

Interactive comment on “El Niño in a changing climate: a multi-model study” by G. J. van Oldenborgh et al.

Anonymous Referee #1

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Review of “El Nino in a changing climate: a multi model study” by Oldenborgh, Philip, and Collins

Only until recently can one take outputs of all climate models and examine how these models simulate important modes of variability, such as the El Nino and Southern Oscillations (ENSO). Thanks to the IPCC process, this is now possible. But where should one start: there are so many models with so much data and so many issues that can be examined. The authors of this paper have done a reasonably good job. Taking into account that the performance of these models is rather diversified, they have, firstly, distilled some of the essential processes of ENSO and benchmarked the large number of models against available observations and reanalysis in terms of these processes. They then assess how ENSO and the mean state may respond to climate change. In terms of changes in the properties of ENSO in the future climate, the paper

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gives a null result. In terms of changes in mean state, the study finds that most realistic models show either no change or a small shift towards an El Niño-like pattern. There are several areas in which further improvement can be made, which I list below.

(Section 3) While this study put all models in the same platform, and because of this, it can not go to too much detail, I believe that each individual modelling group has published separate papers describing in much greater details the simulation of ENSO. The readers should be pointed to these publications. In places, the authors have made comments which are contradictory to those in some of the papers on individual models, for example, at Page 272, Paragraph 4, the description regarding ENSO properties in the CSIRO Mark 3 is inaccurate. The spectral of the Niño3.4 index in that model (see Cai, W. J., Collier, M. A., Gordon, H. B. and Waterman, L. J. (2003): Strong ENSO variability and a super-ENSO pair in the CSIRO coupled climate model. *Monthly Weather Review*, 131, 1189-1210.) shows that the high frequency signal is too strong and that the amplitude is too high. These features are opposite to what is described of that model by the present paper.

Further, I thought that the majority of model suffers from a cold tongue problem in two areas: one, it is too narrow in terms of meridional extent, and two, it extends to far west. The second defect is clear from Figure 2, but the first is not reflected. I believe that the authors' normalisation procedure has hidden this model defect. One way to compare all models is to multiply the EOF ENSO pattern (co-variance EOF) with the one standard deviation value of the associated time series. This would give the one standard deviation anomaly pattern for each model and allows comparison on an equal basis for each model.

The analysis in section 4 finds that for the majority of the models, the amplitude of the thermocline variability is under-estimated. One should note that the variability alone might not be a good indicator. Why not compare the mean thermocline depth, the slope (east-west) and the sharpness (tightness) of the thermocline. These have been found to be the critical parameters that control simulated ENSO properties.

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Section 5 discusses the response of wind to SST anomaly in the central and eastern Pacific. The authors believe that the weak response explains why in most models the thermocline variability is too weak. I don't quite see it, because when I look at the three models (CNRM-CM3, FGOALS-g1.0, and INMCM3.0) with large thermocline responses (greater than the observed) at the location of maximum wind response, the actual wind stress response to central and eastern SST is weaker than in the NCEP. Thus some factors that have not been considered might play a role, or at least the authors' interpretation is not accurate.

Regarding the comment on the narrow latitudinal response, I am not sure this is a low resolution issue. My experience is that higher resolution sometime makes it worse. Also, I am not sure that what is done here serves the intent. Why not use pattern correlation (or regression) analysis with the observed pattern to gauge which model responds in a more realistic manner.

Section 6 is most interesting and it will provide useful information for some individual models to explore the dynamics for any discrepancies.

Section 7 assesses how ENSO and mean state may change under global warming. A clean way to address whether the mean is El Nino-like or La Nina-like is to find (through EOF analysis) an ENSO pattern in the control experiment, and then regress (pattern regression) SST fields from climate change experiment onto the ENSO pattern of the control experiment. By examining the time series of the pattern regression coefficient, one would be able to explore the mean state change and the change in ENSO properties. The other way is applying EOF analysis directly on the SST fields as in Cai and Whetton (2001 and 2000: Cai, W. J., and Whetton, P. H. (2001). A time-varying greenhouse warming pattern and the tropical-extratropical circulation linkage in the Pacific Ocean. *Journal of Climate*, 14 (16): 3337-3355. Cai, W. J., and Whetton, P. H. (2000). Evidence for a time-varying pattern of greenhouse warming in the Pacific Ocean. *Geophysical Research Letters*, 27 (16): 2577-2580). The way in which the authors' analysis is conducted is perhaps not the best as it relies on the normalisation,

which has the effect of artificially enhancing trends in areas where trends are small or suppressing trends in areas where they are strong.

Interactive comment on Ocean Science Discussions, 2, 267, 2005.

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