aspects" and questioned two model assumptions (steady state and linear aggregation). The second reviewer submitted the exact same comments as in the "Access review", which we had already replied to. We have posted our comments on line, but they were already addressed in the revised manuscript that is now posted. Perhaps he accidentally uploaded the wrong file?

Before we answer the first reviewer's key questions, we should restate the purpose of this study. First, we introduce a new Bayesian method to infer particle dynamic rate constants from new geochemical tracers (chloropigments) sampled using commonly used large volume pumps. Wang et al. (2017) suggested the use of chloropigments as new tracers to model particle dynamics, which opens a new door to the community. In that study, chloropigments were sampled using settling velocity sediment (SV) traps. However, SV traps have not been widely used due to lack of the commercial availability. In contrast, large volume pumps are widely used, but there is no method for using chloropigment data from large volume pumps. This study presents such a method. Second and more importantly, by comparing results obtained using thorium and chloropigment tracers collected using the same sampling technique, and using the same tracer but different sampling methods, we found that the tracer has a greater influence on parameter estimations than does the sampling method. Overall, either of the aspects deserves publication in BG.

Now we answer the two key questions raised by the first reviewer. 1) "the whole text is largely driven by methodological aspects", we actually just replied to this question: developing this method is what the study aimed to do. 2) "linear aggregation and steady state". The reviewer does not believe that the "linear aggregation" and "steady state" assumptions are valid. First, we argue that linear aggregation is not without merit, especially when aggregation is between the same kinds of particles. As the reviewer stated in his/her comments, "particles have to meet in order to aggregate". Therefore, aggregation has to be related in some way to the concentration of particles, no matter whether the particle concentration is high or low. By assuming a first-order reaction, we have

$$\text{aggregation} = \alpha[\mathrm{P}],$$

where α is an aggregation rate constant, and [P] is the particle concentration. Higher particle concentration results in a higher chance to collide, and thus more aggregation. In most empirical studies, including with the current data, we have no way to build and test a complicated model such as that in Jackson (1990). Also, unlike in Jackson (1990), we are dealing with particles below the euphotic depth, so do not consider algal division and other euphotic zone processes. Many previous models have assumed first-order reaction kinetics and show reasonable results

(Murnane et al., 1990; Clegg and Whitfield, 1991; Marchal and Lam, 2012; Lerner et al., 2016, 2017). Second, we assume "steady state" since we are using only one concentration profile. We do not have enough information to build a non-steady-state model. To achieve our second goal, to compare the influence of sampling techniques and tracers on parameter estimations, we have to make the same assumptions as we did in previous studies (Wang et al., 2016; 2017).

We realize that there will always be disagreements among researchers, and certainly particle dynamics has more than its share of "sides", but we feel that we have addressed the reviewers' major concerns and hope that you will agree.

Sincerely,

Weilei Wang