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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

Anonymous Referee #2

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Review of "Ekman layers in the Southern Ocean: spectral models and observations, vertical viscosity and boundary layer depth," by Elipot and Gille, a manuscript submitted to Ocean Science

Overall, this is a very good paper. I enjoyed reading it, and it contains some ideas that will have broader impacts, beyond Ekman dynamics and the Southern Ocean. In particular, the analysis in terms of the transfer function between wind and currents is ingenious, and the careful treatment and discussion of various theoretical models and transfer functions is helpful. I recommend publication after minor revisions.

Some people might complain that the paper does not give much insight into the actual

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Interactive Discussion

Discussion Paper

wind-driven flow in the Southern Ocean, because the actual data analysis is not too compelling and the paper contains a total of nine theoretical models that all appear inadequate for giving a good physical description of the observations. While this may be true, the value of this paper is in the discussion of the spectral response of the various models to wind forcing and the overall conclusion that theoretical models of Ekman dynamics will likely need to account for surface buoyancy forcing explicitly. Testing this handful of models was necessary to arrive at this conclusion, and the discussion of the spectral response of these models is general enough and thorough enough that it is likely to be useful in other contexts.

The manuscript, while well written, is hard to follow at times. In general, the authors could do more to clarify the presentation and help the reader anticipate what is coming. Some of my specific comments are intended to help in this regard.

Specific comments:

(1) Section 5.2: I initially understood this to be an argument that there is some timing mismatch between the drifters and the wind products. (I also initially believed it, too.) A linear slope of coherence phase (drifter vs. wind) with frequency would pretty much prove it in my mind, if the phase slope was not a function of position. However, the text does not make it very clear that the estimated lag is a function of latitude. (Table 2 makes it clear, though. The lag is smallest at high latitudes, and it varies by a factor of two from 30S to 60S.) I find this disturbing, and I am sure the authors do, too. I do not know what to make of this, and I would be more upset about it if I saw the primary focus of this paper as an observational one.

The latitudinal variation in the lag makes it seem very unlikely that there is a simple timing offset. However, the discussion in the first two paragraphs makes it seem like this is what the authors think it is. The title of the subsection, "Correcting a spurious constant time lag" gives that impression, too. The time lag is not constant; it is a function of latitude. And, not being a constant, there is nothing in the manuscript to convince me it is spurious.

I would like to make a productive suggestion here, but I do not know what to say. Certainly, the text should be clarified. The main issue is that there appears to be a time lag between ECMWF wind stress and drifter motion that is a function of latitude but not frequency. Discussion of NCEP and timestamps seems like a distraction. Given that Table 2 shows latitudinal variation in the lag, it also seems a little misleading to say that "a data-specific possible explanation for this is that there is a spurious misalignment of the timestamps...". In fact, I believed that there was a simple misalignment of timestamps on the first reading, but I felt misled when I realized that the offset is a function of latitude. I guess my recommendation would be to be very frank and clear about the situation, describe how the lags were calculated, describe them, and move on. It would also be nice to know how the results would differ if these lags were not introduced.

On a related note, the lags given in Table 2 should be stated in hours, not degrees of phase per cpd. (Unless I missed a factor of 2*pi, 1 hour equals 15 degrees of phase per cpd.) If I have interpreted the units correctly, the imposed lags are 1.41-3.2 hours–this should be stated in the text.

(2) p. 295, lines 22-23: I am sure it has already occurred to the authors, but could the wind slip, which increases the "phase between stress and drifter velocity at all frequencies", explain the latitude-dependent phase lag discussed in the previous subsection?

(3) I like the discussion in Section 7.1. However, the last statement of this subsection is too strong, or it is not giving the right impression. It seems more likely to me that the models are inadequate to explain the observations than that the von Karman constant is not close to 0.4. It is reassuring that the results match an expected scaling with a proportionality close to what is expected, but surely the authors do not mean to suggest that people should use an unconventional value for the von Karman constant based on this analysis. (I doubt that was their intent, since the next sentence starts a discussion of what I see as the main shortcoming of the models they considered.)

Minor comments: (1) p. 280, line 26: "To skeptics" seems unnecessary.

(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.

(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.

(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".

(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.

(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.

(7) p. 286, lines 21-21: Did Ekman really propose constant viscosity? I did not reread 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.

(8) p. 289, lines 3-4: It would be good to specify the equations for delta1 and delta2 in the text. (They are currently only defined in a table caption.) It might also be good to discuss these modified Ekman depth scales.

(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.

(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.

(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)

(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 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.

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

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

(15) Table A1: "transfer functions" is intended to be possessive here. I'd suggest rewording: "Mathematical expressions for the limiting behavior of the transfer functions"

(16) Fig. 9: Third panel not described?

(17) Figs. A1 and A2: The captions appear to be switched.

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