Review of "The role of atmosphere and ocean physical processes in ENSO" by S. Philip et al.

This manuscript investigates ENSO mechanisms in a perturbed physics ensemble using the HadCM3 coupled GCM. Building on previous work by the authors, an intermediate complexity model (ICM) is used to understand the different ENSO behaviour in the simulations. It is argued that because the mean state does not change between simulations, due to a flux correction devised for each member, the process study can focus on the ENSO coupling feedbacks themselves rather than on the role of the mean state like in multi-model ensembles. Using this approach, it is found that the main feedbacks influencing ENSO properties in this specific ensemble are the response of the SST to local wind variability and damping via atmosphere fluxes. The thermocline feedback and the Bjerknes feedback come second, while atmosphere noise and ocean processes play a minor role.

<u>Recommendation</u>: accept with major revisions, including points 2-5 below clearly addressed.

<u>Main comments</u>:

1. This study builds on a very influential series of papers by the authors who used the powerful approach of an simple model (ICM) to understand ENSO properties in complex coupled GCMs.
2. While I have been very interested in this previous set of studies, I am somewhat puzzled by this one for one major reason: the use of flux correction (FC). Several studies have pointed out the adverse and unphysical impact of FC precisely on ENSO feedbacks (e.g. Dijkstra & Neelin - J. Climate, 1995: Ocean–atmosphere interaction and the tropical climatology. Part I: The dangers of flux correction). FC have the potential to strongly alter surface flux feedbacks (even changing its sign), especially in the east Pacific where errors of un-flux corrected models are large. As the authors argue that heat flux damping is a key feedback explaining ENSO differences, much more care (and additional diagnostics, especially of the FC) should be made to ensure that this is not due to FC. As such, the absence of any convincing argument about the non interference of the FC hampers the rest of the paper, which could otherwise by a valuable contribution.
3. The second main comment is that the ICM fit does not seem to be working so well (e.g. Fig 8). It also unclear why is it used in a different way than in Philip and van Oldenborgh (Clim. Dyn. 2009b). More discussion is needed especially to explain the inconsistencies between the HadCM3 results in the two studies (see point 14 below).
4. The third main comment is the lack of links to physical mechanisms, in particular those associated with the specific modified parameterisations. Only speculations are presented and this was a disappointment when reading the manuscript. A discussion of physics beyond statistical results is often missing. This would also have helped deal with the FC interference issue.
5. The title is much too general to be helpful and should be "The role of atmosphere and ocean physical processes in ENSO in a perturbed physics ensemble"

<u>Detailed comments</u>:

1. The introduction lacks a proper structure with sub-titles (1.1 context, 1.2 methodology, …)
2. P.2039, l.10: other works could be referenced here (e.g.: Dewitte al. J. Clim 2007)
3. P.39, l.28: other works could be referenced here (e.g.: Guilyardi et al. J. Clim. 2009, Kim & Jin Clim. Dyn. 2009 submitted).

4. P.40, l.6: other works could be referenced here (e.g. Philip and van Oldenborgh, Clim. Dyn. 2009, Guilyardi et al. BAMS 2009)
5. P.40, l.11-14: two issues here that would benefit from further discussion: 1) non-flux-corrected model have coherent physics which is not the case of FC models - this should be noted, and 2) the Dijkstra & Neelin study should be presented and discussed as, in the present formulation of the manuscript, the FC is a key weakness of the study. FC can easily reach several 100's of Wm-2 the equatorial eastern Pacific which is the same order of magnitude of flux changes during El Niño or La Niña. How can this not affect damping mechanisms ? I believe a few plots showing the FC for the different members are absolutely needed to deal with this issue (for instance the annual cycle of FC in the east Pacific).
6. P.40-41, l.28, 10: should this paragraph move to section 3 ?
7. P.41, l.1-18: how different are the simulations used here when compared to that of Toniazzo et al. ? This paragraph should be moved to the discussion section as the context for some of the phrases is missing (e.g. thermocline feedback not defined).
8. P.42, l.27: remove "dynamically" as the coupling is also physical.
9. P.43: top: What are the limitations of HadCM3 ? How can they affect the results here ?
10. P.44, top: again the FC issue. Which fluxes are corrected ? Heat and momentum ? It seems the design of these simulations was not aimed at ENSO understanding but to have less drift for regional studies. Given what is discussed in the points above, do the authors really think FC are adding any "credibility" in the context of the present study ?
11. P44, l.9-11: Several studies have shown an impact of the north Atlantic on ENSO (Timmermann et al., J. Clim. 2007, Dong and Sutton J. Clim, 2007, …),
12. P45, l.19: "assuming linearity" in adding those two effects is a very strong assumption that should be both discussed, verified and, if applicable, presented as a limitation of the study.
13. P46, l.9-14: Much more validation is needed as 1) surely the model is not perfect and 2) the rest of the study builds on this. For instance, the HadCM3 model has been documented (cf. Reading University group studies) with a too high atmospheric response to SST and too damped ocean (due to its coarse resolution near the equator). Fig 2. for example shows that gamma has very large errors compared to observations (see Philip and van Oldenborgh Clim. Dyn. 2009, their fig. 4 or Lloyd et al. 2009) : the maximum is not located in the east but in the west Pacific ! Also why is the present HadCM3 model analysis different from that of the CMIP3 version in Philip and van Oldenborgh Clim. Dyn. 2009 (their fig. 4) which does show closer agreement with observations ?
14. P47 l.25: please define "reasonable agreement" as we don't necessarily all share the same "reason" ☺.
15. P.48, l.1: it is unclear if the noise is computed from HadCM3 output or from observations.
16. P.49, l.1-8: A validation of Kelvin waves in HadCM3 would be needed here as it coarse resolution (1.25 degrees) prevents it from properly resolving them. Is the high correlation discussed true for all members ? This is unclear. A thermocline cannot be "important"- a rephrasing needed.
17. P.49, l.23,24: what about this model ? What is the sensitivity to the extent of the box ?
18. P.51, l.1: 3.2 K seems quite large to me !
19. P.51, l.10-12: why this correlation ? Could it be due to clouds (e.g. Lloyd et al. 2009) ? A discussion of the physics beyond the statistical results is missing.

20. P.51 l.20: I strongly believe (unless proven wrong) that without FC, the patterns discussed here would vary as considerably as those in "structurally" different models. So the world "structurally" is misemployed in that context.
21. P.52, l.1-5: the phrase should clarify whether the east or the west Pacific is discussed here.
22. P.52, l.11-12: this statement (and the next phrase or two) is not very convincing from the reading of the figure.
23. P.52, l.22-26: this statement is difficult to follow because of the FC issues discussed above.
24. P.53, l.3-7: the ATM ensemble indeed uses the same ocean parameter BUT also different flux corrections. Can the later explain the differences seen ?
25. P.53, l.8-12: again here how can one rule out the potentially large impact of FC ? Lloyd et al. (2009) and Guilyardi et al. (JClim 2009) proposed the notion of convection threshold: can this apply here as well (and again could the FC artificially drive some members above or below that threshold ?)?
26. Following parag: "some" correlations is vague – please be more specific.
27. P.53-54: I could not find the meridional width of the wind stress response in Table 2.
28. P.54, l.3-4: again, this may not be true when FC are used. Next phrase: please describe Fig 6a before commenting it.
29. P.54, l.10-11: these are speculations. Show it, especially since convective processes may be influenced by the heat FC.
30. P.54, l.14-15: the eyes of faith are needed to find any significant relation in Fig 6b !
31. P.55, l.2: please describe Fig. 7 before commenting it. What can be inferred from this low noise level ? Next lines: is this signal really significant ?
32. P.56, l.9-10. I agree and this could have been a very interesting focus of the paper, given that the author have everything at hand to conclude on this aspect.
33. P.56, l. 25,: clearness -> clarity
34. P.57, l.18: some conceptual models of ENSO do include off equatorial processes.
35. P.57, l. 20-24: only a partial view is given here: what about the majority of members where the ICM *does not* fit the GCM results ? Objective metrics to compute that fit could help (like RMS, etc…).
36. P.58, l. 4: "reasonably good" is both vague and un-justified. Rest of parag. is not very conclusive. Same comment for l.20: "reasonably"
37. P.59, l.1 (cont'd from p. 58): FC can alter feedbacks and the "clean isolation" should be demonstrated rather than just stated.
38. P.59 l.14-15: do the author imply that representing the amplitude and period of ENSO is both not of interest and not important for an ICM to get right ?
39. P.59, l.19-20: Fig. 10 is quite hard to read as it is too small. Rest of page: can you relate these differences to changes in perturbed parameters or all of it is due to flux corrections ?
40. P.60, l.2-6: could this very low sensitivity of LH flux to SST be due to the FC applied ? More has to be shown here.
41. P.60, l. 15-22: this parag. seems out of place
42. P.61, l. 3-6: no revolutionary finding here and I could not find where the ENSO period is discussed.
43. P.61, l. 14-15: the sum of both effects is not provided to assess if this statement is correct as it is impossible to infer from the maps in Fig. 10 alone.
44. P.61, l.18-20: which is considered here: east or west Pacific ?

45. P.61, l.26-27: I have to disagree here: knowing what we know about ENSO in CGCMs, results from a particular ensemble of a particular model using ad-hoc flux corrections can certainly not be generalised !
46. P.62, l.2-6: could this surprising result again be due to the FC employed ?
47. P.62, l.9-10: this result agrees with that of Lloyd et al. 2009 and this could be stated.
48. P.62, l.21-28: this is an interesting discussion and one would like to see more analysis and plots. For instance, my understanding is that the heat flux feedback has an influence on ENSO amplitude in the *east* Pacific, not the west (where LW and SW feedback usually cancel each other and were LH feedback is small).
49. P.63, top: this conclusion is most certainly model specific and this has to be mentioned.
50. P.63, l.23-25: this is not correct for non-linear processes like convection, where a threshold on SST exists. With FC you can have regions where SST is not in balance with the fluxes or where SST/Heat Flux variations fall below/above the threshold due to the FC. Given the dominant role of gamma found here, this aspect needs more careful investigation.
51. P.64, l.1-2: “28 out of 33”: this is first time this number is encountered. How was this devised ? What is the criteria//metric and how is it relevant to the study ? Surely one does not infer this from Fig. 8a and 8b !
52. P.64, l.21-24: in the real system, the damping of SST anomalies is strongest in the east Pacific, where ENSO develops. Why is nothing shown or discuss on gamma on the key region of the east ?
53. P.65, l.4: “can directly be related”: no direct conclusive relation has been shown in this study and this is missing.
54. P.65, l 6-9: Lloyd et al. (2009) who compared gamma in CMIP3 shows that this statement is not correct.
55. P.65, l.14-20: again this conclusion (provided FC do not have a hidden dominant role) holds for this specific model that uses FC. It is important to note this limitation of the study.
56. P.70, table 1: please provide error bars for the simulations as well (for <sigma> and period)
57. P.71, table 2: define terms in table in caption or refer to equation in text
58. P.72, table 3: specify where <sigma> is computed (east or west Pac)
59. Fig. 5 is too small
60. Fig. 9: adding the 1:1 diagonal would help the reading
61. Fig. 10 is too small