

## ***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 2 for the review of this paper. We would like to reply to the specific and minor comments raised by this referee:

Reply to the 3 specific points:

(1) Regarding the time lag, the referee is right that we find the time lag disturbing, and we have been frustrated at not being able to explain it completely. We will follow the referee's recommendation for the revised manuscript: we will be clear that we are unable to understand this time lag and also that the Ekman models considered in this study are, according to our analysis, unable to account for a phase lag that is a linear function of frequency and also a function of latitude. In the revised version, we will state

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that we remove the part of the phase of the signal that cannot be accounted for and describe the latitude dependence of this lag as reported in Table 2. In this table, we will change the units of the lag from degrees per cpd to hours as suggested. We will also replace the term “spurious” by the term “unexplained” to characterize this un-explained phase lag.

(2) Could the wind slip explain this latitude-dependent phase lag? This is a possibility but the current understanding of the wind slip is that it is linear with respect to the 10-m wind speed (in the form given by equation (26) of the manuscript). Such a wind slip does not translate into a linear frequency-dependent phase lag at constant latitude. Instead, it would imply an additional positive, constant lag at all frequencies.

(3) Regarding the last sentence of section 7.1, we will remove it altogether in the revised manuscript, as it is not a crucial conclusion of this study.

Reply to minor comments (referee comments are in italic):

(1) *p. 280, line 26: “To skeptics” seems unnecessary.*

This will be removed in the revised manuscript.

(2) *p.281, lines 15-18: In a similar vein, there are efforts to estimate global surface currents from satellite data (e.g., Bonjean and Lagerloef, 2002, JPO), and simple models of the wind-driven flow are employed along with satellite wind observations.*

We would like to thank the referee for this comment. We have thought carefully about this, and we feel that the Bonjean and Lagerloef (2002) paper, while interesting, is not relevant in this particular discussion.

(3) *p. 282, lines 18-21: The wording is a little confusing. The use of “component” is especially confusing here since it was used with a different meaning earlier in the same paragraph.*

The first occurrence of “components” will be replaced by “velocities” to avoid confusion.

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(4) p. 285, line 3: “...where  $U(t,z)$  is the horizontal velocity forced solely by the wind stress  $\tau(t,0)$ .” My admittedly nitpicky comment is that  $U(t,z)$  is forced solely by (parameterized) turbulent stresses, which is more than just wind stress. I would like this sentence better if “solely” were removed, possibly to be replaced by “ultimately”.

“Solely” will be removed.

(5) p.285, lines 7-9: (Sentence beginning “Angular brackets represent...”) This would be a very confusing statement if I did not already know what the authors were trying to say.

“Angular” will be removed just to say “brackets.”

(6) p. 286, line 5: “Then, using the Fourier transformed BCs, a solution for  $U$  is found in the form given by Eq 5.” It would help the reader anticipate the structure of the manuscript if the authors pointed out that this will be shown in section 3.3.

We will add “(see Sect. 3.3)”.

(7) p. 286, lines 21-21: Did Ekman really propose constant viscosity? I did not re-read Ekman (1905), but my recollection is that he suggested that constant viscosity is probably a poor model, but easy to solve, so a good starting point.

Ekman used a constant “coefficient of viscosity”, we presume by analogy to molecular friction, but he pointed out that a “virtual” value should be used rather than the viscosity of water, in order to model the effect of turbulent motions. We will omit the word “propose” in the revised manuscript.

(8) p. 289, lines 3-4: It would be good to specify the equations for  $\delta_1$  and  $\delta_2$  in the text. (They are currently only defined in a table caption.) It might also be good to discuss these modified Ekman depth scales.

$\delta_1$  and  $\delta_2$  are defined and described in the text of the appendix A. In the revised manuscript we will point this out.

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(9) p. 290, lines 1-2: *The authors probably appreciate that gridding the drifter positions in time reddens the velocity spectrum. I wonder what this implies for the cross-spectral calculations.*

It is certainly the case that high-frequency variance is attenuated by the gridding of drifter velocities (see as an example Elipot and Lumpkin (2008)). It is the case also that the variance of ECMWF re-analyses winds at 6-hour intervals is underestimated (See as an example Gille (2006)). Altogether, this certainly has consequences for the cross-spectral calculation and this is discussed in another paper in a different but related context: “Estimates of wind energy input to the Ekman layer from surface drifter data”, currently in press in JGR.

(10) p. 296, lines 1-4: *I am only being provocative here: Does it make more sense to use a velocity that is known to be corrupted? At least the sense of the correction is probably correct.*

We agree with the referee that the sense of the wind slip correction is probably correct but we would like to maintain the opinion stated in the manuscript that it “does not make much sense to first remove a linear fraction of the wind in the form of an unvalidated wind slip correction and then subsequently to conduct a cross-spectral analysis between the “corrected” velocity and the wind stress”.

(11) p. 300, lines 18-19: *When model 3b returns values of  $K1$  that cannot be distinguished from zero, it is a good sign that the values of  $Ko$  and  $h$  are within error bars of the values from 1b. (... since model 3b reduces to 1b when  $K1=0$ )*

We agree with the referee on this point, and this is what we meant by stating that model 3b degenerated to model 1b. We will change the wording of this sentence slightly to make this more explicit.

(12) *In section 7.2 and elsewhere (e.g., final sentence of main text): It sometimes seems like the word “stratification” is used to refer to buoyancy fluxes and changes in*

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*stratification. For example, the last sentence says stratification is omitted in the Ekman-type models considered here. It seems the layer models considered here give a rough approximation to the effect of stratification. However, they are not implemented in a way that allows for changes in h.*

We agree with the referee and will change the word “stratification” to “buoyancy fluxes” at lines 10 of page 304 and at line 21 of page 307 (the last sentence of the manuscript).

*(13) Table 1: It would be good to remind the reader what the numbers mean (as was done for the letters).*

We will do this.

*(14) Table 2: I think the lag is discussed in Section 5, not Section 4 (as stated.)*

Correct, this will be corrected in the revised manuscript.

*(15) Table A1: “transfer functions” is intended to be possessive here. I’d suggest re-wording: “Mathematical expressions for the limiting behavior of the transfer functions”*

We agree, and this will be changed in the revised manuscript.

*(16) Fig. 9: Third panel not described? (17) Figs. A1 and A2: The captions appear to be switched.*

The captions to Figures 7-11 and A1-3 were mixed up and we will make sure that the captions are matched to the correct figures in the revised manuscript.

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

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