

Geosci. Model Dev. Discuss., referee comment RC2  
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## Comment on gmd-2021-413

Anonymous Referee #2

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Referee comment on "Assessment of the sea surface temperature diurnal cycle in CNRM-CM6-1 based on its 1D coupled configuration" by Aurore Voldoire et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-413-RC2>, 2022

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### General Comments

This paper describes the developed of a coupled single column model, and provides an example of it's use to explore the impact of coupling on the representation of the diurnal cycle, and it's sensitivity to certain model choices. Such modelling frameworks are relatively rare and the development of a coupled modelling framework for parametrization development, particular for those parametrizations closely linked to air-sea interaction, would be potentially useful in the development of coupled climate models.

It's not clear that the case (or experiments) chosen or the results shown are the best to demonstrate the potential of this framework, as the results about the sensitivity to coupling timescale or vertical resolution in the ocean can largely be obtained from ocean single column models (as in Bernie et al., 2005). Although, the small sensitivity to coupling is in itself a useful result.

Given the strong role of shortwave radiation in driving the diurnal cycle of SST (and in controlling the strength of it in light wind conditions) I find it surprising that there is no mention of the modelled shortwave radiation or cloud cover particular given the noted differences in precipitation (and hence probably cloud) (see 9&10 below). Overall the analysis of the processes resulting in the small differences between coupled and uncoupled or sensitivity experiments in the coupled framework is limited, and doesn't sell the potential of the modelling framework. The authors miss an opportunity to carry out a more detailed investigation of the impact of the coupling on the rectification of the diurnal cycle, to which this framework is uniquely suited (see 9 below).

### Specific Comments

- In lines 156-160 and again in lines 253-255 the authors refer to the fact that the DTR of the mean diurnal cycle and the mean DTR are not the same “as it is a non-linear calculation” and in 254-255 link this to varying amplitudes. This is miss-leading as it is not variations in amplitude which lead these differences but variations in phase (or shape) of the diurnal cycle which can lead to these effects.
- In lines 170-172 the description of the forcing of the atmosphere component could perhaps be improved. In particular the figure seems to indicate (and elsewhere in the text – the description of the advection scheme), that the model is given a large-scale vertical velocity which it uses to compute its own vertical advection term, plus an imposed horizontal advection term, and it is also nudged back to the reference profile. It would help to be explicit about some of these choices, particular as in some atmospheric SCM frameworks, the vertical advection is sometime also prescribed, and vertical velocity information is used only when it is required by a parametrization scheme (e.g. in convective triggering).
- Could the authors provide more information on the discussion of the ocean-tuned experiment L227-L234). The opening line of the paragraph suggests that the vertical diffusion is unchanged (and low) during the whole integration, but surely the presence of the a diurnal cycle in the diffusivity is a critical component of the evolution of the diurnal cycle in the ocean (see e.g. figure 5 of Doney et al., *Journal of Marine Research*, 1995, 341-369, <https://doi.org/10.1357/0022240953213133> ). Is this night time cooling dealt with by a non-diffusive mixing term? It would perhaps be helpful here to see a vertical profile of the diurnal evolution of the ocean, e.g an equivalent of figure 15c. It would also help to see the time evolution of the profile over the course of the integration rather than just a mean profile as shown in figure 3.
- In the atmosphere only experiments why is there a phase shift between the RV Revelle SST and the Atmosphere 1hr SST?
- L267-8 Although the peak amplitude of the precipitation is larger in observations (what horizontal scale and temporal resolution are these), the model precipitation appears to have a longer duration, how to the daily averages compare, although the logarithmic scale would suggest that the extended duration is not enough to account for the differences. If there is a significant difference in the precipitation (and cloud cover) between the observations and the model even when forced with diurnally varying SSTs the subsequent limited sensitivity to coupling is not surprising.
- L290 Is the daytime drying when forced by hourly SST cf daily SST just a relative humidity effect of changes in temperature or does it reflect a drying in absolute terms. Daytime increases in SST would tend to imply increased LH flux and hence moistening during the day, but increased low-level instability could deepen the atmospheric boundary layer, or promote shallow convection that could mix humidity out of the boundary layer, the relative moistening and cooling at 900hPa in figure 7 could be evidence of this.
- L326-7 I’m not sure of the intended meaning of this sentence “This also tends to show that improving the representation of the nighttime cooling necessitates to better represent the processes involved (Moulin et al., 2018).” Do the authors mean that there is a still a need for improvement of the representation of the nighttime cooling?
- L344-350. Whilst the authors are correct to observe that the model DTR is relatively unchanged during the period, they focus on the relatively low DTR period of the second half of the simulations. From figure 10 it would seem that the DTR is comparable to observations during that period (with the possible exception of 20/11), when it is perhaps a bit large and 19/11 when it may be a bit small. However, during the early period it seems that the model seriously underestimates the DTR, and that it is perhaps during this period that the model is most at fault. Figure 10 also seems to suggest a potential weakness of the authors method of measuring the diurnal cycle in that the model seems to have relatively regular diurnal cycle (in terms of it’s phase and shape) where as the observed diurnal cycle shows more variability in shape and possibly timing. This may well be a reflection of the impact of individual clouds passing

overhead, where as the model sees only the grid-box mean cloud field.

- L410 on the rectification effect in the coupled model, this is a rather interesting result and a nice example of the potential value of this framework, but the authors do not give it the attention it deserves given the purpose of the paper. Why does the rectification effect not occur in the coupled framework? Is it because the non-linearity in the latent heat flux and upwelling LW radiation acts as a net cooling effect, or are there changes in the SW flux introduced by the coupling which don't appear in the forced simulation, which offset the rectification when both models have the same forcing?
- L420-425 The discussion of the impact of the coupling frequency on the diurnal cycle might be better illustrated by showing e.g. line graphs of the temperature (or RH) at a given height (or depth) in the respective boundary layers at simply noting that the effect is limited to the lowest (shallowest) height (depth) as in previous figures
- L426-7 The authors indicate there is little impact on the diurnal cycle of cloud cover when changing coupling frequency, but do not indicate anywhere, what the that diurnal cycle of cloud cover is. Indeed, there is no mention of clouds anywhere else in the document, which I find surprising given that one might expect the diurnal cycle to impact the clouds cover (e.g. Ruppert and Johnson, JAMES, 2016, <https://doi.org/10.1002/2015MS000610>), but make no mention of the impact of the presence or absence of the diurnal cycle on the clouds.
- L484-5 "This is probably the absence of dynamical feedbacks in 1D configurations": I'm not sure this statement is justified, haven't the authors shown here that the rectification is a result of a lack of thermodynamic coupling between the atmosphere and ocean.,

## Technical comments

There are a number of grammatical errors in the text, I've not picked them all out, but have tried to identify ones that I found made the paper difficult to read or risk a misunderstanding of the meaning.

L202 "The mean salinity is accurate to a depth of a few meters" might be a better way to phrase this

L224 "A test in which the initial current profile was imposed through the simulation did not strongly impact the thermal profile nor the upper ocean stability" might be a better way to phrase this.

Fig 9. What's the significance of the blue 97%

L363. "This shows the impact .." would be better than "This pictures the impact...."

L407 & L418 suggest replacing "picture" with "show"