

Interactive comment on “Modeling the effects of size on patch dynamics of an inert tracer” by P. Xiu and F. Chai

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Reply to Referee #1 Comments

Thank you so much for the valuable comments. For conveniences, we have repeated the referee's comments and followed by our responses.

This paper considers the evolution of a passive tracer in a high-resolution ocean model. The tracer is released in a finite region and its subsequent distribution is analyzed. The influence of initial size of the tracer patch on the subsequent evolution is examined. The study is motivated by iron enrichment experiments. The key results of the paper concern the dilution rate of the tracer, for which two regimes are identified, one linear and one exponential.

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With some additional work and more careful writing of the paper, I believe that this could be an interesting study, although the applicability of the results for iron enrichment experiments is limited by the use of a passive tracer. (Also, I'm not sure it is really true to say that the finite duration over which ships can observe the fertilized patch is the *main* difficulty in terms of extrapolating the results to regional and long-term impacts, since there are other difficulties as well, see e.g. Boyd et al., 2007.)

Response: Thanks for pointing this out. We have modified that as follows, around page 3, line 3:

“One important issue is due to the limited observational capabilities and logistic constraints that the research vessels can only remain within the iron patch for 30 to 40 days, which is usually not long enough to observe the complete impact of iron and the entire iron patch dispersion process (Buesseler and Boyd, 2003; Boyd et al., 2007; Chai et al., 2007).”

The description of the simulation needs to be expanded - in particular with reference to the tracer. The details of the initialization are a little vague, and there is no explanation of the numerical diffusion acting on the tracer, nor the scheme used to evolve the tracer.

Response: We have modified that part as follows, around page 6, line 11:

“To investigate patch dynamics, an inert tracer (IT) is introduced, which is controlled only by circulation and diffusion. The behavior of this IT is very similar to the SF6 tracer used during the iron fertilization experiments (Law et al., 1998; Boyd et al., 2004; Law et al., 2006). In the physical model, a nonlocal vertical-mixing scheme called K-Profile Parameterization (KPP) was used for viscosity and diffusivity (Large et al., 1994). The KPP scheme does not assume a priori that the boundary layer is well mixed and explicitly predicts an ocean boundary layer depth. The turbulent mixing is parameterized using a nonlocal bulk Richardson number within the boundary layer and through the local gradient Richardson number and a background mixing scheme below the boundary layer. Our model experiments include six different initial sizes of IT patch

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(Table 1) in the eastern equatorial Pacific Ocean (3oS, 90oW), close to the locations of the IronEx I and IronEx II experiments. For each model experiment, IT tracer was continuously added into the upper 30m during 14 days starting from January 7, 2004. After 14 days injection of IT, first we normalized the IT concentration with respect to IT0 (the highest concentration of IT at day 14), then defined the IT patch as where the normalized IT concentration (denoted as the IT concentration hereafter) is higher than 0.1 (0.1 versus $1/e$ used by Law et al., 2006). By doing so, this allows us to compare results among different sizes of the IT patch. Finally, after 14 days injection and normalization, we followed the movement of the patch to study its dilution behavior."

The tracer is normalized by the peak concentration at the end of the injection period and then the patch is defined as the region with concentration higher than 0.1. As noted by the authors, Law et al., 2006 (and other papers, e.g. Lee et al., 2009) use a value of $1/e$ instead. The reason for choosing 0.1 is not given. More crucially however, previous studies considered the evolution of the area within the contour $c=m/e$, where m is the peak concentration in each period, whereas instead here the contour is fixed at value 0.1. The results presented in this paper are interesting, but I think the distinction between the choice of the contemporaneous peak or initialization peak as the reference needs to be pointed out clearly since the results are quite different. Moreover, I think the paper would be strengthened, both in terms of clarity and impact, if the authors calculated the more standard area based on the contemporaneous peak and then discussed the behavior of both results in relation to the original Garrett description.

Response: Thanks for pointing this out. This study is motivated by past iron enrichment experiments in the equatorial Pacific. We try to simulate a real physical condition that drives the tracer patch moving around. The contemporaneous peak mentioned here considers the evolution of the area within the contour $c=m/e$. As indicated by Lee et al. (2009), the contour $c=m/e$ encompasses about 63% of the total tracer load when considering a two-dimensional case, which will not change during the patch evolution time. Fig.1 (a) shows the time evolution of the area within the contour $c=m/e$. Just as

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we can imagine, the area increases all the time if we fix the total tracer load to a certain level due to the decreasing tracer concentration. However, in the iron enrichment experiments, people might be more interested in the iron concentration but not the total iron load within the area, because phytoplankton growth could be strongly stimulated only when the iron concentration reaches a certain level. Thus, we focus on the initialization peak in this study. Another reason why we use initialization peak is for different model simulation comparison. By doing initialization peak, we can compare our model results among different initial patch sizes by using the same standard (the only difference of the model experiments is the initial patch size).

There are two reasons why we use 0.1 but not $1/e$. The first is that we want to study the total patch area but not just the core. As indicated by Law et al. (2006), they use m/e to indicate patch center but $m/7.5$ to represent total patch area. Also in Lee et al. (2009), they also use a value of m/e^3 to represent an area which encompasses about 95% of the total tracer load. The second reason might be due to the intrinsic characteristic of our experiments. Fig. 1(b) shows the time evolution of the area within the contour $c=1/e$. Note that the consequence of defining the patch to be $1/e$ is that the region with tracer concentration lower than $1/e$ are treated as ambient waters. The reason why we only see the decreasing phase in Fig. 1(b) is probably caused by the high concentration ($<1/e$) in the ambient waters. However, in the iron enrichment experiments, the ambient waters always stay iron-limited. Therefore, $1/e$ is too high for our experiments. But we also can't just choose the zero contour line as the tracer patch, because it's not consistent with iron enrichment situation and the area of zero contour line will keep increasing during the time evolution. Also, since our model experiment is only run for one year, if we choose a relatively low value like 0.01, we will not see the decreasing phase of the surface area or total volume within one-year study. Thus, 0.1 is chosen from our many trial experiments to better mimic the real iron enrichment experiments and to illustrate the point of this study.

Throughout the description of the technique used, terms are introduced without being

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defined. For example, x, y, width, length.

Response: We have added the following descriptions in the manuscript. page 7, line 13 “The x is the major axis of the ellipse while y is the minor axis. $\sigma_x(t)$, $\sigma_y(t)$ are the standard deviations of the distribution in the x and y directions, respectively.” Page 9, line 17 “Previous studies showed that, in the first few days during the patch expansion, length (major axis, $2\sigma_x$) of the fitted Gaussian ellipse is observed to increase exponentially with time in response to the strain rate (γ), while the width (minor axis, $2\sigma_y$) of the fitted Gaussian ellipse remains constant, where the thinning effect of strain controls the widening tendency due to diffusion (Abraham et al., 2000; Law et al., 2006).”

The results find little difference in the strain rates for the different initial patch sizes for the first 32 days, but a difference in the dilution rate. I wonder, however, how much the dilution rate is influenced by the choice of 0.1? Looking at fig 1, it seems that for the larger initialization patches, a thin filament is pulled around an eddy during this time period and may be influencing the results: the result for a concentration value encompassing more of the tracer, e.g. 0.05, might give a result independent of initial patch size as for the strain rate.

Response: Please note the dilution rate used in this study is defined from eq. (2) in the manuscript as $r=(A(t+dt)-A(t))/(A(t)*dt)$. It is slightly different from the strain rate. Strain rate evaluates how σ_x changes with time. This rate r is a normalized value and the unit is d⁻¹. It tries to evaluate how the area change varies with time. As we stated in the manuscript, σ_x changes with time at a constant rate, but σ_y doesn't, which leads to a different dilution rate from the strain rate. The reason we choose this dilution rate r, is that we try to minimize the impact due to the different choices of patch definition like 0.1 or 0.05. As shown in Fig.2, the values for 0.1 patch are a little different from the 0.05 patch, but we still can see that the dilution rate is initial size dependent, which is the focus of this study. Also, as we stated in the previous responses, since our experiment is only run for one year, if we choose a relatively low value like 0.01 or 0.05, we will not see the decreasing phase of the surface area or total volume within one-year model

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simulation. Thus, 0.1 is chosen from our many trial experiments to better represent the real iron enrichment experiment and to illustrate the point of this study.

It would be interesting to see the results for 2005 - at present they are only alluded to with statements like 'the dilution rates during the first stage are mostly higher than zero', which hint that there is considerable variability. Indeed this is what I would expect for the smaller patches where a considerable difference in the initial evolution might be expected according to whether the initialization were within a coherent structure (i.e. an eddy) or not. Indeed, overall, I would expect there to be essentially two regimes of behavior, one for initialization patches that are below the eddy scale and one for patches that are above.

Response: This is an interesting point. Fig. 3 shows the results for 2005 case. We can see that during the first few days when the patch size is below or around the eddy size, 12.5km, 25km, and 50 km patch almost have a similar dilution pattern, which is significantly different from the larger patches, indicating the existence of two regimes of behavior. If we look back at the 2004 case closely, we still can see the two regimes. Therefore, we added the followings in the manuscript, around page 11, line 21:

“During the first few days, the smaller patches (12.5×12.5 km, 25×25 km and 50×50km) of which the length scale is below the eddy scale have a similar dilution pattern, which is different from the behavior of larger patches (200×200 km and 400×400km). “

Finally, the link with phytoplankton blooms is a little tenuous given the absence of biogeochemical processes. Even with very simplified representation of such processes, radically different behavior can be observed according to the size of the initialization patch (Neufeld and Haynes, 2002; see also Elliot and Chu, 2003). This should be noted in the final discussion and perhaps less emphasis should be given to iron fertilization experiments as motivation and more to passive tracer injection experiments.

Response: Thanks for the suggestion. We have modified the manuscript as follows,

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around page 15, line 9:

“We understand that only physical processes are included in this study, however, biogeochemical processes may change the behavior of patch dilution significantly (Neufeld et al., 2002; Elliott and Chu, 2003).”

Figure captions:

Fig.1. Variation of surface patch area with time for 2004 case. (a) considering the patch to be $c=m/e$; (b) considering the patch to be $c=1/e$.

Fig.2. Variation of the dilution rate defined in eq.(2) with time. The left figure is for the patch that is defined as concentration higher than 0.1; the right figure is defined as higher than 0.05.

Fig.3. Variation of the dilution rate defined in eq.(2) with time for 2005 case (patch is defined as 0.1).

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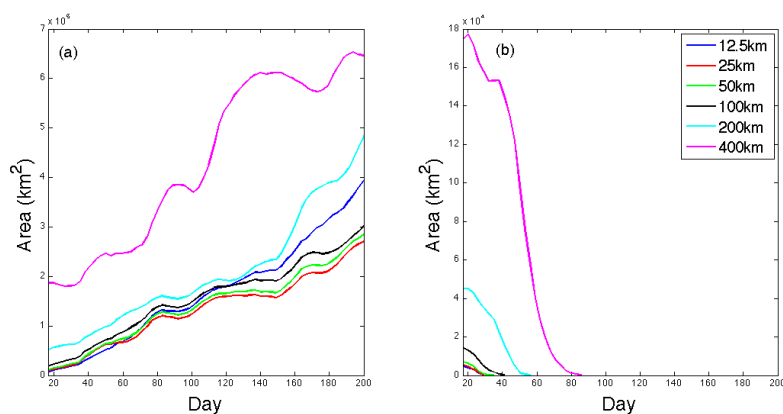


Fig. 1.

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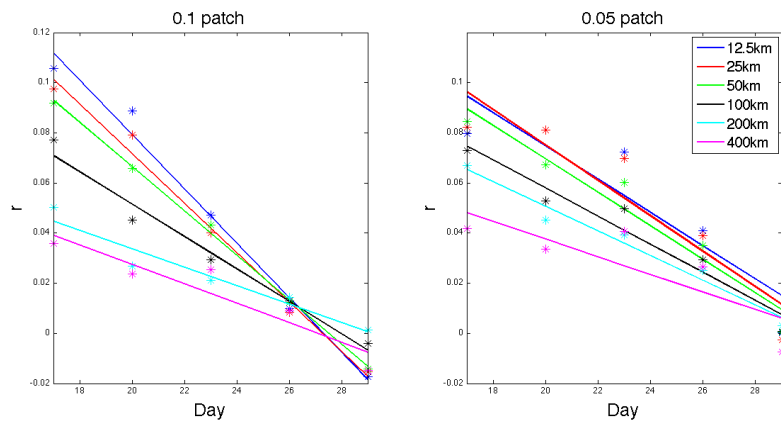


Fig. 2.

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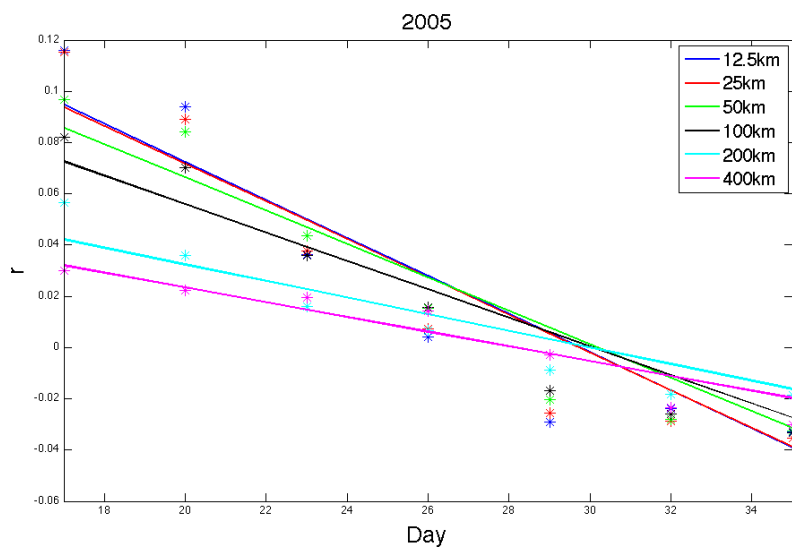


Fig. 3.

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