Ocean Sci. Discuss., 3, S644–S647, 2006
www.ocean-sci-discuss.net/3/S644/2006/

Ocean Science Discussions

Interactive comment on "Inferring the zonal distribution of measured changes in the meridional overturning circulation" *by* A. M. de Boer and H. L. Johnson

A. M. de Boer and H. L. Johnson

Received and published: 31 October 2006

Thank you very much for your comments. They have helped to identify unclear aspects of the paper. We address each comment below (our response in small letters).

THIS MANSUCRIPT IS, ESSENTIALLY, A COMMENT ON THE PAPER BY BRYDEN ET AL (HENCEFORTH BLC). HAD THAT PAPER APPEARED IN A REGULAR JOURNAL, SUCH AS JOURNAL OF PHYSICAL OCEANOGRAPHY, THEN THIS MANUSCRIPT COULD, AND PROBABLY SHOULD, HAVE APPEARED IN THE NOTES AND CORRESPONDENCE SECTION. IN FACT, NATURE ALSO ALLOWS RESPONSES TO ITS ARTICLES, VIA SHORT LETTERS, AND I'M A LITTLE SURPRISED THAT THE AUTHORS DIDN'T FOLLOW THAT ROUTE.

Our paper is indeed a comment on BLC05, but is not in disagreement with their fundamental results (of a decreased MOC) nor a criticism of their method. We simply discuss the interpretation of their result. As such it is not suitable for Nature. Furthermore, because a slowing of the MOC and the interpretation of observations of it is relevant to a broad community, and very timely, we think that Ocean Science Journal is more appropriate as it allows open discussion by the wider scientific community.

THE AUTHORS MAIN POINT IS THAT SVERDRUP BALANCE PUTS SEVERE CONSTRAINTS ON THE MERIDIONAL FLOW, AND SO THAT ANOTHER, 'MORE LIKELY', INTEPRETATION OF THE BLC DATA IS THAT THERE HAVE BEEN CHANGES IN THE NONLINEAR REGIME OFF THE FLORIDA STRAITS.

Our main point is slightly more subtle. We do not claim that there are restrictions on the meridional transport because of Sverdrup balance (i.e., Eq. 6), because we do not assume a priori that it holds in the North Atlantic. Rather, we claim that the assumptions made by BLC05 lead directly to Eq. 6 in linear regions (where eddies are not prevalent) and therefore that the BLC05 results must be consistent with this equation, whether or not the underlying assumptions are valid. That is why we do not begin our discussion with Eq. 6, but instead derive the equation based on the assumptions used by BLC05. We are also hoping that this comment will attract a wide readership, and not everybody can be assumed to be familiar with Sverdrup theory.

1. I DON'T WHOLLY FOLLOW THE AUTHOR'S ARGUMENTS, AND I CONCLUDE THAT THEY ARE NOT EXPRESSED AS CLEARLY AS THEY MIGHT BE. SVERDRUP BALANCE PROVIDES A RELATION BETWEEN THE LOCAL WIND STRESS AND THE VERTICALLY INTEGRATED FLOW ABOVE A LEVEL AT WHICH THE VERTICAL VELOCITY IS ZERO. THIS LEVEL MIGHT BE TAKEN TO BE THE BOTTOM OF THE OCEAN (ESPECIALLY IF THE BOTTOM IS FLAT) OR A LEVEL OF NO MOTION. IN EITHER CASE IT DOESN'T REALLY CONSTRAIN THE UPPER LEVEL FLOW.

The misunderstanding is probably a result of our failure to properly define what we

OSD
3, S644–S647, 2006
Interactive Comment

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

EGU

mean by the upper layer flow. We use the term upper layer flow to describe all the meridional transport which occurs above the reference level (at 3200m in the ocean interior). With this definition, Sverdrup balance (or more exactly, linear vorticity balance), does constrain the upper level flow. We shall clarify this in a revised manuscript.

AND MY INTERPRETATION OF BLC IS THAT ONLY THE VERTICAL DISTRIBUTION OF THE FLOW HAD CHANGED.

We agree that, by construction since the Gulf Stream and Ekman components have not changed, the vertically-integrated (top to bottom) mid ocean flow has not changed. This is only true in the zonal integral, which is what is shown in the BLC05 figure 2. This figure is indeed confusing, because the zonal integral does not represent the vertical flow structure at any one point on the section. The deep southward transport below 3000m occurs mostly in the DWBC.

THEIR FIGURES ARE HARD TO INTERPET, BUT IN TABLE 1 THERE IS A GREAT DEAL OF CANCELLATION IN THE FLOW AT VARIOUS LEVELS, AND ONE INTERPRETATION OF THAT TABLE IS THAT THE VERTICALLY INTEGRATED FLOW HAS HARDLY CHANGED, BUT ITS VERTICAL DISTRIBUTION HAS, AND THIS DOES NOT VIOLATE SVERDRUP BALANCE. IF DE BOER AND JOHNSON ARE MAKING AN DIFFERENT ARGUMENT THEY NEED TO BE MORE CLEAR ABOUT IT.

Table 1 shows that the zonally-integrated southward transport above 3000m depth has increased. (For all practical purposes, this depth is the same as the reference level at 3200m.) Because the upper layer transport (above 3200m) in the linear basin interior cannot have changed unless the windstress has, the changes listed in table 1 must have occurred in the nonlinear region on the western side of the basin.

2. NOTE THAT DBJ’S EQUATION (4) IS REALLY INCORRECT AS WRITTEN, BECAUSE THE UPPER LIMIT OF THE INTEGRATION SHOULD BE THE BASE OF THE EKMAN LAYER, NOT Z = 0, IF THE VERTICAL VELOCITY IS WE, THE EKMAN PUMPING VELOCITY.

OSD

3, S644–S647, 2006

Interactive Comment

We agree. We should have stated that the integral of Eq. (3) is actually to the bottom of the Ekman layer, and we will do so in a revised version.

I WOULD CAUTION AGAINST USING SUCH FIGHTING WORDS AS ’MORE LIKELY INTERPRETATION’ IN THE ABSTRACT. ’ALTERNATE ITERPRETATION’ WOULD SEEM MORE REASONABLE.

It is not our intention at all to sound aggressive in our comment. We have the highest respect for the authors and their work. However, we do believe that our interpretation is more likely as opposed to just an ‘alternative’ interpretation. In fact, we believe that it is the only valid interpretation of the data (that does not negate the main result) and that is the whole point of our comment.

Interactive comment on Ocean Sci. Discuss., 3, 1653, 2006.