

Interactive comment on “Controlling atmospheric forcing parameters of global ocean models: sequential assimilation of sea surface Mercator-Ocean reanalysis data” by C. Skandrani et al.

Anonymous Referee #1

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The authors describe an interesting method for adjusting atmospheric forcing parameters to produce an ocean solution with better representations of SST and SSS than would be possible without the adjustments.

The presentation is quite clear. The outline of their method is easily grasped, although a reader unfamiliar with some of the techniques would have to retreat to the textbooks to understand all the detail, but that's fair enough.

Perhaps one point where I would have liked more clarity was the decision not to include

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wind in the control vector. I wasn't entirely convinced by the remark that it was because of its correlation with wind-stress. Certainly the SST and SSS “observations” wouldn't have provided enough constraint to control the wind, but perhaps SSH could have been included as a surrogate for altimetry. I was left wondering if they had gone down this path and retreated in the face of difficulties or if they were planning to take this up in a further extension of this work? If I seem to belabor this point it's because the tropics are of particular interest and getting the tropics right means getting the wind-stress right.

I think Section 4 on the results is fairly done. The authors are equally clear about the successes and limitations of the method. The results show that, as a technique, the method applied here can largely correct the SST and SSS over much of the model ocean (with some important exceptions). The question then is: are these improvements accomplished through realistic corrections to the atmospheric forcing parameters. It is in response to this question that the discussion in Section 4.3 is satisfyingly frank. For example, the authors show where the parameter increments saturate (P in the tropics, C in the tropics, GS and SO) and where they seem to be compensating each other rather than improving the SST and SSS innovations (CE, CH and Ta at high southern latitudes). As I say a good section, but I am still wondering about a couple of things. I understand why CE and CH increments might be similar, but in Figures 8 and 9, they appear to be almost identical. Is that OK? Also, cloud cover, C, seems to be preferentially saturated. Is that to be expected?

Altogether I think this a good paper describing an interesting method with room for further improvements. In my mind the most important payoff of a method that corrects surface forcing rather than directly correcting the ocean state is that it leaves the ocean dynamics intact and allows for clean ocean diagnostics. This will require further advancements in methods such as this but improved ocean models as well.

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