Ocean Sci. Discuss., 6, C931–C933, 2009
www.ocean-sci-discuss.net/6/C931/2009/

Ocean Science
Discussions

Interactive comment on "The gyre-scale circulation of the North Atlantic and sea level at Brest" *by* P. L. Woodworth et al.

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Received and published: 21 December 2009

Author Comments on Reviews of "The Gyre-Scale Circulation of the North Atlantic and Sea Level at Brest" by P.L. Woodworth et al.

Reviewer 1

We are very grateful to Prof. Sturges for his interest in this paper. Prof. Sturges reproduced our figures and conclusions, tried some ideas of his own (some mentioned in his two reviews), and discussed the paper with colleagues. That was the sort of interest amongst oceanographers (and ocean modellers in particular) that we hoped to encourage by submitting the paper to Ocean Science. In brief, we believe (as does Reviewer 2, see below) that the suggestions raised by Miller and Douglas, and ex-

tended in the present paper, need more detailed investigation. We take on board the implication in his two reviews that our paper needs a little more statistical information on the relationship between sea level and air pressure, and we will attend to that in a second version.

Reviewer 2

We are also very grateful to Prof. Tsimplis for his comments. His remarks in the first paragraph of his section (1) are very reasonable i.e. that the implications of the Miller and Douglas paper, and of this paper by extension, need to be further assessed. However, we cannot do that ourselves. In our opinion, it requires detailed ocean modelling which is why we submitted the paper to Ocean Science, hoping to stimulate oceanographers (like Reviewer 1) and modellers to take up that challenge. Meanwhile, we cannot comment on the far reaching consequences, as he suggests, any more than Miller and Douglas did. Similarly, we do not want to comment further at the present time as he suggests in his Guidance section (3), although we certainly hope to be able to do so when modelling studies we and others are commencing have been completed.

In the second paragraph of his section (1), he remarks that Gomis et al. (2006) used an NAO index based on the same atmospheric reconstructions used in our paper. That is incorrect. As far as we understand things, the Gomis et al. paper used NAO time series from Luterbacher et al. (1999 and 2002) which were based on instrumental air pressure and some proxy information (all terrestrial in source), whereas our study has been based on the recent air pressure field reconstructions of Küttel et al. (2009) which are based primarily on ship log-book information and thereby provide air pressure information over the ocean itself. The confusion may have come about as there are many co-authors in common in these various meteorological reconstructions. However, Gomis et al. is certainly an interesting and relevant study which we will refer to in the second version of our paper.

We take on board his request to have more information on correlations between pa-

rameters and we shall add a little more on this. (See our remarks to Reviewer 1 above also.). He is incorrect, however, that the data we have used have been heavily filtered. No filtering has been done, other than that implied by presenting annual mean values.

He remarks (to paraphrase him) that, without further analysis on our part, our paper is just a reassertion of the speculation of Miller and Douglas. In a way he is correct: we have not proposed any different scientific interpretation than was included in the Miller and Douglas paper. However, what we have done is show that the relationships explored by those authors persist over timescales more than twice as long as they employed. That, and the need to encourage oceanographers to explain the findings, were our sole aims in submitting the paper to Ocean Science. (We also hope that we have been successful in advertising the availability of some remarkable sea level and air pressure data sets that we are sure will be used by many other authors.)

Interactive comment on Ocean Sci. Discuss., 6, 2327, 2009.