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Response to the Reviewers' Comments on the Manuscript Titled "The Impact of Synoptic Meteorology on Observed Surface Heat Fluxes Over the Southern Ocean"

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We are grateful for this opportunity to address the reviewer's insightful comments and further refine our manuscript. In this revised version, we have comprehensively addressed the reviewer's concerns, and the responses are summarised below.

1) The introduction section has been revised to include comprehensive studies for the evaluation of ERA5 fluxes across various regions worldwide. The significance of assessing fluxes in reanalysis over the Southern Ocean is thoroughly elucidated.

2) More studies and findings related to open and closed mesoscale cellular convective clouds have been incorporated into the introduction section for clarity and depth.

3) A dedicated subsection for ERA5 has been included in the methodology section, where the variables utilised from the dataset are systematically listed and explained.

4) In this study, we use the M-index and the estimated inversion strength (EIS). Although both are stability metrics, prior work shows that they differ and also demonstrates different abilities to delineate cloud-layer boundaries, with the M-index generally performing better (Naud et al., 2020).

5) The source of open and closed MCC data, as well as the variables used for HYSPLIT runs, has been detailed in the revised manuscript.

6) Figures 2 and 4 have been updated to incorporate the temperature and humidity gradients between the ocean surface and overlying air, with corresponding explanations included in the edited version.

7) Figures 5 and 8, which illustrate the relationship between surface fluxes and the M-index and EIS, have been improved in the revised version. The figure descriptions have also been appropriately added and refined.

8) A discussion on surface flux variability and inversion characteristics specific to different regimes (HPR/CAA) during open and closed MCC cases has been included in the supplementary section. This analysis supports the finding that moderate flux differences observed during all cases of open and closed MCC at the Southern Ocean Flux Station (SOFS) arise from variations in the frequency of occurrence of these cloud patterns relative to different weather regimes.

9) In the revised version of the manuscript, a final section comparing ERA5 fluxes with observed fluxes has been added. This section encompasses a comprehensive comparison of fluxes under different weather regimes, as well as for open and closed MCC cases.

10) All references suggested by the reviewers have been incorporated, along with several additional references that further strengthen our results.

A detailed point-by-point response follows.

Response to Reviewer 1

Comment 1: The organisation of the presentation is somewhat confusing. There are some results on an ERA5 evaluation, others regarding surface fluxes and weather regimes but then the focus becomes open versus closed mesoscale cellular clouds. At times it feels as if the results section is jumping back and forth through all three topics but never really reaches a conclusion for any of them. A decision is to be made on what the ultimate focus of the paper is, and a tighter organisation of the various results leading to the main focus should be implemented. Based on the extensive discussion that finishes the paper on open versus closed MCCs versus meteorology (which is unexpected so probably needs to be better motivated), it would appear the motivation is to examine whether or not surface fluxes play a role or are impacted by cloud organisation. In which case, the evaluation of ERA5 feels like a distraction. Whether ERA5 performs well or not does not seem to matter for the analysis. If it does, this is not clearly expressed. Furthermore, Seethala et al (2021) performed such a comparison in similar cold-air outbreak conditions, but a different location, i.e. the Gulf Stream region. It would make the ERA5 evaluation stronger if the results were compared to this other study. In any case, as currently presented, there are a lot of missing details and analysis to make this evaluation really interesting and solid.

Thank you for your suggestion. The primary focus of the paper is to examine the relationship between surface fluxes and different weather regimes and during open and closed mesoscale cellular convection (MCC). The secondary focus is to evaluate the performance of ERA5 flux data in this context. We agree that in the initial version, the comparison of surface fluxes from observations and the ERA5 is scattered across different sections. In the revised version, we have included a dedicated section for comparing ERA5 and observed fluxes. There, we are comparing the ERA5 and observed fluxes seasonally, during various weather regimes, and also for open and closed MCC.
We have included the citation for Seethala et al. (2021) in the revised manuscript.

So I strongly encourage the authors to focus instead on the MCC versus surface fluxes analysis, and add to it. In particular, it is unclear at present if the two MCC types are examined separately in each weather regime during which they occur. Does the relationship between MCC and their environment different during CAA versus high pressure “HPR”? This aspect should be better delineated, to help place this study in the context of current research on cold air outbreaks in particular.

Our analysis of surface fluxes and their relationship with the M-index and EIS did not consider individual weather regimes in the first version of the manuscript. The limited sample sizes influenced this decision, with 1,643 occurrences for closed MCC and 4,748 occurrences for open MCC. However, we have analysed the fluxes and stability characteristics for open and closed MCC occurrences within high-pressure/Ridging (HPR) and cold air advection (CAA) regimes in the revised version, and the figures are provided in the supplementary section. Analysis indicates that during the HPR regime, the mean sensible heat flux (SHF) for open and closed MCC is -15.5 W/m² and -16.5 W/m², respectively, while the mean latent heat flux (LHF) values are -76.5 W/m² and -80.5 W/m², respectively (Fig. S3, Supplementary Materials for the manuscript). In the CAA regime, the open and closed MCCs show comparable mean SHF values, which are 41.0 W/m² and 45.0 W/m², respectively, while the LHF are -144.9 W/m² and -147.6 W/m², respectively. Despite the similar surface flux release during open and closed MCCs when HPR or CAA occurs, the M-index and EIS differ significantly across these two cloud patterns. In the HPR regime, the mean M-index (EIS) for open MCC is -4.6 (5.3) K, whereas for closed MCC, it nearly doubles to -9.4 (9) K. Similarly, during CAA, the mean M-index (EIS) for open MCC is -0.4 (0.2) K, while for closed MCC, it is -2.8 (5.8) K.

Comment 2: There are a number of technical details that are missing. See the multiple specific comments below on the various subsections of section 2.

We have addressed all the specific comments in the revised manuscript. Thank you.

Comment 3: There are insufficient discussions on how the work fits with current literature and knowledge. Suggestions are made below for information but the list in not exhaustive and by no means the author should feel compelled to specifically use this examples. However, the point here is that the results would be much stronger if they were placed in the context of current research. This way, their overall contribution to current research should be much clearer to readers.

Thank you for recommending the supporting articles. We have cited all of them at the relevant points in the revised manuscript (L24–25, L45–49, L64–66, L71–77, L234–236, L300–303).

Specific comments:

Comment 1: Line 19: Issues with the representation of clouds and associated radiative bias in the southern oceans in most current climate models have been discussed for quite some time now, and at least the paper by Trenberth and Fasullo (2010) that first raised the alarm should be cited, Bodas-Salcedo et al. (2014) who identified the missing clouds as occurring in the cold sector of extratropical cyclones, along with that of Zelinka et al. (2020) on the most recent models still struggling with this.

Thank you for suggesting important articles on Southern Ocean cloud radiative biases. We have cited these articles in the revised version of the manuscript (L24–26).

Comment 2: Line 31: ERA5's citation is not Dee et al (this is for ERA-Interim) but Hersbach et al. (2020).

We have cited Hersbach et al. (2020) in the revised version of the manuscript (L92–93).

Comment 3: Line 62: "the structure of this cloud" is odd, should it be "the structure of this cloud type"?

That particular sentence has been removed from the revised manuscript.

Comment 4: Section 2, data and methods: there is no sub-section on ERA5, despite its heavy use. Please consider adding such a section, to specify which output are used, at what spatial and temporal resolution, which are time averaged or instantaneous, and which are provided vs calculated. For example L78 is the only mention of the ERA5 data being hourly.

More specifically, it is not clear if the surface latent and sensible heat fluxes are calculated by the authors or provided as an output. This is important as it is not clear presently if the input for the flux calculation are at the same atmospheric level for the buoy versus ERA5. And fluxes change if the wind, T, q are at 2m or at 10 m, or some other level. As presented in the manuscript, one wonders if discrepancies between buoys (that are usually measuring at 2-4m) and reanalysis (that often uses 10m as the level of reference) are simply caused by slight differences in where winds are collected.

Thank you for highlighting this point; clarifying it will help avoid confusion. In the revised manuscript, we include a dedicated subsection describing the ERA5 dataset, the variables used, and their specifications. For the flux analyses, we use the direct ERA5 products given as one-hour mean (https://cds.climate.copernicus.eu/datasets/reanalysis-era5-single-levels?tab=overview). The COARE 3.5 algorithm, used to calculate surface flux (All observational data are available at https://thredds.aodn.org.au/thredds/catalog.html), also provides surface variables at the standard reference levels by ERA5. Derivation of wind speed U, air temperature T, and relative humidity RH at standard heights (*U10m, T2m* and *RH2m*) is based on vertical shear structure of the boundary layer, determined by the logarithmic profiles, according to the Monin-Obukhov similarity theory (MOST) (https://www.pmel.noaa.gov/ocs/flux-documentation). This allows a coherent comparison of observations and ERA5 at matched reference heights.

Also, Figure 1 includes profiles, but I could not find any information on where they are obtained. I assume these are from ERA5 but even the figure caption does not say. This should also be mentioned in this subsection on ERA5. How are the profiles averaged? Is there some fixed level that is used as an anchor for the averaging as in Norris (1998)?

In the revised manuscript, we detailed the Skew-T–logP profiles presented in Figure 1, which are derived from the ERA5 pressure-level data at the grid point nearest to the SOFS. Within each regime, we have taken hourly profiles at fixed pressure levels (1000–500 hPa) from ERA5 for Skew-T–log p analysis. Regime-specific composites were derived as the mean and standard deviation at each pressure level; we did not adopt the profile-averaging approach of Norris (1998).

Comment 5: L76: "south of Tasmania", how far is the buoy from land? Why not refer to Fig 1 that actually shows where the station is.

The buoy is situated roughly 500 km from the nearest land of southern Tasmania. Its location is indicated by Figure 1 in the revised manuscript.

Comment 6: L81: Fig S1 suggests there are quite a few periods without data in the early years, does this affect the seasonality? In other words were there seasons with significantly less data? Could this affect some of the results?

Thank you for your comment on Fig. S1 and their potential impact on seasonality. We acknowledge that there are missing data points in the observational records. However, it is important to note that these gaps are not concentrated in specific months or seasons. The distribution of missing data is as follows: DJF (35.81%), MAM (35.09%), JJA (37.68%), and SON (28.88%).

For the year 2022, we have non-missing values and have assessed the seasonality with respect to the climatological data (Fig. 1; see the figure section of this document). Our analysis indicates that the seasonality of fluxes during years with complete data aligns closely with the climatology from 2010 to 2023. In years with complete data, we observed that ERA5's sensible heat flux tends to underestimate observations, while the latent heat flux from the ocean to the atmosphere is overestimated. The pattern is consistent with the climatology, with minor deviations expected as a result of taking the mean over multiple years.

Comment 7: Section 2.2: how many clusters were chosen initially? 5 are discussed, but what happens if 4 or 6 are a-priori chosen?

To fix the cluster number to 5, we analysed cluster numbers ranging from 3 to 8 and found that 5 clusters most effectively represent the individual weather regimes at the SOFS.
For clarification, we have provided the spatial pattern of Mean Sea Level Pressure (MSLP), 975 hPa wind, and mean surface precipitation for cluster number 4 (Fig. S2, Supplementary Materials for the manuscript). The elbow method also suggests k = 4 as an appropriate choice at SOFS (Fig. 2; Supplementary Materials for the manuscript). We found that: - Cluster 0 corresponds to High Pressure/Ridging (HPR), Cluster 1 represents Zonal conditions, Cluster 2 reflects Cold Air Advection (CAA), and Cluster 3 indicates patterns associated with a frontal system characterised by northwesterly flow and considerable precipitation. However, we observed that the Tasman Blocking High (TBH) was not clustered appropriately when using 4 clusters. Based on the observations of MSLP, wind profiles, Skew-T log P characteristics, and seasonality, we concluded that 5 clusters offer the best representation of the weather regime for our analysis. The revised manuscript includes details about the number of clusters selected for the initial analysis, as well as the method used to determine the fixed cluster number of 5 (L162-169).

Comment 8: Section 2.3: EIS and M are obtained with ERA5, correct? This should be specified. For both EIS and M a few words on what their values indicate would help, e.g. EIS large means inversion strong, or M positive means a marine cold air outbreak (MCAO) and shallow convection/instability (e.g. Fletcher et al. 2016a,b use this index to explore a climatology of cold air outbreak). Also, EIS and M are quite strongly inverse correlated. It would be interesting to know what R-squared is at the site for each weather regime.

EIS and the M-index are derived from the ERA5 dataset. The M-index highlights air–sea potential-temperature contrasts, i.e., the potential temperature between the surface and 850 hPa, and is closely tied to surface flux forcing, making it a better predictor of cloud-boundary (base/top) heights (Naud et al., 2020). In contrast, EIS is a capping-inversion strength metric that correlates more weakly with cloud boundaries (Naud et al., 2020).
We computed the R^2 values to quantify the strength of the relationship between EIS and M-index across different weather regimes. The R-squared values we found are as follows: 0.34 for High Pressure/Ridging (HPR), 0.34 for Tasman Blocking High (TBH), 0.4 for Zonal Flow, 0.4 for Frontal Systems, and 0.26 for Cold Air Advection (CAA). These values suggest varying degrees of dependency between EIS and M across these regimes, indicating that the relationship is stronger in Zonal and Frontal regimes, whereas it is weaker for CAA.

Comment 9: Section 2.4: it was not clear to me whether the authors conducted their own open vs closed MCC identification using the Lang et al algorithm or if this is an existing product?
We utilised the open and closed MCC dataset developed by Lang et al. (2022), and these details are included in the revised manuscript (L142-146). This algorithm identifies mesoscale cellular convective clouds using Himawari-8 brightness temperature data, and the dataset covers the period 2016–2021. We have acknowledged Lang in the acknowledgements section for providing the dataset used in this research.

Comment 10: Figure 1/section 2.5: the k-o panels are not clear, at least not in the captions. This is the number of time a trajectory passed through one of these points? Out of how many? Just one per hour with a flux at the buoy? What are the ERA5 input used for the trajectories? More should be explained in section 2.5.
To clarify, the k-o panels show the probability density (on a logarithmic scale) of air mass trajectories associated with each weather regime. The probability density is calculated from the number of times a trajectory passes through each latitude–longitude grid cell, normalised by the total number of trajectories for that regime. In other words, the colour scale represents the $\log_{10}$ of the fraction of trajectories passing through each grid cell, where higher values indicate a greater

density of trajectories. The total number of occurrences for each regime over the study period is: HPR (13,294), TBH (6,905), Zonal (16,476), Frontal (8,945), and CAA (13,950).

The HYSPLIT back-trajectory analysis was done using ERA5 reanalysis data. HYSPLIT requires both surface and upper-air fields, which are provided through two ERA5 files: a 2D file (containing surface variables such as surface pressure, mean sea-level pressure, and surface fluxes) and a 3D file (containing meteorological variables on pressure levels, including horizontal wind components, vertical velocity, temperature, and humidity, https://www.ready.noaa.gov/hysplitusersguide/S141.htm)

Comment 11: Section 3.1: it would help visualising the different weather regimes if the paragraphs for each were numbered or the name of the regime was bold/underlined or such. the trajectories are not discussed for the first 3 regimes it seems.

We have highlighted the names of the synoptic weather regimes using bold font, and descriptions of trajectories are added. Thank you for the comment.

Comment 12: Figure 2: it would help if delta-T and delta-q were also plotted, otherwise it is really hard to see what the air-sea contrast in temperature (c.f. L 169, "their difference is greatest" is hard to see; L170 "this strong SST-AT gradient" is hard to tell) and q is doing through season or across regimes (for fig 4).

Thank you for the insightful comments on Figure 2. In the revised version, we have included delta-T and delta-q in Figure 2. The revised figure (Fig. 2f and 2h in the revised manuscript) highlights that while a surface temperature (SST) and air temperature at 2m reach their lowest between May and September, the difference between them is maximum during this period, approaching nearly 1°C. Similarly, we observe a peak in delta-q from May to September. During this period, the combination of a delta-T, delta-q and stronger winds contributes to an enhanced SHF and LHF release from the ocean.

Also L172 qa is mentioned but we do not know what qs is assumed to be for the LHF. Figure 2 also compares ERA5 and observations, but it is hard to tell what meteorological variables are causing the differences in surface fluxes, differences which tend to look quite systematic throughout the year.

The updated figure also demonstrates that during May to September, ERA5 exhibits a cold bias for delta-T, which contributes to the underestimation of SHF. This underestimation is compounded by the underestimation of wind speed observed in ERA5 compared to actual measurements. In contrast, the ERA5 shows more LHF release from the ocean compared to observations, primarily attributable to an overestimation of the air–sea moisture humidity gradient. The LHF and SHF values are taken directly from buoy measurements. The surface specific humidity (qs) was derived by calculating the saturation vapour pressure over seawater and converting it to specific humidity, following the methodology of Fairall et al. (2003).

Comment 13: Section 3.2: adding some discussion on how the results compare with other studies would help gauge how much this work brings that is novel information about the SO. For example, "CAA" has the strongest fluxes, but this is a well-known feature of cold air outbreaks, cf. multiple papers by Lukas Papritz and colleagues, most notably Papritz et al. (2015) for the SO. So how do the fluxes at SOFS fit into the flux climatology reported by Papritz et al (2015)?

We thank the reviewer for pointing us to Papritz et al. (2015), and we have added this citation to the revised manuscript. In their study, Papritz et al. (2015) found that strong cold air outbreaks play a significant role in winter upward turbulent heat flux at the Antarctic sea-ice edge. They reported that these CAOs contribute about 37% of the heat flux in the Ross Sea and around 28% in the Amundsen–Bellingshausen Seas. Consistent with the findings of Papritz et al. (2015), SOFS shows a notable winter peak in both sensible and latent heat fluxes, with the highest flux exchanges occurring during cold air advection (CAA) conditions.

Line 180, M is high in MCAOs as should be expected given it is the index used to identify MCAOs. The values obtained here are close to those discussed in Fletcher et al (2016a, 2026b) who chose 800 hPa (close to the 850 hPa chosen here) to have M > 0 K as the threshold for flagging an MCAO occurrence. EIS is low because there is no inversion, this is expected. Regarding Fig 5, since M and EIS are so strongly inverse correlated, it might make things simpler to pick one variable and not have both? EIS is more appropriate for regimes where an inversion is present, and M is more appropriate for CAAs. This should be taken into account/discussed. The weaker fluxes in the periods of warm air advection is also discussed in other studies, e.g. Naud et al. 2021, 2023 show a strong contrast in fluxes across cold fronts with large fluxes in the post-cold frontal region of extratropical cyclones versus weaker fluxes in the warm sector.

As suggested, we have added references quantifying the magnitude of flux exchange during CAA and WAA: Fletcher et al. (2016a, 2016b) for CAA and Naud et al. (2021, 2023) for WAA.

To evaluate potential overlap between our metrics, we calculated the regime-wise R^2 values between M and EIS with the following results: - High-Pressure/Ridging (HPR): 0.34 - Tasman Blocking High (TBH): 0.34 - Zonal: 0.40 - Frontal: 0.40 - Cold Air Advection (CAA): 0.26 The variance inflation factor (VIF), calculated as VIF = $1/(1-R^2)$, range from 1.35 to 1.67, which is below the typical thresholds for concern. We're keeping both M and EIS in Figure 5 because they provide complementary insights. The M-index highlights air–sea potential-temperature contrasts, i.e., the potential temperature between the surface and 850 hPa, and is closely tied to surface flux forcing, making it a better predictor of cloud-boundary (base/top) heights (Naud et al., 2020). In contrast, EIS is a capping-inversion strength metric that correlates more weakly with cloud boundaries (Naud et al., 2020).

Line 196: the relationship between fluxes and M is also to be expected as M describes the transition between stable and unstable conditions, therefore from larger to lower cloud cover, and weak to strong fluxes.

The revised Figure 5 shows the dependence of surface fluxes on the M-index across different EIS conditions. For EIS < 0, the flux–M slope is roughly double that for EIS > 0, indicating stronger sensitivity under weak or negative inversion.

Comment 14: Section 3.3: see previous comment on how fluxes are obtained in ERA5 versus the buoy that could create systematic biases. As far as evaluating ERA5 fluxes in the case of CAA, results should probably be discussed in the context of previous work by Seethala et al (2021) who evaluated both ERA5 and MERRA2 in these conditions across the Gulf Stream. E.g., L215, are the overestimates consistent with the biases discussed by Seethala et al?

In this study, we use the bulk surface latent and sensible heat fluxes directly from ERA5, which are diagnosed within the ECMWF Integrated Forecasting System (IFS) using Monin–Obukhov similarity theory and based on the first model level above the surface for temperature, humidity, skin temperature, and wind speed (IFS Documentation, CY41R2).

The observed data of bulk surface flux are taken from SOFS measurements (AODN Thredds Catalogue), where the flux products are derived using the CORE 3.5 algorithm. This dataset also provides 10 m wind, enabling comparison with ERA5 wind at a common reference level. The comparison reveals the underestimation of wind speed in ERA5, which in turn causes an underestimation of SHF (Fig. 4f, revised manuscript). However, in the case of specific humidity or delta-q, ERA5 is drier at the surface, which compensates for the understatement of wind speed and results in an overestimation of LHF from the ocean surface (Fig. 4d and 4e, revised manuscript). Seethala et al. (2022) reported that ERA5 overestimates both SHF and LHF during intense CAOs over the Gulf Stream, with biases up to ~100 W m^{-2}. In contrast, our analysis shows that ERA5 performs significantly better for the CAA cases considered here, with a mean SHF bias of −2.93 W m^{-2} and a mean LHF bias of −12.11 W m^{-2}. Further, this relevant reference is included in the revised manuscript.

question: are the biases in wind a function of wind speed? Same question for biases in T, q? this could help explain the variations with season? Also could this explain the different biases in fluxes across the weather regimes (c.f. L 222)?

To understand how the ERA5 bias in wind speed, temperature, humidity, and SST varies with different magnitudes, we categorised observations into fixed bins and calculated the ERA5 bias for each variable in those bins. Findings revealed that bias in wind speed increases with higher wind speeds (Fig. 2d; see the figure section of this document), which is consistent with the underestimation of wind speed in ERA5 at its higher values. In contrast, for temperature, humidity, and SST, we observed no such relationship; the biases for these variables remained relatively constant across their observed ranges.

Comment 15: L 232: "difference in the SHF and LHF between these boundary layer clouds is moderate" is intriguing. In McCoy et al 2017, one variable that is used to characterize the environments of open vs closed MCC is the air-sea temperature contrast, in addition to EIS and M. Their Fig 8 clearly shows that open MCCs have low EIS, M>0 and delta-T > 0; closed MCC have EIS large, M negative and delta-T small or negative. Using delta-T as a coarse proxy for SHF, one would expect SHF to be positive or small in closed MCC and largely negative (large absolute values) in open MCC. So it would be interesting to know why it is that this contrast in SHF is not seen here, is the delta-T contrast compensated by a wind contrast in the other direction? Furthermore closed MCC should have larger cloud fractions, again forcing SHF to be small or positive. It is intriguing that Fig 4 suggests no clear difference in SST or AT between open and closed MCC but there are larger wind speeds in open MCC, which should bring some contrast in fluxes that is indeed quite modest in Fig 3. Given fig 7a indicates that open MCC dominates in CAA conditions, but closed MCC occurs in both CAA and HPR in relatively close frequencies, one wonders if the weather regime matters: are the comparisons in Fig3/4 between open and closed

MCC performed regardless of regime or for just CAA? This is never specified, but maybe it is an important distinction to make when comparing the fluxes.

McCoy et al. (2017) identified significant differences in the delta-T values associated with open and closed MCCs, with open MCCs showing a larger delta-T compared to their closed ones. Their analysis, as depicted in their Figure 11, reveals that the geographical distribution of open and closed MCCs is notably different. Open MCCs are predominantly situated in the warmer waters of lower latitudes, whereas closed MCCs are more frequently observed at higher latitudes, where SST is lower in general. Thus, when colder air masses advect through relatively cooler SSTs prevalent in higher latitudes, resulting in reduced delta-T values, where the closed MCCs are most frequent. Conversely, in lower-latitude regions, warmer SSTs and cold air advection over that warmer surface water result in higher delta-T, representing the dominant open MCC region in their research.

In our study, we analyse open and closed MCCs at the same location, where there is minimal difference in sea surface temperatures. We have plotted delta-T and delta-q for both cloud types, revealing a similar distribution for open and closed MCCs (Fig. 3; see the figure section of this document). However, wind speeds differ: open MCCs exhibit higher wind speeds, primarily because they occur more frequently in regimes with stronger winds (Fig. S3, in the Supplementary Materials for the manuscript) and moderately stronger fluxes.

We include all open- and closed-MCC cases in the main manuscript because the sample sizes over the study period are limited (Fig. 8, revised manuscript). Additionally, we analyse open and closed MCC separately within the HPR and CAA regimes, with results provided in the Supplementary Materials for the manuscript (Fig. S3). These regime-stratified analyses indicate that surface sensible and latent heat fluxes (SHF, LHF) are broadly similar between open and closed MCC when evaluated within the same weather regime. In contrast, the M-index and EIS differ significantly—even within a given regime—implicating inversion characteristics and boundary-layer stability as the primary controls on the cellular organisation of convection rather than surface-flux magnitude alone.

Comment 16: L 234: I wonder if the wind being more important for fluxes in open vs closed MCC could simply be explained by comparing one type (open) that is mostly occurring after the passage of a cold front and therefore strongly impacted by the winds produced by the parent cyclone, compared to the closed type that occurs slightly more often when the area is dominated by a more "anticyclonic" regime, therefore a lot more quiescent. This leads back to my comment above that comparing the fluxes for each type might have to be done separately in each weather regime when they both occur.

Thank you for the thoughtful suggestion. We agree that open MCCs often follow post-frontal passage with stronger synoptic forcing, whereas closed MCCs occur more frequently in anticyclonic regimes.

In our dataset, however, the sample sizes within each regime are limited, so we did not perform a complete regime-specific flux evaluation during open and closed MCC. As a check, we compared the wind-speed distributions for open and closed MCCs within the HPR and CAA regimes. The mean wind speeds were indistinguishable: in CAA, both MCC types average ≈ 12 $m\ s^{-1}$; in HPR, both are lower but comparable at ≈ 7.5–8.0 $m\ s^{-1}$. The modest flux differences observed in the overall wind (Fig. 4f, in the revised manuscript) during open and closed MCC may primarily result from the frequency with which each MCC type occurs in each regime. For example, suppose open MCC occurs more often in CAA (a windy regime) and closed MCC occurs more often in HPR (a calmer regime). In that case, the pooled averages will show open MCCs having slightly larger wind and fluxes—because they appear more frequently in the windy regime. We have added a few lines about this context in the revised manuscript. A comparison of the fully regime-modulated flux during open and closed MCC would be valuable once larger samples are available. Thank you.

Typos: We have corrected typographical errors in the revised version. Thank you.

Response to Reviewer 2

Comment 1: One of the key focuses of the study is to assess air–sea interactions and boundary-layer instability under open and closed MCC conditions. However, the Introduction provides limited details on the cloud characteristics of MCC. It is recommended that the authors expand this section to contextualise better MCC features and their relevance to air–sea coupling.

We have updated the introduction by providing detailed characteristics of open and closed MCCs, as well as associated air–sea coupling (L71–77, L257-260). Thank you.

Comment 2: Additional references on the link between synoptic storms should be included. eg. (https://doi.org/10.1029/2023JD039386).

The suggested article has been cited in the revised manuscript.

Comment 3: While ERA5 data are central to the analysis, the manuscript would benefit from a more detailed description of the ERA5 dataset (e.g., resolution, temporal coverage, and specific variables used).

A separate subsection is incorporated to provide a comprehensive view of the temporal and spatial resolutions of ERA5, as well as the specific ERA5 variables utilised in this study. Thank you for the valuable suggestion.

Comment 4: The rationale for selecting ERA5, rather than other available reanalysis products, should be explicitly discussed. Providing a short justification for the choice of ERA5 (e.g., its higher resolution, improved representation of fluxes, or suitability for Southern Ocean studies) would strengthen the methodological framework.

The revision incorporates the suggested changes (L37-49).

Comment 5: In Figure 1, trajectory analyses are shown. The manuscript should specify which ERA5 variables were used in the trajectory calculations to enhance transparency and reproducibility.

The revision incorporates the ERA5 variables used in the trajectory analysis (L153-158).

Comment 6: The comparison between observed buoy fluxes and ERA5 reanalysis fluxes is currently explained across two separate sections. It is suggested that this material be consolidated under a single subheading. This will improve readability and allow the reader to more easily understand how ERA5 fluxes differ from observations across the identified synoptic regimes and boundary-layer conditions.

In the revised manuscript, the comparison between the observed and ERA5 fluxes has been addressed within the same section (L287-319). Thank you for your valuable suggestion.

Comment 7: Figure 1 caption is inaccurate: it currently refers to "(k–m)" but should read "(k–o)."

Correction included. Thank you.

Comment 8: Tables 1 and 2: Please indicate the units in the caption.

Correction included. Thank you.

Comment 9: Line 162: Replace “SOFSS” with “SOFS”

Correction included. Thank you.

Response to Reviewer 3

Comment 1: The need for evaluating ERA-5 derived fluxes is not sufficiently substantiated in the introduction.

Thank you for your valuable suggestions. We have emphasised the significance of evaluating ERA5 surface fluxes in the introduction of the revised manuscript (L37-49).

Additionally, the current structure of the manuscript does not clearly justify the inclusion of the ERA-5 evaluation, as the authors present the evaluation and clustering results simultaneously. Presenting the ERA-5 evaluation first, followed by the clustering analysis, would be a more logical approach. This would allow the authors to better justify the use of ERA-5 data in the clustering analysis. This is only a suggestion, and I leave it to the authors to decide on the structure they feel is most appropriate.

We agree that in the initial version, the comparison between ERA5 and buoy fluxes was dispersed across various sections. Consolidating this comparison into a dedicated section will enhance the coherence and structure of the manuscript. However, rather than presenting this comparison at the beginning, we will discuss the performance of ERA5 in the final section (L287-319). This approach will enable readers first to understand the variability of fluxes under different weather regimes and subsequently assess how effectively ERA5 represents this variability. Thank you once again for your input.

Comment 2: The authors averaged the 1-minute observations to hourly data to facilitate comparison with ERA-5. However, several variables in the ERA-5 archive, such as the 2m air temperature, MSLP, and relative humidity, etc. are provided as hourly instantaneous values rather than hourly averages. It is unclear whether the authors used the instantaneous or averaged fields in their analysis, particularly when comparing with the SOFS observations. Please ensure that all datasets are consistent in terms of temporal scale, and clarify this explicitly in the methods section.

Thank you for bringing up this important point. For surface heat fluxes (both sensible and latent), we used the hourly mean rates from ERA5 (expressed in W m^{-2}) and compared them with the hourly mean from the SOFS observations (L99-100, L103-104). For the near-surface meteorological variables, such as 2 m air temperature, 2 m dewpoint temperature, and mean sea-level pressure, ERA5 provides hourly instantaneous values. We compared these hourly ERA5 values with the hourly means from the SOFS observations.

Comment 3: The authors need to provide more detailed information about the buoy observations. It is unclear whether the ERA-5 fields and the buoy data were compared at the same height levels and whether any interpolation was applied. Please clarify this in the methods section.

The buoy data and ERA5 fields, such as 2-meter temperature, 10-meter specific humidity, and 10-meter wind speed, are compared at the same level (L95-97, L109-111). This is because the COARE 3.5 algorithm, used to calculate surface flux, also provides surface variables at the standard reference levels by ERA5. Derivation of wind speed *U*, air temperature *T*, and relative humidity RH at standard heights (*U10m, T2m* and *RH2m*) are based on vertical shear structure of the boundary layer, determined by the logarithmic profiles, according to the Monin-Obukhov similarity theory (MOST) (https://www.pmel.noaa.gov/ocs/flux-documentation).

Comment 4: I feel it would be great if the authors included a diagnostic plot such as an elbow plot or silhouette analysis to support their choice of five clusters in the k-means classification.

In the revised manuscript, the selection of five clusters is detailed, accompanied by a physical rationale related to weather regimes, rather than relying solely on the Elbow method (L161-170). While the Elbow method suggests that four clusters would be a suitable choice (Fig. S2, Supplementary Materials for the manuscript), as they adequately explain much of the structural variability, this approach presents a limitation. Specifically, opting for four clusters didn't capture the Tasman Blocking High, a significant synoptic feature situated near the Southern Ocean Flux station. In contrast, the fifth cluster effectively incorporates the Tasman Blocking High regime and comprehensively represents weather regimes.

Minor comments

Minor comment 1: L31: Correct citation to ERA5 is Hersbach et al. 2020 not Dee et al.

Correction included. Thank you.

Minor comment 2: L50-51: Bharti et al. is cited twice.

Correction included. Thank you.

MONASH University

School of Earth, Atmosphere and Environment,
Monash University, VIC 3800, Australia

Minor comment 3: L50-53: I feel it would be helpful for readers if the authors mentioned which dataset Bharti et al. used in their study.

We have incorporated details about the dataset used in Bharti et al.'s study into the revised manuscript (L61-64).

Minor comment 4: L61-62: SST is already defined above.

Correction included.

Minor comment 5: Some of the constants/variables described after Eqn 1 and 2 did not appear as they are used (Cp, CH, etc.). Please correct them.

Correction included.

Minor comment 6: Section 2.2, and everywhere else: I think a more appropriate usage would be *k-means* instead of *k-mean* (this is also in line with the original paper by Hartigan and Wong, 1979) as the technique involves computing means corresponding to multiple clusters.

Correction included. Thank you.

Minor comment 7: Fig 1: How are the skewT-logP diagrams plotted? From ERA-5 at the nearest grid point to the buoy? Please mention this in the manuscript and the figure caption.

Thank you for your comment. The Skew-T log-P diagrams are generated by using the temperature and dew point (calculated by using ERA5 relative humidity and temperature) profiles at the nearest grid point to the buoy from ERA5 data during the occurrence of each cluster. Subsequently, the mean and standard deviation for each cluster are calculated from all corresponding profiles, and the results are presented in Figure 1. This information is included in the figure caption.

Minor comment 8: I couldn't follow much on the back-trajectory spatial map. I understand that the shading indicate the parcel frequency. How's it calculated? Please mention this.

Figure 1 (k-o) shows the probability density (on a logarithmic scale) of air mass trajectories associated with each weather regime. The probability density is calculated from the number of times a trajectory passed through each latitude–longitude grid cell, normalized by the total number of trajectories for that regime. This information is included in the figure caption.

Minor comment 9: Fig 1 caption: The back trajectories are shown in subplots “k-o”, right? It’s written as “k-m”. Please check and correct.

Correction included. Thank you.

Minor comment 10: L158: I’m not sure I’d describe it as “near-saturated” between 1000—500mb. Lower troposphere (up to ~850mb) is indeed near-saturated.

Correction included. Thank you.

Minor comment 11: L162: Change “SOFSS” to “SOFS”.

Correction included. Thank you.

Minor comment 12: I liked Fig 6 — it’s really useful!

Thank you.

Minor comment 13: Section 3.1 and Fig 4: does it worth showing SST-AT gradient and the moisture gradient as well? I think that might be useful as the authors mention that the cold bias in ERA5 is contributing to a negative bias in SHF.

We have calculated the SST-AT gradient and the moisture gradient, which are presented in Figures 2 and 4 of the revised manuscript. Thank you.

Reference:

Fairall, Chris W., et al. "Bulk parameterization of air–sea fluxes: Updates and verification for the COARE algorithm."*Journal of climate* 16.4 (2003): 571-591. https://doi.org/10.1175/15200442(2003)016<0571:BPOASF>2.0.CO;2

Naud, C. M., Booth, J. F., Lamer, K., Marchand, R., Protat, A., and McFarquhar, G. M.: On the relationship between the marine cold air outbreak M parameter and low-level cloud heights in the midlatitudes, Journal of Geophysical Research: Atmospheres, 125, e2020JD032 465, 2020.

Sincerely,

Sreenath A V

Figures

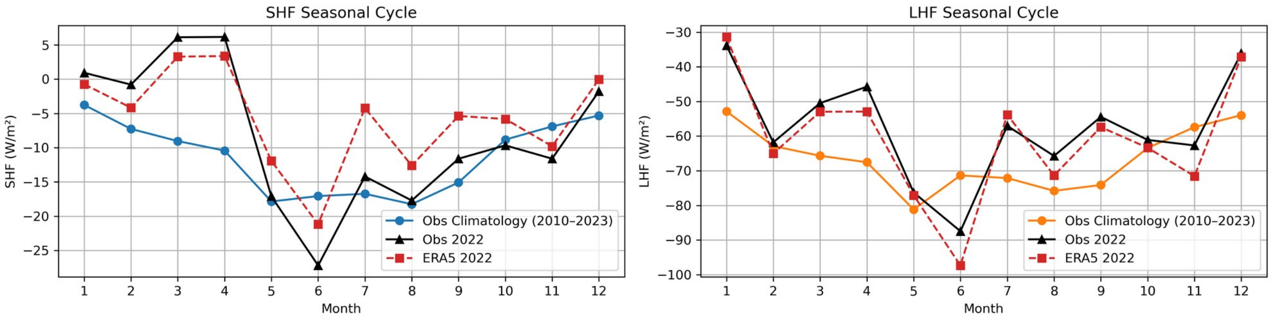

Figure 1: Seasonal cycle climatology (2010–2023) of SHF and LHF from observations, with the 2022 seasonal cycles from observations and ERA5.

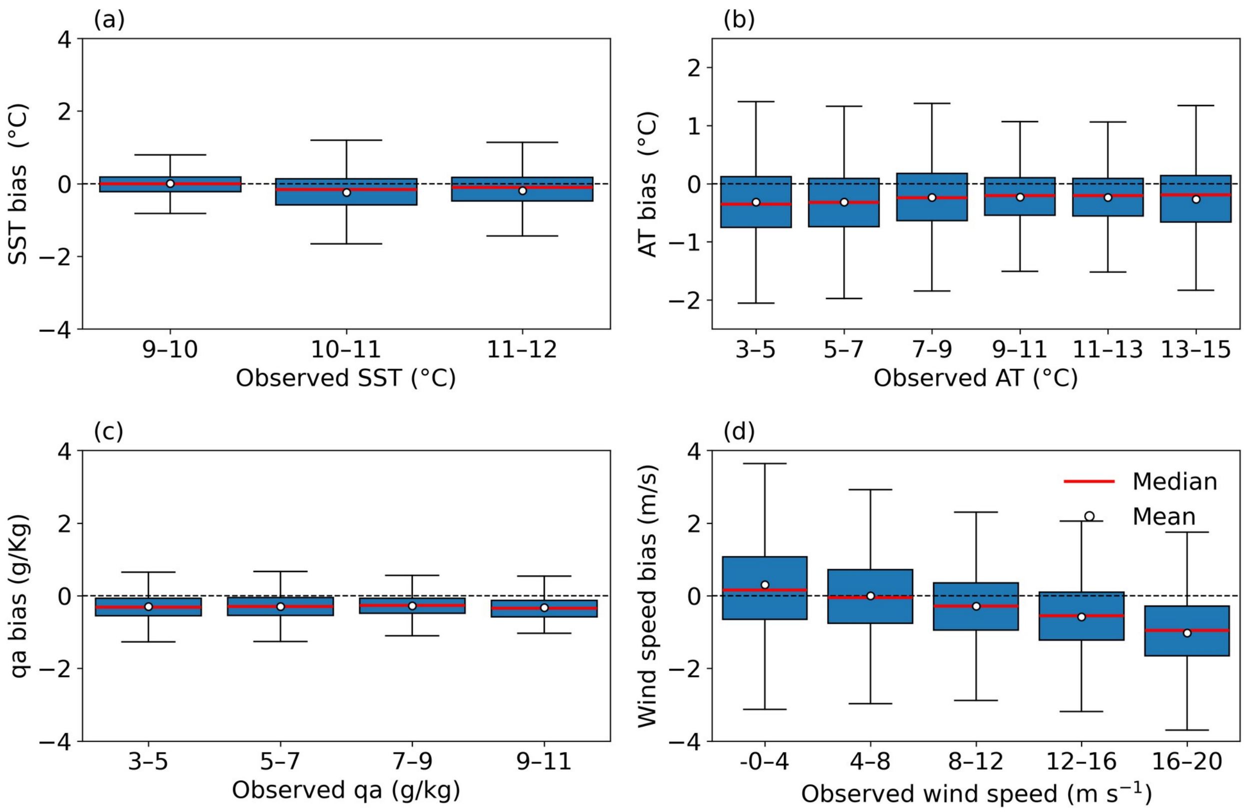

Figure 2: Box and whisker analysis of ERA5 bias on the magnitude of the observed variable: (a) SST, (b) 2-m air temperature (AT), (c) 2-m specific humidity (qa), and (d) wind speed. The x-axis shows the observed variable in different bins; the y-axis shows the mean bias (ERA5 - observations) for each bin.

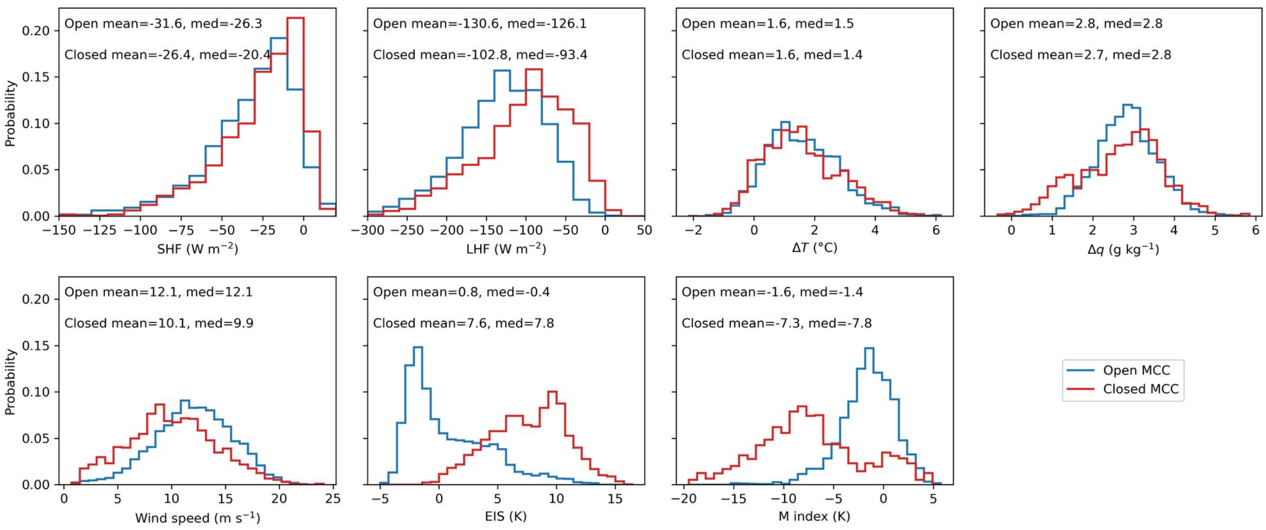

Figure 3: Probability distributions of surface fluxes (SHF, LHF), thermodynamic contrasts (Δ T, Δ q), wind speed, and stability metrics (EIS, M-index) for open versus closed MCC, computed over all cases at SOFS.