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Interactive comment on "The sensitivity of primary productivity to intra-seasonal mixed layer variability in the sub-Antarctic Zone of the Atlantic Ocean" *by* W. R. Joubert et al.

Anonymous Referee #1

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General Comments: This paper presents a very interesting dataset from an important but relatively undersampled part of the Southern Ocean. It follows through on a line of analysis that is interesting but, in my view, is too narrow to fully put the processes considered into context, and consequently possibly exaggerates their overall importance.

Specific Comments: The paper introduction goes through arguments about iron and light limitation in the Southern Ocean. The suggestions that the Southern Ocean is generally light limited in summer, though referenced, are at odds with the small scale blooms seen downstream of islands and shallow sediment (eg see Fig 1 or several other references). These blooms are not associated with locally increased light levels. The blooms have short characteristic length scales, typical of ocean flow whereas light

levels, due to their forcing, generally change over larger length scales typical of atmospheric variability. The analysis (and later calculation) of mixed layer depth, focusing on convective overturning and temperature differences, is appropriate for north of the Polar Front, but not the whole Southern Ocean.

In the methods, it would be nice to know the density difference equivalent to the temperature difference used. Using a delta-T criterion is understandable given the use of XBTs, but a delta-sigma value is needed for comparison to values south of the polar front, where delta-T can't be used due to the dominant effects of salinity on density. The assumption of O2/Ar being in steady state (requires steady conditions for 10 days, pg 4340, line 6) is not totally consistent with the timescale of MLD variability (2-5 deepening events per month, pg 4343, line 11). I'm not certain, but I would imagine non-steady state conditions would lead to higher values O2/Ar values being found in shallower mixed layers compared to deeper mixed layers.

The results start with an analysis of the relationship between O2/Ar and MLD. The ranges given (pg 4341, lines 10-11) should use the same percentile so as to be comparable. The inference that deep mixed layers to the south limit primary production is not consistent with the elevated chl-a values found in the far south of the transect in/south of the Polar Front (fig 1, fig 2c) where mixed layers are deeper and incoming irradiance is lower (fig 3). The 'threshold' value of 45m identified is, in my view, used too strongly through the paper and at times morphs into a 'critical value' (pg 4344, line 9) which it isn't (there is definitely no peak at 45m – pg 4342, line 1 should be 'below which' rather than 'where' after the 'threshold'). I don't agree that the relationship is 'strongly non-linear' (pg 4347, line 5)

The increased chl-a near the Polar Front is due to advection of iron downstream from South Georgia (on average there is a monotonic decrease of chl-a along the downstream flow), suggesting that spatial variations in iron availability are more important in setting chl-a values in summer than light, even away from the large blooms. Relevant to this study, it is likely that downstream advection from the South American shelf

area is important. Along with this, lateral exchange between the circumpolar waters (N replete, Fe limited) and South Atlantic waters (N limited, potentially elevated Fe concentrations – this should be stated) may also help sustain production in the SAZ. On pg 4336, line 13 the system is described as at times 'iron replete'. Although iron addition stimulates growth, the relatively low chl-a values (all <1 mg m-3) suggest that there was still significant limitation on phytoplankton growth – iron limitation is a sliding scale.

The estimation of vertical iron supply (equation 3) puzzled me. Firstly I'm not sure what element of the integral is a function of z and I'm not sure why the 45m value seems to be being used to define a ferrocline depth across all latitudes. Surely this should vary depending on the normal range of MLD variation (deeper mixed layers bringing iron up from deeper depths and leading to a deeper ferrocline). Overall it seems to be set up to find high values where the MLD varies each side of 45m and not otherwise. I'm not convinced that all assumptions that go into such a formulation are valid and, as discussed above, there are other plausible reasons for this region of enhanced production that should be considered (eg at pg 4345, line 26-27).

Overall, I don't believe that the data presented supports any general conclusion about light limitation and I'm also uncertain about the specific calculations of iron fluxes and the (limited) range of other values they are compared against.

Technical corrections:

pg 4337, line 25: sub-seasonal; pg 4338, line 5: i.e. -> e.g.; pg 4341, line 14: elevated; pg 4342, line 1: +/- 20.4 is too precise; pg 4346, line 11: results; pg 4347 line 17: add 'of' after persistence; Fig 2c: figure doesn't match caption;

Interactive comment on Biogeosciences Discuss., 11, 4335, 2014.