Reply to reviewer #2

The paper by Jouanno & colleagues investigates the role of thermodynamics and dynamics in the Equatorial Atlantic interannual variability, namely Atlantic Nino events. They use a suite of well-designed simplified ocean-ABL experiments together with a mixed layer budget to analyze the different contributions in the heat content budget.

The paper is well presented and relatively easy to follow. The design of the numerical simulations for the present study is appropriate, and the addition of the mixed layer budget is necessary to draw conclusions about the physics of the interannual variability. I recommend the paper for publication, however only after the authors revised several parts of the analysis and their interpretation.

1) The numerical simulations attempt to explore the role of dynamics by replacing the interannual wind variability by its climatology. I find myself wondering how much does the temperature/moisture adjust to this change? The conclusion that dynamics is the number one factor in setting the variability cannot be concluded this way. I would like to see the ML budget analyzed for the REF-tauclim, and most importantly how much is the thermodynamics forcing changed between the two simulation due to the changing winds.

It is not clear to us why the reviewer suggests that the predominance of the mechanical forcing cannot be concluded from our methodology. First of all, the heat and freshwater surface fluxes in simulation REF-τ_{clim} are forced by the interannual data (including inter-annual winds), so the sensible and latent heat fluxes are still function of interannual winds. This means that the direct influence of the wind on the thermodynamic forcing is kept interannual. We agree that the air-temperature, humidity (together with latent and sensible heat fluxes) will adjust but only as a consequence to the changes in mechanical input of momentum to the ocean surface (i.e. through modification of the SST in response to dynamical processes). We carefully read our result and discussion sections and we do not see any conclusion that would be invalidated by the fact that temperature/moisture adjust to the removal of the interannual variability of the mechanical input.

The ML budget analyzed for the REF-Tclim simulation is shown below as Figure R1. It does not bring much more additional information: the seasonal cycle and main balance between the terms are very close to REF and the differences between Nino and Nina years are much weaker than REF. This was expected since removing the interannual variability of the wind stress strongly weakens the interannual variability of the ACT temperatures weak [Fig 2a]. So we prefer to not include/discuss this budget in the manuscript.

2) The explanation of the ML budget is a bit confusing to me. I might have missed this somewhere but the contribution from the forcing to the ML tendency is positive, yet the authors keep saying that it acts as a damping (and vice versa for the mixing). Looking at the plots, I am not entirely convinced by the explanations for either set of runs (REFs or BIASEDs). It might be a wording issue, but this needs to be clarified.

Indeed, the contribution of the air-sea fluxes to the ML tendency remains positive all the year, which means that the atmospheric heat fluxes act to warm the ocean. However, we notice that during Niño years (i.e. warm anomalies), the warming by the air-sea fluxes is reduced compared to Niña years. This implies that air-sea fluxes are not the first order driver of the interannual anomalies in the cold tongue area (if it would be the case we would expect larger air-sea warming during Nino years). We rephrase as follow to try to avoid confusion: "In contrast and as noted earlier in Planton et al. [2017], the warming by air-sea fluxes (FOR) and horizontal advection (HOR) is increased during cold events and reduced during warm events, so these processes act to reduce the temperature anomalies."

3) The difference between Ninos and Ninas events are fairly small in all the plots (especially in the BIASED runs - about 1m change in isotherms!). We cannot clearly distinguish the changes between the events when the seasonal cycle is so dominant. The authors should concentrate on the variability, remove the seasonal cycle, this might give us confidence that the changes seen between the events are significant, and a better way to interpret the changes in the ML terms.

As suggested, we did the exercise to remove the seasonal cycle. This is shown in Figure R2 in this reply. It provides additional information on the fact that Niño and Niña events are almost symmetrical in terms of amplitude, phase and processes (as already shown by Lubbecke and McPhaden 2017, GRL). But our feeling is that keeping the seasonal cycle helps to the interpretation. First, because it provides information on the sign of the different contributions to the ML budget. Second, because it allows to show that the interannual anomalies are weak compared to the seasonal cycle, a specificity of the Tropical Atlantic compared to the Tropical Pacific where Niño/Niña are much larger than the seasonal variability. The "1m change" in the thermocline depth between the warm and cold events in BIASED is informative by itself. It highlights that the surface warm/cold anomalies in BIASED are not due to changes in the thermocline depth.

Some minor comments:
* introduction line 33: should be Bjerknes
This has been corrected.

* simulations page 2 line 33-35: which variables are forced? any issues due to the adjustment of the flow?
The horizontal velocity, temperature, salinity and sea level are specified at the lateral boundaries. This is now mentioned. All the simulations are run from 1958 to 2015 and only data from 1979 to 2015 are analyzed. Owing to the small size of the domain (20°S-20°N), the 21 years spin-up is long enough for the upper ocean flow to adjust.

* simulations page 3: see comment 1) - T and q must adjust to the wind, so there is a thermodynamical adjustment due to the changes in dynamics which might mask the true response.
It is not clear to us what the referee call "true response". When removing the interannual variability of the dynamical forcing, most of the interannual variability of the SST in the cold tongue area is removed. We agree that there is an indirect modification of the air-sea fluxes in response to the changes in the dynamics (due to the adjustment of T and q and also SST), but since they are the result of an adjustment we do not see how they could contribute to the removal of the interannual variability of the SST and avoid our conclusions.

* page 4: Qns and Qs definitions are cryptic, please specify which fluxes they represent; also make sure you use subscripts where appropriate * page 4, line 31: long living = long-lasting ?
We now precise the fluxes they represent : Qns (now renamed as Q*) represent the non penetrative part of the air-sea fluxes and Qs represents the penetrative part of the air-sea fluxes.
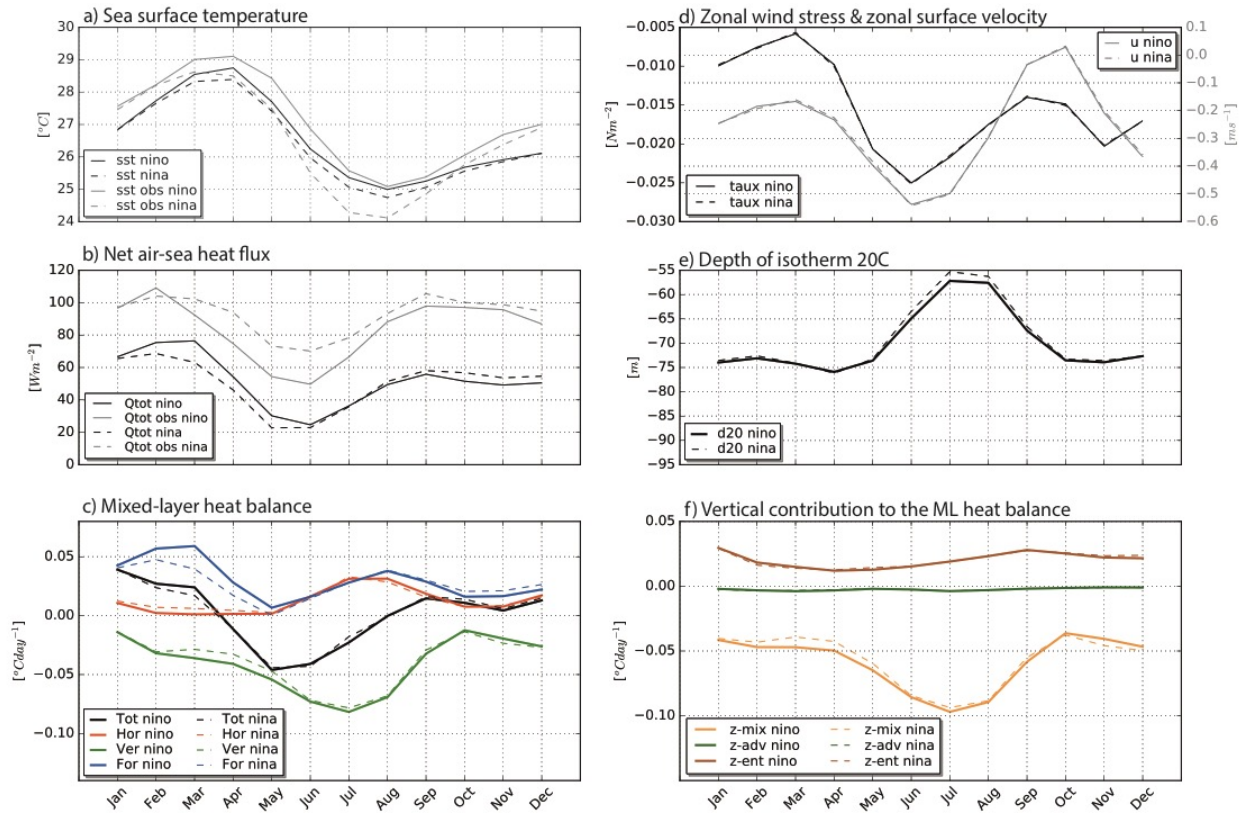"Long living" has been replaced by "long lasting". Thanks.

* page 5, line 10: can you precise what is an acceptable range in term of variance to be more quantitative.
This has been removed.

* pages 5-7: see major comments 1, 2 3 above. In addition, you might want to avoid the use of "most probably", "could also" when building your argument.
We rephrased when required.

Figure R1: As Figure 3 of the manuscript but using REFtauclim simulation. Here, Niño and Niña years were also selected using observed Atl3 SSTs from TropFlux.
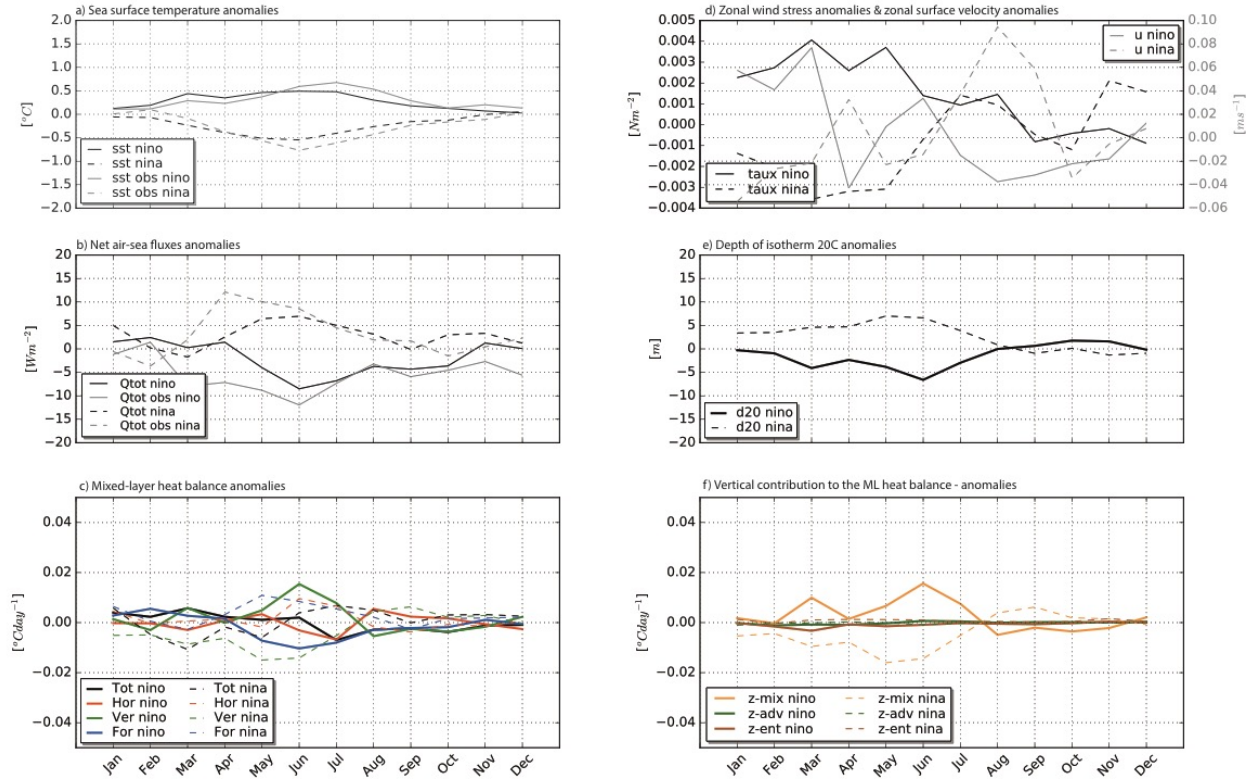
Figure R2. Same as Figure 3 of the paper but shown as anomalies compared to the seasonal cycle.