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Ocean Science Discussions

Interactive comment on "Towards measuring the meridional overturning circulation from space" *by* D. Cromwell et al.

Anonymous Referee #3

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General comments:

The paper addresses a highly relevant scientific question. The methods are valid and as far as I can judge properly applied. The paper is clearly written. The general idea is straight forward to understand. I would recommend publication in Ocean Science after the paper has been complemented by information that is available to the authors, but not to the reader.

Generally I would be interested in much more information than is provided. Just as an example, the authors discuss the drift in their model, but neither give numbers nor show a graph. How long was the spin-up of the model ? What is the remaining drift in global sea surface elevation ? The authors indicate in sections 4 and 5 that the drift

might be important, but the analysis is not done very systematic. How about the drift in bottom pressure ? No discussion is offered in how realistic bottom circulation can be captured with short integration times as (I assume) were used in the high-resolution integration presented. A discussion of the integration time which is needed to reach equilibrium in bottom circulation is needed. For this a comparison with coarser models might be useful.

Secondly, the authors are very careful in their conclusions and leave a lot for the reader to interpret. I would appreciate if the authors could considerably extend their conclusion section and give discussions on how relevant the results are beyond the applicability to the specific model used. For example, they show that their statistical method can predict the overturning strength in the model POCM, but do not discuss in how far the obtained EOFs are relevant for the real ocean or other models. No EOF is shown nor are they compared to available satellite data. Does one find similar patterns in satellite data ? If not why not ? Seeing the EOFs would also allow the reader to compare with their own results and highly increase the usefulness.

The paper is very nicely read, but when going into details, I find it is too brief most of the time (see specific comments). The “technical comments" should also be taken seriously here. Especially, because I believe that some parts have been handle in a slightly sloppy manner (see for example equation (1)) and citations have been used very selectively.

In summary, I like the paper but would like more information.

Specific comments:

1) Section 2.2 and discussion: As commented above: Please discuss the spin-up and provide more hard information about it. This could include linear regressions from the

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spin up simulation for global quantities such as overturning strength, global sea surface elevation, bottom pressure, bottom circulation etc.

2) Section 3.1, Equation (3) and (4): Since the paper is relevant for a wide community it would be good to explain the “jargon" used here. For example, what is $N(0, \sigma^2)$ and E(..) ? These might be obvious for the authors but not for all readers.

3) Section 3.1, paragraph following equation (4): This should be explained in much more detail including a number of equations. In my view, the present representation does not allow to follow the procedure or even reproduce it. The regions should be illustrated with a map, which could be combined with other information for example the EOFs (see below).

4) Section 3.1, equation (5): What is the physical relevance to distinguish predictors of sea surface elevation north and south of 26N ? I would very much like to know how good the prediction is when using only basin wide quantities. At least it should be discussed why these additional predictors were taken. To me it seems that by artifically increasing the number of predictors one can get a better fit with the modelled MOC, but without a physical reason for a specific predictor one does not really improve the ability of the model to predict the future. That is why I would assume that one can get similarly good predictions for the second half of the simulation without distinguishing north and south of 26N. If this is not such, I would assume that the good predictions might be a bit lucky. More relevant than the two additional predictors seems to me how the drift is taken into account, see point 6)

5) Section 4: As mentioned above, I would like to see the relevant EOFs that give the best fit plus time series of the coefficients, i.e. which predictor is relevant at what time ?

6) Secion 4, detrending the data: In line with my request in 4) of only using basin wide predictors, I would be interested to see how a detrending by the global mean sea surface elevation of the model would change the representation. This seems to be the

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relevant detrending to apply as opposed to a pointwise detrending. The method used by the authors of detrending the EOFs goes in the same direction as the global mean detrending, but is less obvious to me.

7) Please give the error as a function of noise level in a graph.

Technical corrections:

1) page 1624, line 16: Add “for example" before citations and add the recent intercomparision by Gregory et al 2005, GRL.

2) line 18: The downward tendency of the overflows shown in Hansen et al 2001 has not proven robust. The paper should not be cited. Instead one could cite Bryden et al. 2006.

3) line 19: The description of Levermann et al 2005 is not sufficient. Hardly any information is given here, but it should.

4) line 25: Give a proper definition of THC in distinction to MOC or omit the distinction of the two.

5) equation (1): I do not think that z_0 can be the sea surface elevation as stated. It should be defined over which depth the maximum is taken to obtain the MOC index used in the figure.

6) page 1626, line 15: It should be noted that other reconstruction of the overturning give smaller values e.g. Ganachaud and Wunsch 2000 and Talley et al. 2003. It should also be noted that other models give stronger overturning, why is this particular model chosen ?

7) page 1630, line 3: black dots not lines

8) Equation (6): Explain the subscript s.

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9) The figure is much to small. I should be cut into 4 separate figures and made bigger. I can hardly distinguish the blue and green lines. In an enlarged figure, it would be nice to give some running mean for the prediction curves. Instead of the spine which is not directly transparent, could one give an annual mean ?

Interactive comment on Ocean Sci. Discuss., 3, 1623, 2006.