We thank the referees for their positive and thoughtful comments.

Our responses (italics) are inline with the referee comments below. A revised manuscript incorporating the described changes has been submitted.

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Reviewer 1.

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1. General Comments

The topic of this paper, correcting eddy covariance estimates of fluxes from spurious ship effects, is highly topical and necessary. Air-sea interaction requires far more study, and direct measurements (such as described) are essential to reduce the community dependence on derived fluxes.

I think the paper suffers from lack of rigour, edging towards the circular, although the detail of the analysis is very good. This may be my relative weakness in understanding of the topic, but I would then argue that if further clarity is needed for myself, then others may feel the same.

We appreciate the reviewer's honesty and hope that the revisions described below have improved the clarity of the paper.We do not believe the arguments presented to be in any way circular nor lacking in rigour.

The issue (I understand) is that an observed peak in the spectral signal of atmospheric turbulence may be spurious due to ship motion, rather than inherent in the flow (e.g. eddies in the air being due to air flowing over waves on the surface). Further, the observed signature is due to the motion of the instrument (bolted to the ship) rather than the moving ship generating additional variable diffluence.

The reviewer is correct in their summation of the problem in the first sentence of this paragraph. The second sentence however suggests the reviewer has misunderstood the problem – the issue is not one of the instrument moving relative to the ship (it is rigidly mounted), nor one of diffluence (divergence of nearby flow from the measured streamline), but of the measured streamline changing orientation over time in response to the changing orientation of the ship. Changes to the flow over the ship can include confluence (convergence of streamlines) as well as diffluence, though neither are necessarily present, and are certainly not necessary as causes of the distortion observed. We provide evidence suggesting that the source of the signature is time-varying flow distortion correlated with the changing ship orientation..

IF this is the overall theme of the argument (and I may be confused) then the argument may be assisted by some re-arrangement of the presentation, some editing, and attention to figures.

Some Instances

1.1 Figure 1. A schematic positioning and flow diffluence field would be most useful here, to introduce the concept (from front, side and above)The photo does not clearly indicate position of sonic back from the bow point.

A new version of Figure 1, incorporating a schematic, has been provided. The location of the sonic anemometer relative to the ship is also given in Appendix B. The problem addressed does not relate to diffluence per se, so illustrating this adds nothing useful to the discussion. Incorporating flow streamline information from the CFD modeling (described in the given reference Yelland et al., 2002) adds little. Note that the CFD model can only be run for a stationary ship, so does not allow the time-varying flow distortion to be assessed. Figure 4 illustrates the time-varying changes in the streamline orientation (as illustrated by the measured tilt) with changing ship attitude.

1.2 Figure 2. Ogive does not offer much relevant information. Far ore useful would be equivalent spectra from the ship accelerometers.

The ogive is widely used within the turbulence community to assess the quality of turbulence measurements and provides essential information – demonstrating the impact of the distortion in the spectra on the final flux estimate. The ogive is particularly useful when assessing sample lengths for stationarity (see comment 2.1 below), since the ogive (being a cumulative representation) is smoother than the cospectrum.

The requested spectra of the ship's vertical velocity (derived from integration of the accelerometers in the motion pack collocated with the sonic anemometer) are already given in Figure 5.b. Figure 5.d gives cospectra of measured <u'w'> which is almost entirely dominated by the ship velocity since no motion corrections have been applied.

In order to present our results more clearly, addressing both this point, point 3.3 below and point 13 from Reviewer 2, we have removed panel 5d, and added a panel to Figure 2 showing the cospectra prior to applying motion correction (following Edson et al., 1998). This is a more appropriate point to demonstrate the large distortions present in 'raw' cospectra.

1.3 p 15549 line 18: "these frequencies are associated with platform motion...". This is the hypothesis? Then should not be stated.

This is not the hypothesis. The hypothesis is that "Signals in cospectra at scales associated with waves and platform motion (the "motion-scale") result from motion error (due to…) rather than being a turbulent signal induced by wind-wave interaction. We agree that this could be expressed more clearly and have rewritten the sentence for clarity.

1.4 p 15550, line 1 "Motion-scale signal can be removed..." (my italic) . Again, perhaps the issue is whether it is due to motion and secondarily whether it should be removed.

We believe motion-scale signal is an appropriate term, as regardless of its source, the signal does occur at the frequency scale of wave-induced platform motion. Whether it should be removed is the subject of the paper. At this point in the paper we merely state that it can be removed in a straightforward manner: discussion of whether it should be removed comes later on.

1.5 p 15552: set of processes
Again, not my expertise, but I would think that a large ship such as JCR rock like a see-saw in moderate waves, with a near sationarity at the centre of buoyancy (where the gyro's used to be kept). How much difference is there between the observed change flow angle and the pitch of the ship? This information may be in Figure 4 but the presentation is unclear. Perhaps presenting the data as correlation with error of the variables against a single parameter (e.g. sensor height, z_platform), or amplitude and phase (again with error). These data would aid the unraveling of the question.

In terms of ship's pitch only, then the see saw analogy is reasonable, as demonstrated in the oscillation of the pitch variable in Figure 4. However, the flow angle depends not just on pitch, but both it and the vertical platform displacement (z) and secondarily on roll and yaw and motion in the associated axes. Unraveling the exact way in which the different ship attitude/motions may impact air-flow at a given point on the platform is challenging, is likely platform dependent, and beyond the scope of this manuscript (which simply seeks to determine whether these motions lead to a measurement error) as we state later on page 15556, lines 7/8.

We should also note that a fit to (correlation with) a single parameter (z platform, pitch, vertical velocity) does not provide a unique solution for the streamline angle – the same pitch or vertical displacement occurs for both positive and negative vertical velocity; the same vertical velocity occurs at both positive and negative displacements from the mean. Further, because acceleration/velocity/displacement are related via integration over time, and pitch similarly related through the rotation about the centre of mass; the choice of independent variables is – to some extent – somewhat arbitrary, as long as the pair chosen provide a unique description of the ship's position/motion over time. Our choice of vertical acceleration and velocity is made based on the best correlations observed, and is theoretically able to account for 'pumping' of the air over the deck (forced changes of local pressure) in a way that say vertical displacement and pitch could not. This point is noted in the text.

2. Specific Comments

2.1 p 15547 "Fluxes were calculated over 30 minute periods. " Was any study of varying the integration time to ensure stationarity attempted, especially under differing stability?

This is an important point for flux measurement, for which there is always a balance between capturing the full range of turbulent contribution, and minimizing the amount of data lost to non-stationarity, To ensure stationarity in *our measurements, quality control criteria as described in the manuscript were applied (principally the ship maneuvering criteria, and procedures detailed by Foken and Wichura, 1996; Vickers and Mahrt, 1997 – as noted in the text) to remove non-stationary periods. The effectiveness of these criteria has been demonstrated in many previous studies, and was checked here through inspection of the low frequency limits of ogives, from which 30-minute periods were deemed a suitable flux length. With regards to stability, this paper is primarily concerned with moderate to high wind speeds (and the wave conditions that result from them). At winds above 6 m/s, all flux measurements were in unstable or near neutral conditions (here defined as 10/L < 0.2), the norm for the open ocean. At wind below 6 m/s, less than 20% of the measurements were in stable conditions.*

2.2 p 15548 "aligned with the mean stream line". Confirm that this sensible even for mean w rotation: for instance, if flow is diffluent (with a mean updraft) do the eddies also align instantly with the new wind vector?

Yes, this is sensible - this rotation into the mean streamline is standard practice in studies of air-sea turbulent exchange.

2.3 p 15548 Diffluence was estimated to increase wind flow altitude by "1.3 and 3.2 m". Comment on effect on stability (perhaps with ref to Froude number)

We cannot say anything about the effect of stability here – the lifting of the streamline over the ship is determined via a CFD modeling study for neutral stratification. See our response to point 2.1 above for general comments on stability. The primary impact of changes in stability would be to modify the vertical wind speed profile. Some tests have been conducted in the past for CFD modeled flow over commercial ships; even with an extreme change of profile from logarithmic to constant with altitude, the impact on flow distortion was small compared with the absolute distortion.

2.4 p 15549 Comment on justification for elimination of "outliers".

*Drag coefficients above 5*10-3 are, particularly for moderate to high wind speeds, well outside the accepted range of physically plausible values. For the data here, limiting the measurements to our acceptable range of relative wind directions, there were 38 measurements deemed outliers, of which just 6 were for wind speeds of 6 m/s and greater. A note on this has been added to the text.*

2.4 p 15550 Eq 2 "MSC" should be defined early on, e.g. prior to reference to Fig 2.

The motion-scale signal is first shown in Figure 2, and the description and correction of it both follow from this. To improve clarity, we have added a definition of MSC to the caption for Figure 2.

3. Technical Corrections

3.1 p15550 Eq 2 In general, should a wind velocity (m/s) be corrected with a mix of ship velocity (m/s : OK) and acceleration (not so good)? For example, dividing by w'_true gives alpha_2 dimensionless, but alpha_1 still has units.

The velocity is _not_ *corrected by terms with other units. The coefficients are determined by regression, and thus alpha1 has units of s, and alpha2 is dimensionless. A more physical reasoning for these units is provided by the (equivalent) MSCf correction, where the coefficients are "*defined as the ratio of covariances of vertical wind and platform motion to variances of platform motion"*, ie. alpha1 = <w'accz'>/(std accz)^2 (units of s) and alpha2 = <w'velz'>/(std velz)^2 (dimensionless), thus the correction terms both have units of velocity.*

3.2 Figure 3. Offset each error bar group slightly in the horizontal so that over-plotting does not mask data.

We agree this figure could be clearer and have modified it accordingly.

3.3 Figure 5. Unclear why panel 4 has -ve flux. Clarify caption

The negative flux is only at the motion scale, and results from (uncorrected) platform motion. The y-axis labels on Figure 5c,d were erroneously shown as f.C, when they should have been –f.C. We have corrected the labels and altered the figure caption to clarify this (see also the response to point 13 from Reviewer 2 below). Note that the axis limits in figure 5 have be set close around the distortion in the spectra to allow the details to be seen clearly – they are greatly 'zoomed in' compared to those in figure 2. Please also see response to point 1.2.

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Reviewer 2.

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Reviewers: Sebastian Landwehr and Brian Ward

General comments: This paper addresses the motion-correlated signal which has been observed in momentum flux spectra measured on ships, even after standard motion-correction procedures have been applied to the measured wind speeds. The authors present a dataset, where the motion-correlated signal is relatively large, and accounts on average for 20% – 30% of the measured momentum flux signal. The authors provide evidence that the peak in the motion-corrected momentum flux spectrum is not caused by wind-wave interaction, but by recirculation of the air flow at the anemometer location which is caused by the push and pull of the bulky structures nearby. Further they present a simple and efficient way of removing the bias.

We do not agree with the authors suggestion that the overestimation of momentum flux measurements from ships that was reported by (*Edson et al.*, 1998) and *Pedreros et al.* (2003) should be caused by the here addressed motion-correlated flow distortion. This is more likely due to the inaccurate mean wind vector tilt estimation. We see the here presented decorrelation method, however, as a practical approach to reduce bias in direct air-sea flux measurements. We recommend to publish this results with minor revisions.

We discuss the points here regarding the source of errors in previous publications in the specific comments below.

We also note that "recirculation" of the flow is only one of the ways in which flow distortion can be manifested and is not singled out or discussed in our manuscript. On page 15552, line 25 and page 15553 line 1 we briefly mention the possibility of vertical motion of the airflow caused by the vertical motion of the platform, but recirculation of the airflow is unlikely to play a role at all but the lowest wind speeds.

Specific comments:

We provide specific comments below, but there are also several comments embedded in the article file, which we also provide.

We have corresponded with the reviewers and confirmed that all comments are listed below, and that there is no additional article file.

1. (Title) This paper deals with ship motion-induced flow distortion effects in the momentum flux spectra, however, what is the motivation for "wave-induced" in the title?

The ship motion and resulting biases we are concerned with is ultimately induced by wave motion. We also want to highlight the fact that there are two possible causes for the motion scale signal and we are trying to determine which is the case here - a real wave-induced flux component or experimental error. Hence, we feel that this is an appropriate title.

2. (Page 15545, line 1): Add the following references: *O'Sullivan et al.* (2013) and *O'Sullivan et al.* (2015)

We thank the reviewers for highlighting these relevant references, they have been added as suggested.

3. (Page 15545, line 14-16): This is really not surprising considering the location of your mast shown in figure 1. It would appear that the flux instruments are several metres back from the bow. A considerable reduction in flow distortion could be achieved by placing the sensors as far forward as possible. Suggest you include a comment to this effect in the conclusions.

We would first note that the reviewers relate the comment cited to our installation, but it actually relates to the findings of Weill et al. (2003) and Brut et al. (2005) for the R V Thalassa. Nevertheless, we broadly agree with this comment and we have made some minor changes to the conclusions section to better clarify our position on flow distortion as the potential source of the observed bias. Locations of instrument installation are necessarily limited by the design of the ship; while moving the instruments up and forward would minimize flow distortion, it is not technically feasible since there is nothing to mount them on.

The ideal flux sensor location is a complicated issue, discussed fully in Yelland et al., 2002 (Y2002). The location of the mast on JCR is quite typical (see figs 16, 17 Y2002) and CFD modelling gave typical biases compared to other ships.

However, the mast itself on JCR is quite a large, permanent structure whereas other ships often carry a temporary lattice mast. Similarly, the foremast platform on JCR carries quite a few small-scale objects which are not included in the CFD model geometry. These points are included towards the end of Section 2, and are included in the Discussion section (page 15551, lines 17-22).

4. On (Page 15545, lines 21–24): Both *Edson et al.* (1998) and *Pedreros et al.* (2003) show a complete removal of the motion-correlated peak in the momentum flux spectra. It appears therefore more likely to us, that the overestimation of the shipborne fluxes in (*Edson et al.*, 1998; *Pedreros et al.*, 2003) is due to the inaccurate tilt correction, as described in (*Landwehr et al.*, 2015). It is however possible that for the here presented measurements the "time-varying flow distortion" is of greater importance, due to the less favourable anemometer position, i.e., surrounded by bulky structures, while *Edson et al.* (1998) and *Pedreros et al.* (2003) mounted their instrumentation in more pristine locations on slim masts and close to the bow. We had originally applied a similar technique in (*Landwehr et al.*, 2015), but abandoned it for the final version, because one of the reviewers was not willing to discuss this. For this study the reduction in the momentum flux was ≈ 6%. (We did not publish this result in the final version)

We have added a comment to the third para of section 4 (discussion) mentioning the possible contribution of inaccurate tilt correction to the observed motion-scale signal. However, we don't fully agree with the reviewer's assessment of the results in Edson et al (1998) and Pedreros et al. (2003) – it is not possible from the information in those papers to unambiguously assign remaining bias in the flux to a particular source. Research vessels on dedicated flux experiments are often either on-station or steaming slowly. Edson et al., (1998) restricted their flux measurements to ship speed < 2 knots, i.e. errors due to inaccurate tilt correction would be very small. In addition Edson et al., (1998) only show a single, noisy cospectrum, presumably not their worst. Pedreros et al. (2003) QCd their measurements using a ratio of Urel / ship speed > 2 and wind direction bow-on. This does allow inclusion of high ship speeds when the wind speed is high, but much of their data was obtained in the vicinity of the ASIS buoy for their intercomparison. Also, at higher wind speeds ships tend to reduce speed or go hove-to, as shown in our figure A2. The cospectra shown by Pedreros has a log y-axis and broad frequency bins, but even so the measured spectrum is elevated in comparison to the ideal one for fz/U > 0.1, suggesting some uncorrected contamination. Finally, the FETCH experiment took place in the Gulf of Lion, in short fetches where sea state and ship motion would be low in the first place.

We don't claim that motion scale bias is the only issue, just a potentially significant one in some data sets.

5. (Page 15546, lines 8–10): The variation of the residual motion peaks in (*Miller et al.*, 2008) might have another cause: *Miller et al.* (2008) estimated the relative orientation of their anemometer and the inertial motion unit with the planar fit approach from (*Wilczak et al.*, 2001). Small errors in this tilt estimation can lead to a less efficient removal of the ship motion signal. Note that the

magnitude of the tilt correction applied in (*Miller et al.*, 2008) was higher for the low level anemometers. We had observed this effect during the preparation of (*Landwehr et al.*, 2015) when we applied the tilt corrections to the wind vector prior to the motion-correction.

This is a good point and we have added a comment to this effect to the introduction. We have also noted that uncertainty in the alignment of sonic and motion unit (e.g. Brooks 2008) could also contribute.

6. (Page 15548, equation 1): Note that the identification of the natural coordinate system based on a single 30 minute averaging interval can be biased by possible offsets in the vertical wind speed measurement, as elaborated in (*Wilczak et al.*, 2001; *Landwehr et al.*, 2015) this can lead to significant errors in the tilt estimation at low wind speeds.

We have added a comment to this effect to Appendix A, and have added the relevant reference Wilczak et al., 2001.

7. (Page 15550, equation 2): Did the coefficients α_1 and α_2 show any correlation with relative wind direction or the ship speed? If such a correlation exists it could be used as an argument for your hypothesis.

This is a reasonable suggestion. However, the coefficients are dependent on vertical ship motion, which will be correlated with ship speed, and also with relative wind direction (both ship operations, and platform motion are relative wind direction dependent). Hence use of the coefficient correlations to show the source of the signal is problematic. As we state in the manuscript, we do not attempt to provide a comprehensive correction, just an illustration of the problem, its potential size and likely cause.

8. (Page 15550, lines 14–18): The observation of *Edson et al.* (1998) and *Dupuis et al.* (2003) might be more related to the wind vector tilt-estimation, see comment to (Page 15545, lines 21–24).

See our response to comment 4.

9. (Page 15551, lines 9–11): The agreement with the COARE 3.5 parametrisation is no argument for the in-significance of the surface currents. Do you have measurements or estimations of the magnitude and direction of the surface currents?

Current measurements were not available for this experiment, though we anticipate that with the large, varied dataset we have compiled, most of the effect will average out. We have removed the suggestion that agreement with COARE 3.5 implies any effect is small.

10. (Page 15551, lines 12–14): You could mention (*Landwehr et al.*, 2015) in this context.

We have added the reference to this section (see our response to point 4) as suggested.

11. (Page 15553, lines 19-21): Sharp thought!

12. (Page 15554, lines 23-26): This is a very strong argument.

13. (Page 15567, Figure 5): This is a nice illustration. You might zoom in further on the frequency range of interest. It might be worthwhile to increase the frequency resolution of the spectra, as it appears to be very close to the frequency shift that you want to show. I assume (c) and (d) show $f \cdot |C_{uw}|/u^2_*$?

We thank the reviewers for these good presentation suggestions. We have zoomed in further on the motion scale, and the frequency resolution has been increased. Note that this resolution change slightly alters some of the values given on page 15554, lines 12-19, but does not affect the conclusions drawn from them. We have also clarified the label on 5c and 5d and altered the figure caption to further clarify. Panel 5d has been removed and a similar panel added to Figure 2. More details on this change are provided in the response to Reviewer 1 point 1.2.

14. (Page 15568, Figure 6): Figure 6a shows that the average effect of the decorrelation is a reduction in CD, however in Fig. 6b it the effect is the increase the relative CD for abs(ship – relative wind direction) > 20° in comparison to the mea- surements where the wind was blowing bow on. What I want to say is: maybe the label in Fig. 6b should be “linear fit - CD”.

The label is correct – the fit in 6a is calculated from bow on measurements (-20 to +50 degrees) and then the perturbation from that fit calculated as [100(drag-fit)/fit] for drags at all wind directions.*

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