

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: 14 December 2009

This is a speculative but very interesting paper of very important consequence.

(1) Importance of the paper: Apart from the generally well known problem of identifying the cause of global sea level rise and the efforts to close the global sea level budget by various scientists and the IPCC sea level research has been facing a second major associated problem: It has not been clear whether sea level has accelerated over the last couple of centuries. (An acceleration has been claimed over the last decade or so however, in my view, this is not long enough to qualify as acceleration in the “climate” context.) The point is that if an increase of CO₂ in the atmosphere is leading to increased atmospheric temperature and thus increased oceanic volume as well as increased oceanic mass due to ice sheet and glacier melting then one would expect

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some sea level rise occurring during the period in which CO₂ emissions have been increasing. Moreover as the addition of CO₂ has accelerated in the past century then some acceleration is expected to be seen in sea level rise or, at least, some compensating or delaying mechanism must be identified. Such acceleration has been difficult to identify in existing observations of sea level- it could be because there is not enough data, it could be because it did not happen. The leading author of this paper is also the scientist who first identified the possibility of such accelerations in sea level records. These accelerations were found to be of the order of 0.3 mm/yr /century and were based on sea level information from tide gauges primarily of the N. Atlantic European coast. The paper by Miller and Douglas (2007) as well as this paper claim that: “at least part of the century timescale accelerations in European sea level records obtained from tide gauge and saltmarsh data” can be explained by the suggested mechanism. The mechanism suggested is in essence strengthening of the oceanic circulation due to stronger atmospheric centres of action over the Atlantic. Therefore if the assertions made by the authors are correct it is very likely that there has been no acceleration of global sea level rise or it has been much smaller and therefore papers claiming such acceleration on the basis of reconstruction of sea level data, like for example Church and White (2006), may need to be reassessed. Worse, if the reconstruction methods are biased by regional phenomena related to basin or parts of the basins then the question on whether the coastal tide gauge datasets are representative of what is happening away from the coasts will become stronger and the ability to reconstruct may be in doubt. Thus in my view the significance of the paper, provided that the conclusions are robust, is clearly paramount because it questions several parts of the basis for our present understanding of past sea level change. The authors choose to avoid comments of the far reaching consequences their conclusions. I see this as a weakness of the paper that needs to be remedied.

There has been at least one paper by Gomis et al. (2006) where (detrended) sea level in the Mediterranean Sea has been reconstructed on the basis of an NAO index based on the same atmospheric reconstruction used in this paper. The resulting re-

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constructed sea level did show, as Gomis et al (2006) note, an acceleration and the authors note: "Thus if one was observing sea level during the period 1820–1900 and then again between 1900 and 1960, the result would be a change in sea level rise from 0.03 to 0.3 mm/yr for the winters and about a third of this for the whole year. With standard errors from the regression of about 0.04 and 0.07 mm/yr, a statistically significant change of the trends would have been claimed around 1900." Thus this acceleration/ significant change in the atmospheric circulation has been noted before although clearly not linked with sea level acceleration with the particular tide gauge data or with those by Miller and Douglas (2007). In addition it was noted in connection with the NAO a feature well established as influential to sea level variability at decadal and interdecadal scales.

(2) Significance of the results: With the importance of the paper accepted the question then is whether the present paper provides enough support for the claims made. This, in my view can be fulfilled by showing a) the statistical significance of the results in a clear manner and b) there is an estimate of what is the contribution of the suggested mechanism to the observed acceleration, at least at the location of the tide-gauge data. In relation to the first point the authors do not provide a robust statistical analysis. They do claim this is because they "...wished the reader to form his own impression of what appears to us to be a remarkable correspondence between the two quantities on century timescales as shown". With all due respect, the purpose of publication is persuading the reviewers and the readers too and a robust statistical analysis is an important part of understanding whether the mechanism is speculative or the conclusions are robust. A correlation coefficient of 0.5 is stated however there is no statement on the level of significance and the degrees of freedom involved, The data used have been heavily filtered and with the several adjustments made to both datasets a proper statistical evaluation is, in my view, important and far from obvious. In addition a clear statement on how much acceleration is caused by the claimed mechanism (with error bars) is also very important as otherwise the paper is a reassertion of the speculation by Miller and Douglas (2007). The fact that Miller and Douglas (2007) as the authors

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note "...avoided giving correlations completely" surely cannot mean that scientific robustness has been abandoned. Most probably means that the paper by Miller and Douglas (2007) should not have been published without a proper statistical analysis - a failure of the reviewing process of that paper but nothing more. In fact the present paper could be the opportunity for the authors to assess both the statistical significance of the Miller and Douglas (2007) claims as well as that of their own results.

(3) Guidance: Finally as the first author is the leading author in the field a clear statement on whether on the basis of this study and that of Miller and Douglas (2007) the view on sea level acceleration would be most welcome. My understanding is that taking into account the present paper the evidence for global acceleration in the change between the 19th and 20th century is no more supported and the correct conclusion is that the acceleration is regional and due to changes in the atmospheric forcing of sea level.

In conclusion, this paper is an important contribution to sea level science and possibly to climate change science and must be published. The authors can and should provide further details on their results and put the paper in context.

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

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