30/07/10

Response to Referee Comments

On the variability of Florida Straits and wind driven transports at 26N in the Atlantic Ocean.

Firstly, the authors thank both reviewers for their supportive and helpful comments. We are pleased that both referees acknowledge the comprehensive and thorough approach used, consider the results an interesting and worthwhile read and support the publication of the work. The comments of both reviewers are addressed below and have been incorporated into a revised version of the original manuscript as appropriate.

Referee 1 (referee comments in italics)

This is a remarkably comprehensive paper; thorough, interesting, and well written. It struggles of course with the usual compromises: how to present a great deal of data in a limited space. Many of the figures are too small to be really useful in any quantitative sense but it is hard to know how to avoid that, except perhaps for making larger versions available for downloading. Figure 2, for example, is so clear and meaningful that one wonders if it were done by someone else than the person(s) who did Figures 1 or 10.

We acknowledge that several of the figures are quite small in the discussions paper. It is hoped that this will not be a problem in the final typeset manuscript and all figures will be checked for clarity at A4 page sizing before approval. It is noted that Ocean Science uses a portrait page orientation. This should lead to an enlargement of figures such as 6 and 10, which are constricted by page length in Ocean Science Discussions. In particular it is hoped that Figure 10 will be reproduced over a full page however if this is not possible it will be split into two halves (January-June and July-December) and referred to as 10a and 10b. Finally it is noted that all the figures supplied are vector graphics and therefore in the electronic version of the manuscript a zoom tool can be used to magnify the figures without any loss of clarity.

We have known for a long time that the "mean annual cycle" emerges only from a record of many years duration. Their exposition of this is nice. However, with the results from Figure 2, the reader will ask, Do these add up? to what extent are these three results in agreement? We assume that the purpose is to understand what is the forcing of the variability, but one cannot do the numerical additions " by eye." I see no discussion of this.

Figure 2 shows the long-term mean seasonal transport anomalies of the Florida Straits, Ekman and Sverdrup transports at 26N. The reviewer asks to what extent these results agree and what is the forcing of the variability. This is an interesting question and somewhat addressed throughout the paper, e.g.:

‘As expected, the large semi-annual amplitude of Sverdrup transports east of the Florida Straits is not matched by the small amplitude of the Florida Straits semi-annual component due to the blocking effect of the continental shelf and Bahamian islands (Anderson and Corry, 1985a).’

However, this point is not fully discussed in any one part of the manuscript. Three paragraphs addressing this point have now been added to the beginning of the discussion, collating and adding to the points made in the original document. They read:

‘The long-term mean seasonal transport anomalies of the Florida Straits, Ekman and Sverdrup transports at 26°N are presented in Fig. 2. All three seasonal cycles are both well defined and distinct from one another. The Florida Straits seasonal cycle has a dominantly annual periodicity whilst the seasonal cycle of Sverdrup transport has a clear semi-annual component (Table 1). Seasonal Ekman transport integrated across 26°N has no dominant annual nor semi-annual component. No obvious relationship exists between any of these timeseries.

The large semi-annual amplitude of Sverdrup transports east of the Florida Straits is not matched by an equivalent anti-phased response in the Florida Straits, as suggested by simple Sverdrup dynamics. Instead, the Florida Straits has a semi-annual component of much smaller amplitude (though somewhat in anti-phase, Table 1) due to the blocking effect of the continental shelf and Bahamian islands (Anderson and Corry, 1985a). The same is true of the annual components of the Florida Straits and Sverdrup transports, which show no anti-phased relationship and a large difference in amplitude. The disagreement between the observed Florida Straits seasonal cycle and that predicted by Sverdrup dynamics has long been known (e.g. Schott and Zantopp, 1985). The seasonal cycle in Florida Straits transport is instead thought to be set chiefly by meridional wind stress local and downstream to the channel, a view supported by this study.

The general difference in appearance of Florida Straits, Ekman and Sverdrup transport seasonal cycles is a result of the contrasting structure of the meridional, zonal and curl wind stress fields near 26°N. In the western sub-tropical Atlantic, northward Summer wind stress reverses to southwestward wind stress in late Autumn (Fig. 10), driving both the Florida Straits annual cycle and strong northward regional Ekman transport in Autumn. In the central and eastern Atlantic, Ekman transport is semi-annual in character, leading to integrated basinwide seasonal transports of quite irregular character. Whilst Ekman and Sverdrup semi-annual components are nearly in anti-phase (i.e. westward wind stress appears associated with negative wind stress curl) the semi-annual component of wind stress curl is more in phase across the basin (except approaching the eastern basin boundary, Figs. 9e and 10) leading to a Sverdrup transport seasonal cycle with clear semi-annual periodicity.’

One can always find a few quibbles. The authors write, for example:

"Treating each harmonic as an independent estimate of the seasonal cycle, the harmonic coefficients representing the 1980–2007 Florida Straits mean seasonal cycle can therefore be quantified including confidence intervals. These are presented in Table 1." Confidence limits are usually computed when we believe we have multiple measurements of "the same thing." In this case, the variability from year to year is real; it would be nice if the authors made a somewhat clearer distinction between real physical variability as opposed to statistical reliability.

This sentence caused some confusion for both reviewers (see also response to referee 2 below) and has been changed to read simply:

'Harmonic coefficients (including confidence intervals) for the 1980-2007 mean seasonal cycle are given in Table 1.'

Details of the calculation are given in the Table 1 caption. One of the main results of this paper is that decade long timeseries are necessary to calculate the mean seasonal cycle. For the Florida Straits it was demonstrated that for 8-year periods, apparent changes in the mean seasonal cycle are not significant. The purpose of fitting harmonics to each individual year of data was to demonstrate the large interannual variability of the annual cycle and highlight the risks inherent in estimating it from relatively few years of data. Calculations of the seasonal cycle are contaminated by stochastic variability at other frequencies and harmonic coefficients for the long-term mean seasonal cycles are therefore given with confidence limits.

There is almost no mention of how the calculations of actual ocean transport are done from RAPID. It would be particularly helpful to see some discussion of the possible errors inherent in the measurements, rather than such an essentially statistical approach w/o regard to errors in the data. The authors seem to assume that the reader will know all the details of the RAPID measurements, altho' many will not. The issue of genuine error in oceanographic data is a thorny problem, to be sure, but it should not be ignored. Computing transport from measurements at the edges is a clever way to avoid the internal variability, but it is non-trivial, as a great deal of transport can be found along the sloping boundaries.

This paper described properties of Florida Straits, Ekman and Sverdrup transport variability at 26N based on the decadal timeseries available. Whilst Florida Straits and Ekman transports are components of the RAPID observations, a complete discussion of how RAPID calculates ocean transports and estimates error is beyond the scope of this work. The interested reader is referred to Cunningham et al., 2007, Kanzow et al., 2007, Kanzow et al. 2009 and particularly Kanzow et al., 2010, which are referenced throughout the paper and where many of these issues are addressed. Analysis of the RAPID observations and the design of the array are ongoing subjects of research.

Whilst this work is mostly statistical in its approach, we thank the referee for highlighting the need for some further discussion of error. A section addressing this entitled 'Error in the Florida Straits, Ekman and Sverdrup transport timeseries' has now been added to the end of the results section. It reads:

'Whilst this work has mostly followed a statistical approach, measurement errors are now briefly discussed. For the Florida Straits transport, errors in daily mean instantaneous (3-day low-pass filtered) measurements amount to 1.7 Sv rms with the errors predominantly random (Larsen, 1992; Meinen, 2010). The observed variability of daily Florida Straits transports is 3.3 Sv rms, thus random errors account for ≈ 25% of variance and the signal to noise ratio is substantial. Much of this study uses monthly mean Florida Straits transports, constructed from 15 or more daily values (following DiNezio et al., 2009) so random measurement error associated with monthly means is a maximum of ≈ 0.4 Sv rms (1.7/√15). The observed variability of monthly Florida Straits transport is 2.4 Sv rms, thus measurement errors account for ≈ 3% of variance and the signal to noise ratio is large.

For monthly Ekman and Sverdrup transports across 26N, differences in mean transport between climatologies provide an estimate of bias whilst r^2 values and rms differences (between demeaned and detrended timeseries) provide an estimate of random error (because NCEP-NCAR assimilates QuikSCAT, the two cannot be considered strictly independent). For Ekman transports, mean transports for 2000-2005 (where the NCEP-NCAR, NOC v1.1 and IFREMER MWF QuikSCAT climatologies overlap) have a range of 0.2 Sv, whilst r^2 values are > 0.9 (indicating over 90% of the timeseries variance is accounted for between climatologies) and rms differences range from 0.5-0.8 Sv (in comparison with NCEP Ekman transport variability of 1.9 Sv rms). Ekman transport variability across 26N appears well defined relative to the differences (random errors) between climatologies. The mean bias estimated here is consistent with the more conservative RAPID estimate of ± 0.5 Sv (Kanzow et al., 2010) accounting for uncertainties in wind measurements and drag coefficient. For Sverdrup transports, mean transports for 2000-2005 have a range of ≈ 3 Sv (though this is more a result of differing climatology trends), r^2 values range from ≈ 0.6-0.8 and rms differences range from 6-8.5 Sv (in comparison with NCEP Sverdrup transport variability of 8.0 Sv rms). Sverdrup transport variability across 26N appears reasonably coherent between climatologies but care should be taken interpreting short sections of the record where differences (random errors) between climatologies may be comparable in magnitude to the signal of interest. This study only addresses Sverdrup transport properties derived from the full record.'

The plots are clear, but the presentations of power spectra are separated from the coherence; if power is unusually low in either of the input variables, the coherence can be artificially high. Perhaps the authors have allowed for this but it is not clear to the reader.

We are aware that artificially high coherences can occur in the case of unusually low power in either of the input variables and have not attached any significance to any peaks where this is the case. The study itself is mostly concerned with general coherence over broad period bands (e.g. 10-70 days, 60-720 days) whilst narrower bands or individual peaks, which are more susceptible to such problems, are rarely discussed. The main exception is in the case of annual (360 day) periods (e.g. the coherence of the two model passes in Fig. 8) however, no peaks in seasonal coherence are coincident with low power in either input variable.

It is surprising that the authors do not refer to the earlier work of Mayer and Weisberg, who made a more limited study of many of these issues.

The authors thank the reviewer for drawing their attention to this earlier work. This paper has now been referenced in both the introduction and discussion sections in the following contexts:

Introduction: - 'On annual and sub-annual timescales, several links have been made between the Florida Current and both local and remote wind forcing (e.g. Schott and

Zantopp, 1985; Leaman et al., 1987; Schott et al., 1988; Lee and Williams, 1988; Mayer and Weisberg, 1993)…….'.

Results: - 'The range and structure of the seasonal cycle is similar to that previously described for other wind stress data (e.g. Mayer and Weisberg, 1993).'

Discussion: - 'Mayer and Weisberg (1993) found significant coherence in the 182-393 day period band between sea level difference across the Florida Straits near 26.5N (a proxy for Florida Current volume transport) and the shore-parallel wind stress component north of the straits (from the COADS dataset) for the period 1982-1988 and concluded that coastally trapped waves are important for the annual cycle of transport within the straits.

The paper is focused strongly on the annual cycle of variability, rather than variability of transport in the broader sense; I believe it would be helpful if the title were clear on this point. I enjoyed reading the paper.

Changed to:

'On the Seasonal Cycles and Variability of Florida Straits and Wind Driven Transports at 26ºN in the Atlantic Ocean'

Referee 2 (referee comments in italics)

General Comments:

The paper is a thorough statistical analysis to the directly measured Florida Straits transport (FST) and the wind derived Ekman (ET) and Sverdrup transport (ST) at 26N. Whilst it has many interesting features I found it a very difficult read. The language use is good, but the publication reads a bit like a thesis with many unnecessary details and a lot of repetition. The results in the summary are interesting though and I recommend publication of the paper but only after some rewriting to make it more digestible.

Whilst both referees compliment the style of writing, the authors accept there is a degree of repetition in the paper and the section headings could be chosen more carefully. The results and discussion have now been edited to eliminate repetition and clarify section headings wherever possible. See specific comments below.

Specific comments:

- To make the paper easier to read the discussion parts in the results sections 3 can be shortened as it is reproduced for the most part in the discussion section 4 and again summarised in the summary. Of it should go out of 4 and be in 3 only. A little overlap is acceptable but the discussion part in this paper is more of a summary of the results section and the interpretation that is usually in a discussion section forms only a small part and is again often covered in section 3.

Repetition of results has been removed as much as possible from the discussion part of the paper though some elements of discussion have been retained in the result section to help guide the reader from one result to the next. The summary now comprises mostly results and discussion from the results section, which forms the main body of the paper. The discussion section itself now addresses outstanding points from the results and places the results in the context of the MOC.

- Section headings from section 3 onwards are not very clear and with a paper like this it is important to know immediately what the wording in the section refers to. If you took the section headings of this paper and made a table of contents with it, it will become clear how little they help to explain the logical structure of the paper. Here are examples. If section 3.1.1 is "seasonal cycle", then 3.1.3 should be something like "transport at non-seasonal timescales". Section 3.1.2 is a dissection of 3.1.1. If you want to keep it like that, then 3.1.1 should be "observations of the seasonal cycle" and 3.1.2 "relation of the seasonal cycle to ..." Section heading 3.1.3 is equally confusing. You describe variability of FST at other than seasonal timescales but the heading does not reflect that. If is misleading because it says "wind driven variability" and you just convinced the reader that the seasonal cycle of section 3.1.2 is wind driven. Section 3.1.4 should rather say "Influence of internal ocean transport variability". In general, just write the headings as observations at timescales or forcings or make the difference clear and also if the forcing is just related to a certain timescale (i.e., 3.1.2) then say so.

Section headings have now been clarified.

In section 3.4 you have wind-driven again. The word "wind-driven" transports should not refer to Ekman and Sverdrup because you show the FST is likewise driven by winds. Wind-driven is used in this paper to descibe just about everything and is therefore not desciptive anymore. Rather say Ekman and Sverdrup transport from NOC and Quickscat.

References to 'wind-driven' transports have been changed to Ekman and Sverdrup transport for clarity.

Now for section 4 headings. You cannot have 4.1 be the same as 3.1.1 and 4.2.1. I think you should delete the whole of section 4.1 as it seems to be mostly repetition (compare for instance page 940 line 23-24 to just two pages later page 942 line 20-21. Section headings 4.2, 4.3 and 4.4 I find again not very descriptive and I have the same problems with calling Ekman and Sverdrup transports wind-driven transports. Yes, it is wind driven, but that does not distinguish it clearly from the FST. You just called that wind-driven in heading of 3.1.3 as well. Just separate 4.3.1 and 4.3.2 out as 4.3 and 4.4. Section 4.3.1 has quite a bit of overlap with 3.3 again. Discussion should not be so much summaries of result. Overall results are already repeated in the summary. Heading 4.4.1 is again not descriptive. Which transport?

Section 4.1 has been removed and replaced by a comparison of Florida Straits, Ekman and Sverdrup transport seasonal cycles as part of the response to comments from referee 1. Discussion of Florida Straits transport variability has been compressed after removing repetition of results. Section headings have been clarified. Ekman and Sverdrup transport discussion has been removed as this was almost entirely a repeat of the results section (the main results are still summarised in the conclusions).

Technical comments:

Page 921, line5: One numerical model run does not convincingly "show" profound implications of an MOC shutdown on northern hemisphere climate. Either add more references or change wording to "suggest" .

Changed to read:

'A substantial weakening of the AMOC is predicted in response to anthropogenic related changes in high latitude buoyancy flux (IPCC, 2007) and it is suggested that a total shutdown of the AMOC would have profound implications for the climate of northwest Europe (Vellinga and Wood, 2002).'

Page 922, line 26. Wording is a bit weird.

Changed from:

Observations of the Florida Current reveal the seasonal cycle possesses both barotropic and baroclinic vertical structure (Leaman et al., 1987; Schott et al., 1988).

To:

'Observations of the vertical structure of the Florida Current have shown that the seasonal cycle possesses both barotropic and baroclinic components (Leaman et al., 1987; Schott et al., 1988).'

Page 923. Abbreviation of NAO is after its use the first time.

Changed.

Page 924. Lines 13, 22. Could you supply web addresses for the cable transport data and the NCEP page please

Done (note that all web addresses for data sources are also already available in the acknowledgments).

Page 925, line18. The disclaimer is not needed here, because the definition of Sverdrup balance remains the same whether it holds or not.

Removed.

Page 925, line 10. Website reference again please.

Done.

Page 927, line 17. Black should read blue.

Black should in fact read red. This has now been changed.

Page 928, lines 8-11. Strange sentence. Can you clarify please?

The original text read:

'Treating each harmonic as an independent estimate of the seasonal cycle, the harmonic coefficients representing the 1980-2007 Florida Straits mean seasonal cycle can therefore be quantified including confidence intervals. These are presented in Table 1.'

This wording is perhaps unnecessarily confusing. The aim of this sentence was to merely to point out that, having calculated harmonic coefficients for each year of the 28 years of data, mean coefficient values ± 1 standard error (confidence intervals) can now be calculated for the full period. This is what is given in Table 1 (and already explained in the Table 1 caption). To avoid confusion, the text now simply reads:

'Harmonic coefficients (including confidence intervals) for the 1980-2007 mean seasonal cycle are given in Table 1.'

Page 929, line 3. It would read better is you said: "... suggesting that it is possible that some change in seasonal cycle over time occurred."

Done

Page 929. Line 11. You write (particularly 1982-1989). What is mean here? Sure not the difference between these two years. The sentence also needs a 'the' before Florida Straits.

This follows directly on from the previous subsection where the use of the notation '1982-1989' referred to the mean seasonal cycle for the 8-year period 1982 to 1989. However, because this is a new subsection the text has been changed to read:

'Whilst changes in the Florida Straits seasonal cycle remain plausible (particularly when comparing the mean seasonal cycle for 1982-1989 to more recent 8-year periods…..'

Page 930, line 10: Sentence needs refining. "Florida Straits" is a geographical area. Perhaps "to that over the Florida Straits"?

This sentence clearly created some confusion as it was designed to highlight a possible link between wind stress curl over the Caribbean Sea and volume transport in the Florida Straits. Not to a relationship between wind stress curl over the Caribbean and over the Florida Straits. The sentence has been refined to read:

'…..wind stress curl over the Caribbean Sea possesses similar seasonal phase to that of the Florida Straits transport'

Page 931, line 21. EOF needs to be written out the first time.

Done

Page 932, line 8-10. I don't understand this. How can it only project in the western tropical Atlantic if the peak regression values are in the central Atlantic?

Wording was confusing, now corrected to read:

'This is consistent with regression maps of wind stress on the NAO index which indicate that in the western sub-tropical Atlantic the NAO only projects significantly on zonal wind stress (though peak zonal regression values are observed in the central Atlantic).'

Page 932, line15. Please indicate both variables that are being correlated.

Done.

Page 932, line30. Please indicate what remote means.

Originally:

'the difference in local SLP and SLP remote from the Florida Straits'

Now reads:

'the difference between local SLP and SLP at various locations several hundred kilometers outside the Florida Straits'

Page 933, lines 2-3. This needs rewording. As written is sounds as if a very small amount of atmospheric pressure forced the FST. Did you mean "which showed negligible contribution of atmospheric forcing to the FST …"?

Changed to:

'This is in agreement with the barotropic model study of Greatbatch et al. (1995) which showed negligible contribution of atmospheric pressure forcing to Florida Straits transport variability at periods > 2 days due to an approximately isostatic ocean response.'

Page 933. Please indicate which part of the model is spinup.

The text describing the use of the NEMO model run has now been clarified as the use of the terms 'spinup' and 'run' was confusing and somewhat incorrect. The text now refers to a single NEMO model run, comprising two passes, the first of which is initialised from a climatological ocean state, whilst the second is initialised with the final ocean state from pass 1. Each pass covers the period 1958-2001 and is forced using identical external ocean fluxes. A Florida Straits transport timeseries is extracted for each pass. For pass 1, the part of the timeseries where there is strong initial adjustment is discarded (1958-1975), and the same is done for the second pass purely to get two sections of the run with identical forcing. These are then compared.

The text explaining this now reads:

‘To understand the importance of internal ocean variability in the Florida Straits transport measurements, a single NEMO run comprising of two passes of the DFS 3 forcing dataset (ORCA025-N102 and ORCA025-N112), undertaken at the National Oceanography Centre, Southampton as part of the DRAKKAR suite of eddy-permitting 1/4° NEMO model runs (Barnier et al., 2006; Madec et al., 2008; Grist et al., 2010), was used opportunistically to estimate transport variance associated with this process. Each pass covers the period 1958-2001 and is forced using identical external ocean fluxes (momentum and buoyancy) with output available at 5-day resolution. The model starts from rest, with a climatological state. The final ocean and ice state from pass 1 (N102) is used to initialise the start of pass 2 (N112), facilitating studies of climatic drift in the model. For the purposes of this study, these two passes also allow the behaviour of two model oceans with differing initial states but identical surface fluxes to be compared.

The 1/4° NEMO model possesses realistic bathymetry which includes a Florida Straits channel separated from the ocean interior by Bahamian islands. Time series of Florida Straits transport were extracted from each model pass and compared over the period 1975-2001 (where drift effects in pass 1 were no longer evident). The simulated Florida Straits transports are shown in Fig. 8a. Mean transport for the two passes is 29.3 Sv and 27.9 Sv which compares favorably with the observed Florida Straits mean transport of 32.1 Sv. The phase and amplitude of model and observed seasonal cycles are also very similar though model variability is somewhat damped at most frequencies (the std. devs. of pass 1, pass 2 and observations are 1.7 Sv, 1.8 Sv and 3.2 Sv respectively).’

Page 935, line 2 and 6. The double referral to the secondary max sounds a bit strange.

The double referral has been removed.

Page 935, line14. I disagree with the interpretation of figure 9c east of 60w. There is only one minimum in March. There is a positive swing after that and not much of a dip.

We presume the reviewer is referring to figure 9b which is the result being discussed. The authors disagree with the reviewer, the text states:

‘East of 50-60W, Ekman transport shows a pronounced semi-annual cycle with maximum northward transport in January and July and minimum northward transport in March-June and October.’

East of 50W, figure 9b clearly shows a minimum in March-June and October. It is however acknowledged that over the longitudinal range 50-60W, a transition occurs from a somewhat semi annual cycle to a more annual cycle and thus over this range the October minimum is less pronounced. However, the text already states that:

‘The Ekman transport seasonal cycle shown in Fig. 2b can mostly be understood as the sum of two somewhat distinct seasonal transport regimes east and west of ≈ 50-60W (Fig. 9b).’

We feel therefore that the text already makes clear the interpretation that different seasonal characteristics are observed east and west of ≈ 50-60W and that the transition between the two regimes occurs over this longitudinal range.

Page 948. This section is more about transports of the terms of the wind-driven gyres than that of the MOC I'd say. But I can see it is presented as the MOC components because this work is motivated by the MOC observations and that is not technically incorrect.

The authors thank the referee for this thought provoking comment. The interpretation of these terms is often one of personal opinion though some thoughts are included below.

This section addresses Ekman and Florida Straits transport variability at 26N. Although these two transports are fundamental components of the RAPID system of MOC observations, Kanzow et al. (2010) also demonstrate that each term contributes independently to variability of the MOC streamfunction maximum at 26N. Thus it could be argued that it is fair to consider both the Ekman and Florida Straits transports terms of the MOC. This of course is dependent on the timescale of interest (only seasonal and sub-seasonal are alluded to here) and definition of the MOC. Furthermore, because the Florida Straits and Ekman transports comprise almost all the northward flow of water in the upper ocean at 26N, adding their combined transport to southward Sverdrup transport theoretically provides an index of MOC strength (though this relies on simple dynamics that may not hold in the real ocean).

The discussion section 'Relationship of Florida Straits, Ekman and Sverdrup transports to AMOC variability' (formerly 4.4.1 'Transport Variability') has been slightly expanded to further address these ideas.

Page 950-951. The conclusions are well written and my favourite part of the paper. Good way to end.

Figure 2: These transports should really be called transport anomalies. Say STDs are grey.

Changed to read seasonal transport anomalies, not seasonal cycles. The envelope surrounding the daily means is pale blue and represents 1 standard error (as stated in the methods section) thus has not been changed.

Fig 3. e,f,h,i have black spots and the colour is not defined. It might be obvious but it is not correct to say that d-f and g-i are the same as a-c if you change the colour scheme.

Final sentence added to caption:

'Note that for Ekman and Sverdrup transports, black crosses are used to denote all harmonics from 1980-2007.'

Fig 10. Very unclear. Not really useful to have the arrows here.

Figure 10 has already been discussed as part of the response to referee 1.