Review for ‘Sea-ice concentration estimated from microwave radiances in a coupled ocean-atmosphere weather forecasting system’ (manuscript ID: 2026-3421) submitted to TC

In this study, the authors investigate the effects of AMSR2 SIC assimilation on the analysis and forecasts within ECMWF’s new sea ice assimilation framework. The results are quite promising, not only for sea ice analysis and prediction in the polar region, but also for the atmospheric forecasts in the mid-latitudes. Furthermore, the authors provide a clear and informative description of the benefit of the outer-loop coupled assimilation framework adopted in current ECMWF forecasting system, from both scientific and operational perspectives. This discussion is particularly valuable for readers interested in the development of the ECMWF forecasting system. Abundant discussions are also given in terms of the importance of atmospheric or oceanic state on the sea ice concentration prediction and the potential benefit of sea ice thickness assimilation for the model performance.

Overall, I found this manuscript original, well-written, and technically valuable. I provide two major comments and several minor comments below for the authors to consider. I recommend minor revisions before publication.

Major Comment

1) The article seems to be somehow verbose, making it difficult for readers to identify its key message. The issue is reflected in several aspects, including but not limited to the following. Figures 2, 4, and 5 appear somewhat repetitive, as they convey overlapping information. Figures 6 and 7 essentially demonstrate that assimilating AMSR2 SIC brings the analysis closer to observations, although substantial limitations remain in the assimilation procedure. Consequently, the manuscript in its current form reads more like a technical report than a research article, with the main scientific messages somewhat obscured by excessive technical details. Especially, I find the description of the outer-loop coupling framework insufficiently clear, particularly regarding how it operates and how it differs from the previous version. A more concise and focused presentation would substantially improve the readability of the manuscript and help highlight its key scientific and technical contributions.

2) Current version of manuscript focuses more on the quantitative evaluation of forecast

improvements resulting from SIC assimilation. However, a more detailed discussion of the underlying physical mechanisms would strengthen the scientific contribution of the study. For example, the authors show that SIC assimilation improves the atmospheric forecasts over the midlatitudes. However, the physical processes responsible for these improvements remain unclear. I therefore encourage the authors to provide at least a brief analysis or discussion of the mechanisms linking SIC assimilation to improved midlatitude atmospheric forecasts.

Minor Comment

P1 Line7, should this be ‘coupled ocean and sea ice model’ or ‘coupled ocean-atmosphere model’ as indicated in the title? Please clarify and ensure consistency throughout the manuscript.

P5 Lines 137-141, please consider providing the justification for the choice of assimilation parameters, such as the length scales and the error standard deviations. Were these parameters adopted from the previous studies or were based on sensitivity experiments? Please consider to clarify.

P5 Lines 139-141, ‘there are no background error correlations between sea ice concentration and the other ocean variables, so the sea ice analysis does not interact with the wider ocean analysis; in other words it is effectively univariate.’ I’m not fully convinced by the assumption of zero background error correlations between SIC and other ocean variables. Cross-variable background error correlations can play an important role in transferring observational information between different model components. Could the authors provide a more detailed justification for this assumption and discuss its potential implications for the assimilation results?

P7 Line 164, missing dot

P11 Line 284 and P11 Lines 303, what do the three empirical variables refer to?

P21 Lines 476-479, It appears that the model tends to resist decreases in SIC during the forecast. I wonder whether this behavior reflects a deficiency in the model or potential biases in the AMSR2 SIC product. A brief discussion of this issue would be helpful.

P22 Lines 480-485, during which months is the too-slow-developed sea ice most pronounced? This is quite important as the changing of Antarctic sea ice is not linear over time.

P23 Lines 497-498, Could the authors explain why different experiments are evaluated against different reference datasets? How would the results change if the same reference dataset, such as a

state-of-the-art reanalysis, were used for both experiments? In addition, incorporating more in situ observations, where available, could help provide a more consistent and independent evaluation of the two analyses.

P24 Lines 504-505, It is unclear to me how the authors conclude that the RMS difference arises from a small reduction in mean SIC. Could the authors explain the basis for this interpretation and clarify which estimate is more reliable?

P 24 Lines 510, Through what physical processes does the influence of SIC assimilation propagate to the midlatitudes? As noted in Major Comment 2, a brief discussion of the underlying mechanisms would be helpful.

P26 Lines 529, during the summertime of the Antarctic region, SIC assimilation modifies the model performance in the inner Weddell Sea but not the prediction. What might explain this discrepancy?

P27 Lines 549-550, it seems that the impact of SIC assimilation could also impact the stratosphere (Figure 10).

P28, How are the background error covariances between SIC and other ocean variables represented during sea ice assimilation? As noted on P5, Lines 137-140, these cross-variable covariances are assumed to be zero. Could the authors clarify whether and how SIC assimilation influences the ocean state through the coupled assimilation framework?

Figure

1） Figure 7: Two panels are labeled (d).

2） Figure 9: I notice that even in the inner Weddell Sea, where multi-year ice is present, SIC assimilation leads to an increase in SIC, suggesting that the background SIC may be underestimated in this region. Could this be related to the presence of polynyas in this region?

3） Does VW represent wind speed (i.e., $\sqrt{u^2+v^2}$)? If so, how do the results differ when the zonal and meridional wind components are examined separately?

4） Figures 13-14: It would be helpful to explicitly state in the figure captions that cool colors indicate improvements resulting from SIC assimilation.