Review of Rouiller et al.

General comments:

This manuscript by Rouiller et al. presents a novel dataset and interesting hypotheses concerning the transformation and export of particles settling through the oxygen minimum zone (OMZ) of the Arabian Sea. Deep particle size and abundance observations were collected using an emerging optical technique, the Underwater Vision Profiler (UVP), and few data of this type exist at all, let alone in OMZ regions of the ocean. The authors use a particle tracking model to estimate source regions for settling particles, and to rule out significant horizontal advection as the source for large- and small-sized particle maxima observed beneath the OMZ layer. They finally conclude that zooplankton-mediated processes may enhance deep carbon flux in OMZ vs. non-OMZ areas.

I have two major concerns with the observations and modeling results presented, which the authors must address in order to better support their conclusions.

First, it is not clear that the *velocity* outputs from the circulation model the authors used to drive their particle-tracking model were sufficiently validated for the sampling time period and location. This makes it harder to support ruling out horizontal advection as the source of particles observed beneath the OMZ.

Second, the authors base their conclusion that enhanced export is occurring beneath the OMZ primarily on their observations of enhanced large-particle abundances in this layer. However, the data presented in the manuscript (Table 3 size-spectrum slopes, Figs. 8, 9, and 11) do not clearly show large-particle maxima at these depths. Some stations do show enhanced maxima in beam attenuation, which is due to small particles, but I would not expect small particles to strongly enhance export, at least not with the necessary settling velocities to rule out advection from beyond the OMZ.

Specific comments:

p. 19274 section 2.4: Please briefly describe the geography of the Arabian Sea and the seasonal period during which the cruise took place.

p. 19275 lines 16-19: How long before and after the reported transect was the O2 sensor calibrated? How did you ensure the sensor did not drift? What do you mean by "default"? (perhaps the wrong word?)

p. 19276 lines 1-6: In general, this DI concept makes sense, but it is sensitive to the magnitude of biological oxygen production in the euphotic zone, and ignores the possibility of horizontal advection. I am concerned that oxygen consumption in the OMZ might not be best defined according to O_2 concentrations directly above. Is there a consistent deep-water value that might be more appropriate?

p. 19276 lines 10-12; p. 19277 line 23-p. 19278 line 6; and Figures 8 and 9: Nowhere do you provide counting uncertainty or details about your binning/averaging procedures for the UVP-derived observations of LPM. Please give these details and also report the uncertainty on your measurements. To clarify what I mean by counting uncertainty: if the UVP sample volume is

100 cm^3, then one cannot accurately quantify fewer than 10 particles/L without binning the data. The LVP profiles in Figures 8 and 9 may require depth binning in order to reduce the uncertainty, unless the UVP sample volume was large (state its size) or averaging multiple casts at each station was sufficient. If the latter approach has already been taken, please include the uncertainty in particle abundance profiles.

p. 19276 lines 12-13: "Only images from recognizable objects larger than 500 μm were sorted using an automatic classification followed by manual validation." It is hard to understand what is meant by this sentence – were particles larger than 500 μm only counted if they were recognizable? Is there a separate data set consisting just of this "recognizable" subset of >500 μm? If the latter, one would think that this dataset might contain information about zooplankton that could be used to substantiate subsequent discussion.

p. 19278, lines 2-3 and Figure 9: If particle counts have been normalized to bin width then the axis labels in Figure 9 should read [#/L/micron], not [#/L].

p. 19278 lines 8-9: Since you did not directly measure carbon or mass flux and instead use a (globally-tuned) model to assume settling velocity as a function of particle size, there are some important caveats with your flux estimates that you ought to address.

p. 19278 lines 9-10: What do you mean by "profiles of flux were adjusted by the Martin Model"? I assume that you mean you fit the Martin model to your flux data? Please clarify this. Also, the Martin model is often not a good one for measured flux profiles (including many of yours, particularly those where modeled flux increases with depth). You should better justify its use.

p. 19278 line 14: Why z_{ref} = 50 m (rather than euphotic zone depth)? Your stations have deep fluorescence maxima that vary from 22-73 m. Using a fixed reference depth for the Martin curve fit among all stations may only exacerbate problems with the poor fit of that model to your data.

p. 19278 lines 23-26: 1) Do you mean that 3 years of data were used in the validity comparison to T,S climatology? It does not make sense that you would use outputs from 2009 and 2011 to drive a particle-tracking model for comparison specifically to 2010 observations. 2) I'm not sure if it is sufficient to assume that a model correctly reproducing T and S relative to climatology will also correctly reproduce horizontal velocity, which is the relevant parameter for your particle tracking model. I think additional validation of the velocity field is necessary. Could you compare 2010 output to shipboard ADCP, local Argo trajectories, etc?

p. 19279 line 18 (also p. 19286 lines 15-16): That a deep particle abundance maximum exists in every station in the UVP data is dubious. It is not immediately apparent in Figures 8 or 9. Particularly after accounting for counting uncertainty in the UVP data, it may become even less clear.

p. 19282 line 11: do you mean "decrease by a factor of two" rather than "decrease by two"?

p. 19282 line 12: 30-60 particles per square meter integrated from 0 to 100m seems much too low. Figure 8 shows LPM ranging from 2 to more than 30 *per liter*. Is 30-60 particles m^{-2} a typo? Did you mean in the "maximum" layer only? (and if so, how thick was the integration depth interval around the maximum?)

p. 19283 line 20 – p. 19284 line 2: Can you move this part of the text earlier in the paper, perhaps to lead off the Results? It helps orient the reader to the rest of the results.

p. 19286 line 24-25: If you can more rigorously show that your model reproduces horizontal currents correctly (as discussed above), it will strengthen this conclusion.

p. 19287 line 25-27: As it stands, the only clear, deep increase in particulate matter is in the beam attenuation signal corresponding to small particles, and these are just as easily explained, in the absence of advection, by abiotic mineral precipitation at the edge of the oxic layer and by increased bacterial biomass. Unfortunately the LPM data, as presented, do not unequivocally support your conclusion here.

All figures with gray oxygen overlay: Please add an O_2 axis.

Figure 8 caption: Please note the scale difference for c_p as compared to Fig. 5.

Technical comments:

There are many minor grammatical errors and poorly-constructed sentences and paragraphs in the manuscript, which should be corrected before the paper is published in final form. The authors should carefully evaluate their text for proper usage.