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Ocean Science Discussions

Interactive comment on "Ekman layers in the Southern Ocean: spectral models and observations, vertical viscosity and boundary layer depth" *by* S. Elipot and S. T. Gille

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The authors would like to thank Referee #1 for the review of this paper. Here we will respond to the 3 minor points raised by this referee:

1) About the bias from the incomplete retrieval of the geostrophic component, at least at the zero frequency:

The incomplete removal of the mean geostrophic component is indeed of importance for the estimation of the transfer function at the zero frequency; we find that it is also an issue in the frequency bands directly adjacent to the zero frequency band because as a surface drifter drifts over 40 days (the length of time series considered in this study) the spatial mean geostrophic velocity field contributes to some variance in these bands. While we do not specify this in the original manuscript, we have tried mean geostrophic fields other than that from GRACE (such as a mean dynamic topography derived from the atlas data of Gouretski and Jancke (1998) or the Rio03 dynamic topography, see Figure 11 in http://earth.esa.int/workshops/venice06/participants/776/paper_776_gille.pdf). We found that these frequency bands had a small impact on the results (within error bars). The main reason for this is that the cost function used in the optimization procedure emphasizes the higher ‘‘sub-inertial” frequencies. We will add a sentence in section 5.4 of the revised manuscript to make this point more clear.

2) About an idea of how much of the variance is explained by the models:

This is already indirectly addressed in the last paragraph of section 5.4 about the cost function. In this paragraph we state that once the best set of parameters for the models have been obtained, we are able to produce the levels of coherence that are typically found between wind and drifter measurements. In other words, if one plugs in the numbers in the best transfer functions (1b or 3b) and computes the right-hand side of equation (29), one obtains something very close to the coherence squared reported in Figure 2.6 of Elipot (2006) (available online at http://repositories.cdlib.org/sio/techreport/49/) or Figures 2 and 3 of Rio and Hernandez (2003) (http://www.agu.org/pubs/crossref/2003/2002JC001655.shtml). Why is this relevant? In fact, the coherence squared between two quantities is the relative amount of variance of one of the two that is ascribable to the other. In particular for our study, the spectrum of drifter velocities multiplied by the coherence squared is the ‘‘coherent spectrum”, or the amount of variance explained as a function of frequency. The coherence squared typically peaks around 0.3 at subinertial anticyclonic frequencies so that we can state that the model explains up to approximately 30\% of the variance at these frequencies. In the revised manuscript we will add a sentence to this paragraph explaining this point.

3) About the captions of the Figures: they were indeed switched, and we will make

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sure that the captions are matched to the correct figures in the revised manuscript.

Interactive comment on Ocean Sci. Discuss., 6, 277, 2009.

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