**Replies to reviewers.** Additions and changes to the manuscript in *italics*.

Reply to Reviewer 1

Major Comments

1. This is in contrast to reviewer 2 who has requested significantly more detail. To balance these two view-points we have attempted to provide concise replies to reviewer 2's requests.
2. The parameters of the equation are fitted using least squares. This is now indicated in the revised manuscript.

Minor Comments

1. Now clarified by adding the following sentence. *As in Van Oldenborgh et al (2005), the finite upwelling time δ is prescribed from observations (Zelle et al., 2004) and varies from less than one month east of 130W to 5 months at the date line.*

Reply to Reviewer 2

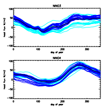
Major Comments

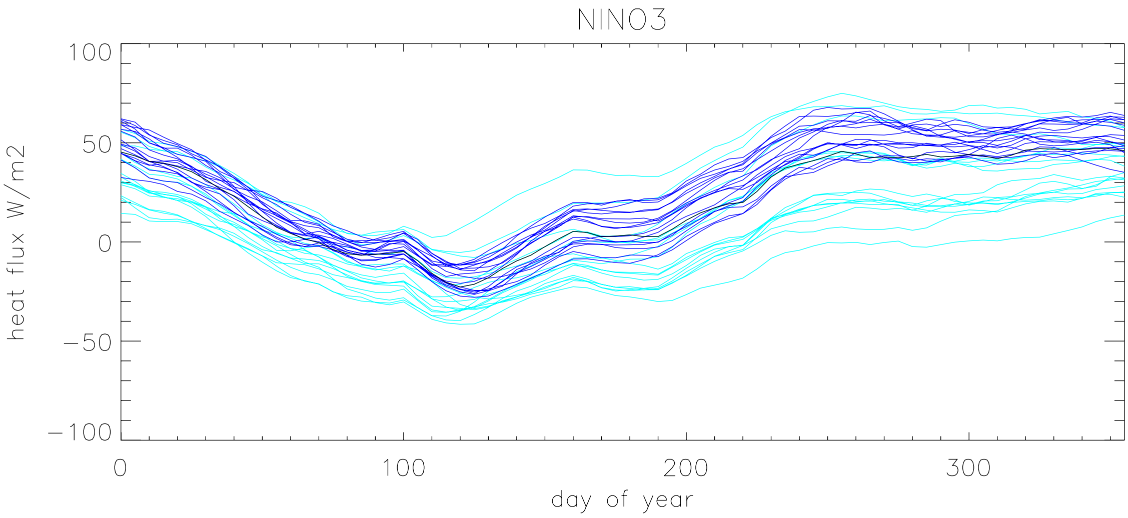
1. Thanks
2. The reviewer is right to point out in this major comment, and in many of his minor comments, that the use of flux adjustments may have important consequences for this study. While we accept that many years ago, Dijkstra and Neeling showed the potential for flux adjustments to impact the dynamics of ENSO in a reduced-complexity model, we think it is interesting to examine these experiments in the light of significant improvements in the ability of complex models to simulate ENSO. We have added to following paragraph after line 11, page 2044: *It should be noted that the flux-adjustment terms apply the same annually varying fluxes of heat and freshwater throughout the experiments. Hence they are invariant with respect to interannual variations in SST, winds and other dynamical variables. Other components of the surface heat flux balance; sensible and latent heating, SW and LW fluxes, can and do vary considerably in concert with variations in SST, winds, clouds etc. While it has been show that flux-adjustments can influence ENSO coupling processes in reduced complexity models (Dijkstra and Neelin, 1995) their role in impacting the ENSO cycle in fully dynamical coupled models is less well understood. Flux adjustments are essential here to prevent considerable model drift that would result in baseline climates much removed from reality and consequently of little interest. In a related project, some perturbed physics versions of HadCM3 have been defined in which the net radiative balance is close to zero and hence flux adjustments are not needed. In these preliminary experiments, the ENSO shows a similar range of behaviour to that presented here suggesting that the flux-adjustments do not limit the validity of this study. Nevertheless, further research should be performed to address this issue as flux-adjusted perturbed physics ensembles have become central in efforts to quantify uncertainty and provide probabilistic climate prediction (e.g. Collins 2007).*
3. The important point is that the differences between the GCM experiments are fitted will by the ICM. The confusion around HadCM3 arises because of different versions of HadCM3 using in this study and in the study of Philip and van Oldenborgh 2009b. The inclusion of the flux adjustments here does alter the dynamics of ENSO in HadCM3 and this has been documented extensively in Spencer et al. 2007 and, to a certain degree, in Toniazzo et al 2008. The Spencer et al study is now cited in the revised manuscript.
4. We have added the following paragraph after line 2, page 2065: *An incredibly useful potential for studies such as this is to improve climate models by linking the attributes of the model climate, in this case the attributes of ENSO, to the individual parameters and parameterisation schemes. Unfortunately, the relatively small number of ensemble members in comparison to the relatively large number of parameters sampled here means that it is not possible to separate the impact of the individual parameters in this way. With several hundred ensemble members, it is possible to "emulate" aspects of the mean climate of versions of HadCM3 that are coupled to a simple slab ocean (Rougier et al., 2009). This will be the subject of future research.*

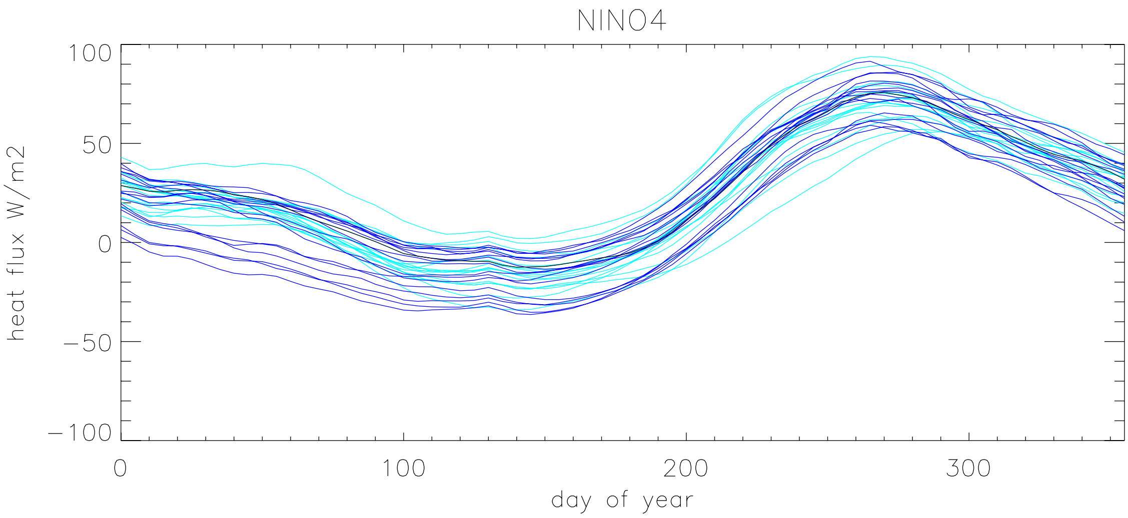
5. We have changed the title to: *The role of atmosphere and ocean processes in ENSO in a perturbed physics coupled climate model.*

Minor Comments

1. It is not common to break down introduction sections in this way unless there is an obvious breakdown to a small number of subsections or if the introduction is very long. We think we have presented a relatively concise introduction and feel that adding subtitles would have a negative impact on the readability of the paper.
2. References added.
3. Guilyardi reference added, although we are not aware of the Kim and Jin paper (only submitted at this stage).
4. References added.
5. The figure below shows the flux adjustment fields in the NINO3 and NINO4 regions in the ensemble (light blue ATM ensemble, dark blue OCN ensemble). This figure could be added to the paper, although we are not sure what purpose it would serve. If the editor would like us to include it, or a similar figure, we would be happy to.






6. Paragraph moved from here to section 3 in the revised manuscript.
7. Section changed to

*The approach is complementary to Toniazzo et al. (2008) who test the variation of ENSO characteristics in a very similar model ensemble. In those experiments, the same HadCM3 atmospheric parameters are perturbed, but with slightly different values. Here we use combinations based on a subset of a much larger ensemble described in Webb et al. (2006) in which a simple slab ocean is used instead of a fully dynamical ocean. In addition, the flux adjustment terms are calculated in a slightly different manner to reduce N. Atlantic and Arctic SST and sea-ice biases and, in this study, we also perturb parameters in the ocean component of the model.*

*In the Toniazzo et al. (2008) study, ensemble members are grouped into subsets of the ensemble with low and high ENSO variability respectively. They find a prevalence for the mode of ENSO which principally involves interactions with the atmosphere and upper ocean (the so-called SST mode – e.g. Federov and Philander (2001)) and trace the variations in the amplitude of ENSO in the different members to variations in the low-cloud cover in the east Pacific and to non-ENSO-related variability in the south-east tropical Pacific. They only find a weak negative relation between ENSO strength and wind response to SST.*

*Here we quantify the influence of different coupling and atmospheric noise parameters of ENSO separately. The influence of the different parts of the ENSO feedback loop is tested in the context of the ICM. This enables us to choose parts of the ENSO feedback loop, individually fit them to different model runs and test the impact relative to a reference run. In the reference ICM we mutually exchange fit parameters from different perturbed physics GCM ensemble members. In this way the influence of each parameter can be quantified separately. The objective is to quantify the importance of different parts in the linear ENSO feedback loop on variations in ENSO.*

8. “dynamically” has been removed
9. We have added the following sentence “*While small-scale processes which effect ENSO are clearly not going to be captured by such a relatively low resolution model, the main large-scale physical feedbacks and processes can be*.”
10. See response to major comment 2 above.
11. We have added the following “*(although some authors have highlighted the possibility of remote interactions (e.g. Timmermann et al., 2007).”*
12. Change to *“and test that the effects add linearly.”*
13. Replaced with *“While these patterns of response are, to leading order, similar to those found when fitting observation fields (fig 4 of Philip and van Oldenborgb, 2009b), there are differences between the modelled and the observed patterns of parameters. The evaluation of ENSO in both flux-adjusted and non-flux adjusted versions of HadCM3 has been well documented (e.g. Spencer et al., 2007) and hence we do not perform extensive further analysis here. We simply note that HadCM3 is competitive with the performance of other models.”* The CMIP3 HadCM3 experiments are not flux-corrected, and hence have different properties, this has been pointed out in the introduction.
14. Changed to “*have a similar leading-order spatial structure to the observations”*
15. Added “*derived from the GCM output”*
16. Added “*(Note that the Kelvin wave speed is a parameter of the ICM. It is not fitted directly to oceanic Kelvin waves observed in the model simulations as these are affected by the coarse spatial resolution of the model. Rather it is a compact way of representing the behaviour of the 20 degree isotherm.)”.* We have replaced “thermocline is important” with *“thermocline is the principal driver of SST anomalies”*
17. Changed to “*As most models, including HadCM3, tend to represent...”*
18. Removed “are not large;” from beginning of this paragraph and “small” from second sentence”
19. Added “*, potentially due to variations in cloud processes across the ensemble (Toniazzo et al., 2008; Lloyd et al., 2009).”*
20. Replaced “structurally different” with “*structurally different non-flux-adjusted”*

21. We are unclear what the reviewer means at this point. This paragraph defines the eastern and western boxes so we feel the manuscript is clear.
22. Added "*in the ATM ensemble (Fig. 5b)"* as we were just referring to that ensemble at this point of the manuscript.
23. Added "*(potentially because of the use of flux adjustments)"* after "perturbing atmosphere-model parameters"
24. See response to major comment 2 above. We do not think that the flux adjustments are the cause here.
25. Again, we have found no evidence that the flux adjustments have a large impact. This part of the discussion is regarding anomalies in clouds etc.
26. This paragraph deleted.
27. Added "*(A-parameters fitted from equation 2)"* after "boxes respectively"
28. This point is implied. We do not believe that it is essential to describe a figure in detail before referring to it (some journals even discourage this).
29. The speculations are based on out physical understanding. A "proof" would require significantly more diagnostics and would distract from the main message of the paper.
30. We admit there is only a weak relationship in the text and even quote the correlation coefficient. We have added "*Although such a weak correlation can only partially explain the variations in ENSO period."*
31. See style comment 28 above. The differences between positive and negative correlations are statistically significant. We are unsure of their dynamical significance. This is left as an open-ended issue for future research.
32. We may pick up this issue in future work.
33. Changed
34. Changed to "*as it is only a conceptual model of equatorial ENSO processes.*"
35. We have removed "remarkable" so that the paragraph gives a more balanced view and added "*For other members, the agreement GCM and ICM variability is not as good."* A quantification of the fit is presented in the text that follows and in figure 9. Further metrics would add significantly to the length of the paper.
36. We have replaced "reasonably good" with "*positive*" and removed "reasonably well" from line 20.
37. Changed to "*Due to the flux-adjusted mean state of the perturbed physics ensemble, there is the potential for greater separation of the effects of mean state errors and coupling processes."*
38. This sentence should have read "…*as the change in σ is mainly seen in small variations in the amplitude and period and not in the pattern…".*
39. Figure 10 can be made bigger at the type-setting phase. See response to major comment 4 above and the significant number of caveats already added regarding the use of flux adjustments.
40. There is nothing special about the flux adjustment in this member (see figure included above). The most likely cause of the low LH flux sensitivity is the combination of atmosphere model parameters.
41. The paragraph falls in a section which describes the interesting behaviour of some of the ensemble members so we don't feel it is out of place.
42. Sure, but it is stated here for completeness. The phrase "and period" is removed.
43. Phrased changed to "*Fig. 10 and other analysis presented indicates that this is a reasonable assumption*"
44. We have added "*..sets of parameters for all ICM boxes with the sets..."*
45. Substituted "general" with "*insensitive to parameter choices"* as this was what was meant.
46. We do not know why this is the case and would not like to speculate.
47. Added *"(see also Lloyd et al. 2009)".*
48. That would be an interesting analysis. Sadly we feel that there is not enough space. We have added "*Further investigation of the relative roles of SW, LW and other surface flux feedbacks in different geographical areas and their possible cancellation would be an interesting area of future work."*

49. Added "*in this GCM ensemble*"
50. Added "*Rather, the leading-order impact of the parameter perturbations is to affect...*" and added a further sentence. "(*However, for non-linear processes such as the threshold triggering of convection, there may be more subtle interactions between the mean state and the physical processes that are perturbed. Such non-linearities are not investigated here.*"
51. Added "*Based on a number of diagnostics, we can say that the ICM successfully...*"
52. The impact of damping is easier to see in the west as there is not so much large SST variability going on. We suspect the damping is impacted by clouds etc. in the east as well so have added "*but also in the east (although the effects are harder to disentangle because of the large SST variability in this region)*".
53. Removed "directly"
54. Added "*(recently Lloyd et al .( 2009) have made some progress however)*"
55. Added "*in the ensemble studied here*"
56. In fact, because the time series used in both the model and observational calculations, there errors are similar. We have added "*Sampling errors in computing model values are of a similar order to those computed for the observations.*"
57. Added "*Terms are defined in equations (1) and (2).*"
58. Added *"amplitude <σ> in the east Pacific (table 1)"*
59. The figure can be made bigger when the paper is typeset.
60. Because the x- and y-axis have the same range, the diagonal is obvious. We would not want to fool the reader into thinking that the diagonal is the line of best fit.
61. The figure can be made bigger when the paper is typeset.