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eEarth Discussions

Interactive comment on "Extracting low frequency climate signal from GRACE data" *by* O. de Viron et al.

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As suggested by 2 referees, we made some deep changes in the paper. (1) We used the latest data set, which is now 4 years long. This makes our computations more robust. In addition, we only use the CSR solution, which improve the readability of our study. (2) We performed the LAD EOF decomposition on 20 years of data.

Two figures are joined showing the new results.

The results of those two new computations confirm our previous results.

The typo and imprecision have been clarified.

Some particular reponses are given here bellow:

Anonymous Referee #1

5. if the LAD time series is 20 years long, why do you only show the time series for 2002-2004 in figure 4?

What we had in mind, when making those computations, was to compare things that should be the same. For this reason, we did EOF analysis of the LAD data only on the time when we had GRACE data. This seems to be a problem for several reviewers. Consequently, we changed to take the whole LAD time series, and show that, there too, the ENSO related signal was one of the first modes.

Anonymous Referee #2

2) The GRACE data that was used spans less than three years of data, and this is very short if one intends to extract inter annual ENSO like signals as is claimed by the authors.

Of course, a study based on 10 year of data would have been more robust. But those data are not available at present. Our purpose in this study was mostly to test if it was possible to make sense out of the interannual signal in the GRACE data. Indeed, up to now, mostly the annual cycle has been tested, and it is well known that the noise level of the available solution is larger than expected. Using the EOF analysis, we were able to find a strong coherent signal at the global scale. The next step of this study was obviously to try to physically interpret this signal, and we find in the ENSO cycle a very promising candidate. As suggested by one of the referees, we redid our analysis with the presently available 4 years of data, and got the same results. In addition, we performed a set of tests on the data, all indicating that ENSO was likely to be the observed signal.

5) section 3 on page 24 and page 25 is well known, I recommend to skip it.

We do not fully agree with the referee. Of course, for number of readers, the EOF decomposition is well known, and they won't need it. On the other hand, when presenting results from this techniques at several meetings, we had many questions about it. Con-

eED

1, S61–S66, 2006

sequently, we think that a very short explanation, as given here, is a good idea, as it allows the other readers to have an idea about the technique without going through the bibliography. We would like to keep it.

6) sentence 9 and 10 on page 26, "In fig 2... SOI" : It may be one ENSO event, but, I don't call this a spectacular result since you should have compared it to several ENSO events. My best guess is that you would need three times the average ENSO period to make such a statement, and this counts to roughly 10 years.

We think we have been very careful in our claims. We have this signal, which is certainly present in the data. It appears that the time series is significantly correlated with the SOI for the time interval we have. This supports the idea that this mode is related to SOI. We do not say “it demonstrates”, simply it supports. If we had published those results with no physical interpretation of the signal, readers would have asked for it. We found the signal to be likely associated with the ENSO, and we give some pieces of evidence for it: the time series is correlated with the SOI, it seems to be associated with a similar signal from hydrology, and this hydrology signal is characteristic of the hydrology variations associated with the ENSO. Of course, we would be more comfortable with 10 years of data, but we have a network of converging facts that seem, at least to us, convincing enough to propose that interpretation.

Anonymous Referee #3

First, while an ENSO signal in the atmosphere has been removed, it has not been removed from the oceans (contrary to what the authors claim). The atmosphere-ocean dealiasing product that is removed during the GRACE processing adequately removes the full atmospheric signal, but not the full oceanic signal because the ocean model that is used for this is a barotropic model that does not adequately capture seasonal and longer signals. In particular, it does not capture the interannual signals associated with ENSO. In order to remove the ENSO signal in the oceans, the dealiasing product should be added back to the GRACE measurements, and then a baroclinic model,

eED

1, S61–S66, 2006

Interactive Comment

like the ECCO model, should be used to remove the full ocean signal. Of course, an atmospheric model would still be needed to remove the atmospheric signal. As a byproduct of this approach, the ENSO signal in atmospheric surface and ocean-bottom pressure could be studied separately and compared to the GRACE measurements, like the authors did for the land hydrology model.

There seems to be a misunderstanding: what we say is that "The ENSO impact on the ocean is known to be maintly steric and is consequently not expected to be associated with a noticable gravity signal", which is very different. In the GRACE product used here the atmosphere and barotropic ocean have already been removed, only the baroclinic ocean should be there in. We tried to see if there were a reasonable correlation between the ECCO Ocean Bottom Pressure signal and the geoid signal associated with GRACE over the ocean, and find it really not significant, even at low frequency. Consequently, we do not feel comfortable to use the interannual signal over the ocean. For those reasons, we choose to focus on the continent and hydrology effects.

Review solicited by the editor

3) It's not clear (at least to me) that how the authors did the EOF decomposition on GRACE data. Was the EOF decomposition performed in gravity (geoid height) domain or surface mass change domain? Every thing is done with geoid. We converted the hydrology mass distribution into the associated geoid perturbation. 6) Appling a 4-month window on a time series of 2.3 years may have significant ending effect on the analysis. Could the authors give some discussions on this issue? We now have 4 years of data. This smoothing limits the boundary effects to 2 months on each side, it should not affect the results.

Interactive comment on eEarth Discuss., 1, 21, 2006.

Figure 1: Results for the EOF decomposition of the GRACE data. This is the second mode, significantly correlated with SOI

eED

1, S61–S66, 2006

Figure 2: Results from the EOF decomposition of the LAD data, over the last 20 years. This is again the second mode, and it is significantly correlated with SOI

eED

1, S61–S66, 2006