Summary: The authors assess the suitability of and differences between reanalysis products regarding winds over the Southern Ocean and the Southern Annular Mode index. They find considerable differences among four contemporary products. They then proceed to characterize the UKESM1 model w.r.t. how it simulates those winds and the SAM index, and what drives multidecadal trends in them. They find that ozone depletion dominates in summer and during the period of increasing ozone-depleting substances (i.e. the late 20th century) but in the 21st century and in other seasons, ongoing greenhouse gas increases drive weaker trends that in the 21st century are counteracting the impact of ozone recovery.

R1.1 I don't have any major issues with the paper but also struggle to see the new, fundamental insights presented by the authors regarding what drives trends in the SAM. The review of the literature does not acknowledge several papers, including foundational ones, that have generally made these points. The comparison of the re-analyses is of course useful, but again the properties found here have at least partly been found in the literature before. The point is made that UKESM1 well represents trends in the SAM index. That may be the case. However, UKESM1 has also been found to simulate quite a large ozone depletion (as acknowledged by the authors), meaning it simulates stronger ozone forcing than any of the few other CMIP6 models with interactive ozone that have been studied, and almost certainly stronger than observed (although observations prior to 1979 are scarce). I thus hypothesize that this fairly large ozone forcing is compensated by difficulties with correctly propagating the dynamical impacts of ozone depletion into the troposphere, i.e. only a muted response ensues in tropospheric pressure and winds. Perhaps the authors can elaborate on this point further.

We have added a new section comparing UKESM1 against available CMIP6 ESMs that ran with interactive chemistry, including observational constraints where we could. With reference to that section; we agree that for the period 1980-1999 UKESM1 has a stronger trend in TCO than most other CMIP6 models (though not the largest trend). This is mainly due to relatively high TCO values in 1980 and the simulated decrease 1980 to ~1990. After this date UKESM1 is rather similar to a few other models. This large TCO trend (1980-1990) does show up when we plot TCO trends against either SAM or average wind speed trends, particularly the change in TCO and SAM trends comparing 1980-1999 against 1980-2019, which do look to be somewhat larger in UKESM1 than most other models (particularly in DJF). These changes are not significantly outside the (relatively weak) observational constraints. Nevertheless, we agree that the UKESM1 TCO trend 1980-1999 is larger than a number of other CMIP6 models (and observations) and this does feed into SAM and wind trends to a degree. We highlight this in the new section comparing UKESM1 with other CMIP6 models, stressing the stronger TCO trend in the 1980-1995 period and the impact of this on SAM and wind trends, for the 1980-1999 period.

Proposed new text:
This text is comprised of a short methods section and section 3.3, which shows the results of the intercomparison. We also include some new figures.

2.5 CMIP6 model intercomparison

To contextualize UKESM1 results, we report trends in the winds and the SAM index, as well as in total column ozone (TCO), for other CMIP6 models with interactive chemistry for which SAM, TCO, and (at least) daily-resolution wind fields are available (Table 2). Spatiotemporal standardization and SAM index calculation is performed as described in sections 2.2 and 2.3. For the intercomparison of the subset of CMIP6 models, we use the historical run for the period 1980-2014 and the SSP 3-7.0 run for the period 2015-2019. We calculate trends in SAM, 10-m wind speed, and total column ozone (TCO) for the band 70°S- 90°S, for 1980-1999 and 1980-2019.We compare CMIP6 TCO trends to an observational dataset (Bodeker et al., 2021). For this intercomparison, to maintain consistency in method, only one ensemble member of each model, including UKESM1, is used.

Table 2

Model	Reference
UKESM1-0-LL	Sellar et al., 2019
CNRM-ESM2-1	Séférian et al., 2019
GISS-E2-1-G	Kelley et al., 2020
MRI-ESM2-0	Yukimoto, Kawai, et al., 2019
GFDL-ESM4	Dunne et al., 2020

Table 2: CMIP6 models with interactive chemistry used in the intercomparison. Only models with both interactive chemistry and 10-m wind fields available at (at least) daily resolution are used. One ensemble member is used per model.

3.3 UKESM1 in the context of CMIP6 models with interactive chemistry

The CMIP6 models with interactive ozone show substantial spread (~1 m/s) in their representation of the mean wind speed over the Southern Ocean (Fig. 6 a)-c)). All models, except GISS-E2-I-G, overestimate the climatological mean wind speed with respect to ERA5. Agreement across the models in wind speed trends is also generally weak. Most models (except MRI-ESM2-0) show larger DJF trends for 1980–1999 than 1980–2019, with GFDL_ESM4 and UKESM1 being notably accurate for both time periods. Furthermore, all models except UKESM1 underestimate the JJA wind speed trend during 1980-1999, though trends in this season are not expected to be strongly forced by ozone trends.

UKESM1 performs well in capturing wind trends compared to other CMIP6 models, reproducing relatively well the wind speed trends seen in ERA5, for both DJF and JJA, as well as for the two time periods considered, including differences between these two periods. UKESM1 falls within the range of CMIP6 models with respect to mean wind speed biases (Fig. 6).

On average, the models overestimate TCO depletion (for the area 70°S- 90°S), though with considerable inter-model variation: MRI-ESM2-0 underestimates depletion, GFDL-ESM4 and CNRM-ESM2-1 are quite accurate, while UKESM1 and GISS-E2-1-G substantially overestimate ozone loss across all seasons and time periods (Fig. 6 lower panels, Fig. S1). Though stronger ozone depletion is expected to correlate with larger wind speed trends, this relationship holds only partially in austral summer. GISS-E2-1-G substantially overestimates ozone depletion in 1980–1999 but fails to produce a correspondingly strong wind speed trend, even in summer. Similarly, CNRM-ESM2-1 reasonably captures the observed ozone loss but underestimates wind speed trends. GFDL-ESM4 best matches both metrics in austral summer but not winter. While the strongest ozone loss typically occurs in austral spring (SON), inter-model TCO trends and their relationship with wind speed trends are consistent whether wind speed trends in DJF are plotted relative to TCO trends in SON or DJF (Fig. S2). To maintain consistency with the rest of this study, we therefore present only DJF results in Figure 6.

UKESM1 overestimates ozone depletion, primarily in the 1980-1999 period, but accurately reproduces the ERA5 wind speed trends for both periods, suggesting the dynamical link between Antarctic ozone loss and near-surface westerlies is weaker in UKESM1 than in reality. The weak TCO–wind relationship across models in austral winter (JJA) for 1980–1999 indicates observed wind increases during this period are very likely not ozone-driven. As in the reanalyses (Fig. 5), SAM trends correlate strongly with wind speed trends across CMIP6 models (Fig. S3), due to the mechanistic link between these metrics. While acknowledging recent trends in Antarctic TCO are larger than observed in UKESM1, our analysis of wind speed and SAM trends, and the implied response to ozone forcing, suggests UKESM1 is suitable for studying (and attributing) past and future changes in Southern Ocean winds, and by extension the drivers of past and future changes in Southern ocean carbon uptake, as carried out in our earlier study (Jarníková et al., 2025).
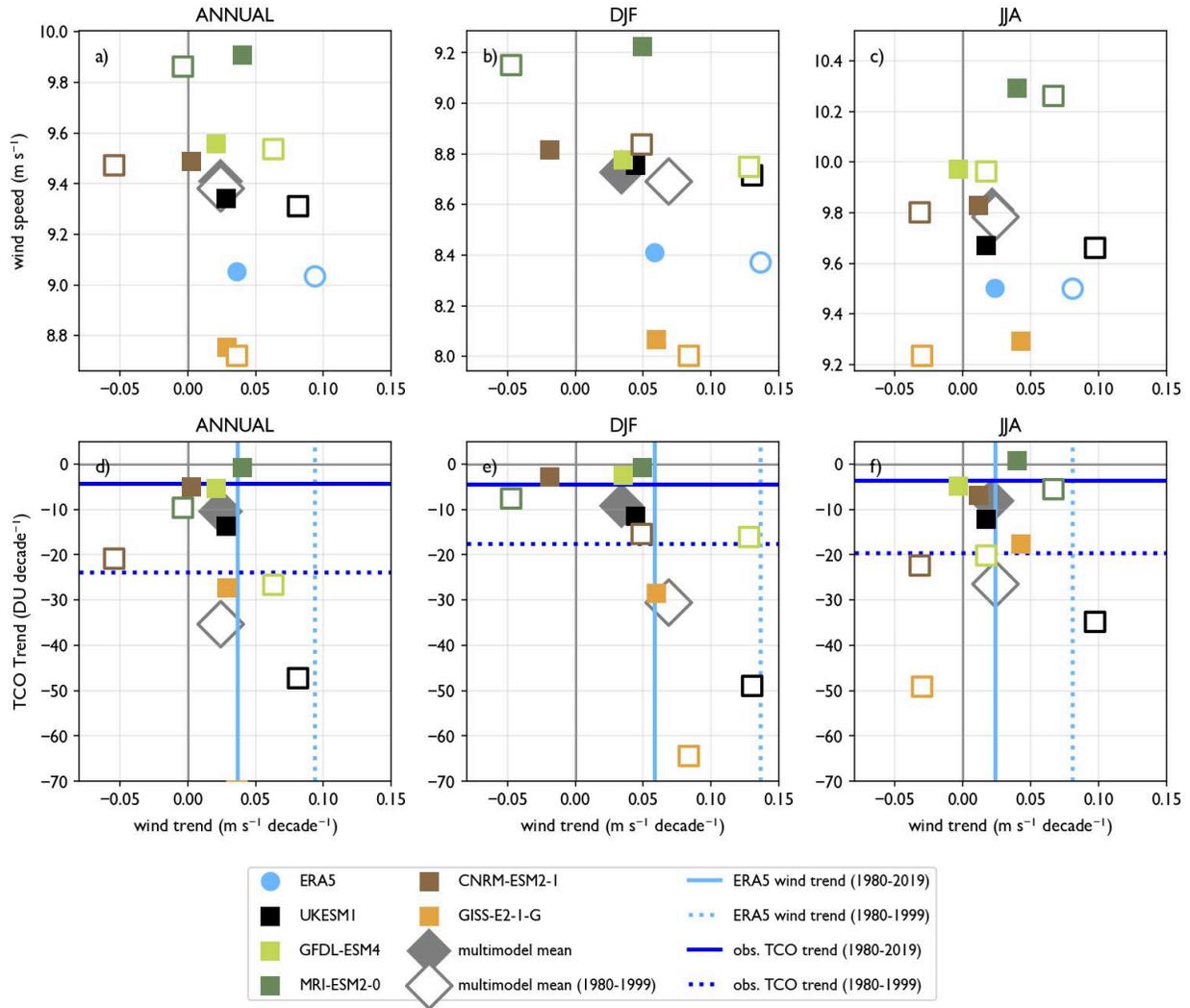
Fig. 6: 10-m wind speed, decadal wind speed trend, and decadal TCO trend for CMIP models with interactive chemistry. *For each model, one ensemble member is used (see Table 2).*
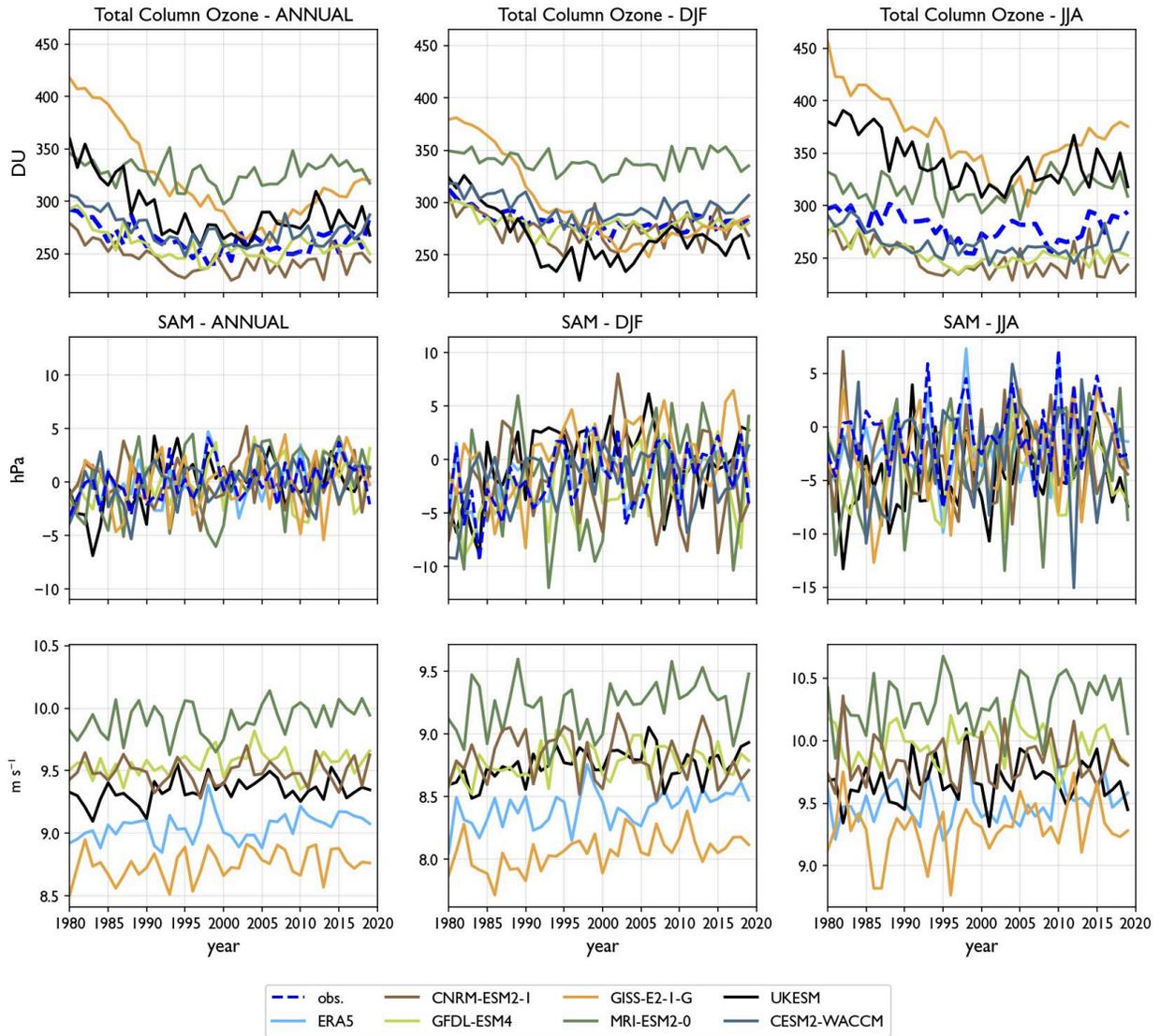
Fig. S1: Timeseries of Total Column Ozone (TCO) for 70-90°S, SAM index, and 10-m wind speed (40-60°S) for CMIP6 models with interactive chemistry. Relevant observations and reanalyses are shown as follows: the Bodecker observational TCO is shown in dashed blue in the top row. The Marshall station-based SAM index is shown in dashed blue in the middle row, and the ERA5 wind speed is shown in light blue in the bottom row.
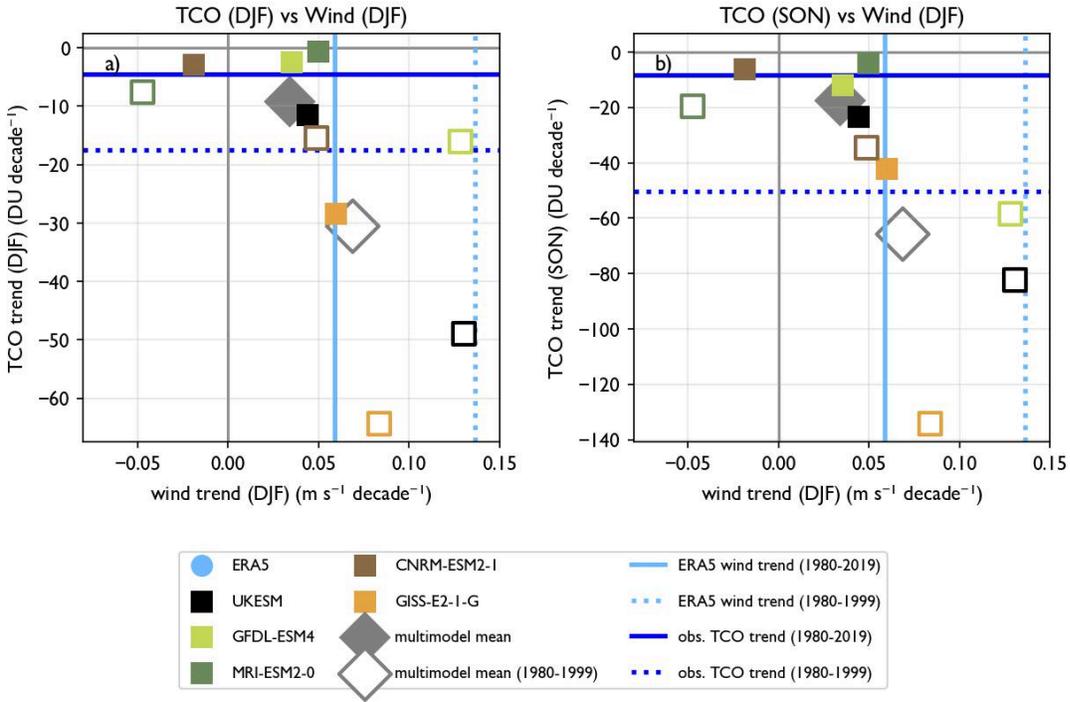
Fig. S2: Trends in TCO (DJF and SON) vs. trends in 10-m winds in DJF. *Though maximum ozone depletion is seen in SON, inter-model TCO trends and their relationship with wind speed trends are consistent whether wind speed trends in DJF are plotted relative to TCO trends in SON or DJF.*
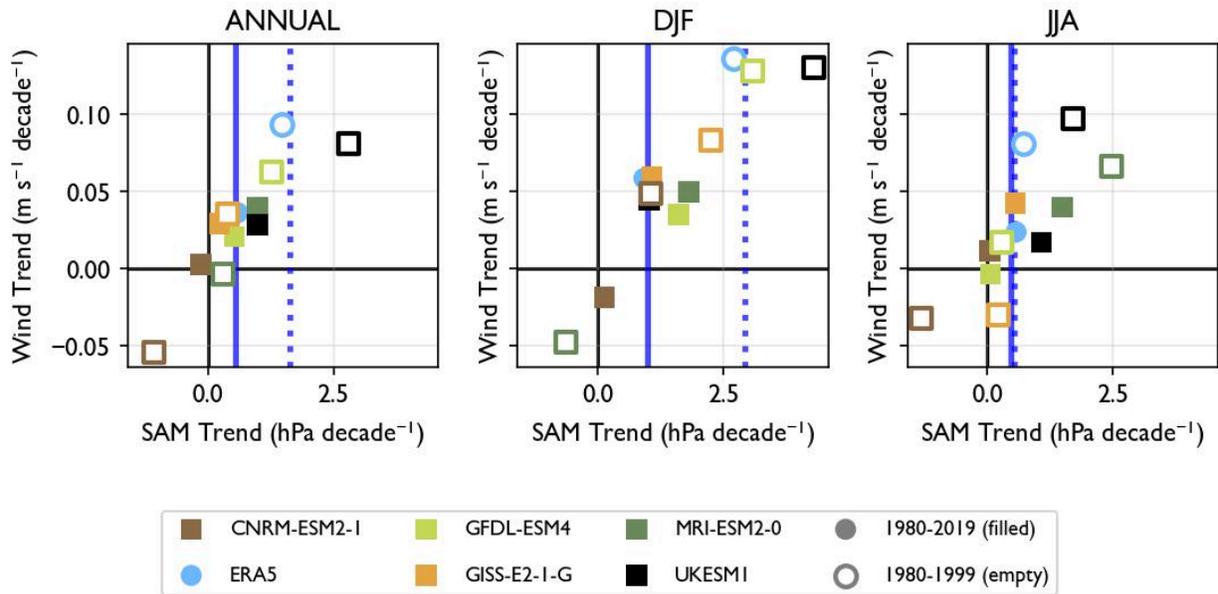

Fig. S3: Trends in 10-m winds vs SAM trends for CMIP6 models with interactive chemistry and ERA5. *See also Fig. 5. For this intercomparison, one ensemble member is used for each model, including UKESM1, so SAM trends here are somewhat higher than in the ensemble mean shown in Fig. 5*

R1.2 Below are several papers that should be touched upon in the discussion:

Son S-W, Tandon N F, Polvani L M and Waugh D W 2009 Ozone hole and Southern Hemisphere climate change Geophys. Res. Lett. 36 L15705

Son S-W et al. 2010 The impact of stratospheric ozone on Southern Hemisphere circulation changes: a multimodel assessment J. Geophys. Res. 115 D00M07

Simpkin & Karpechko, 2012, doi:101007/s00382-011-1121-2

Eyring V et al. 2013a Long-term ozone changes and associated climate impacts in CMIP5 simulations J. Geophys. Res. Atmos. 118 5029–60

Gerber E P and Son S-W 2014 Quantifying the summertime response of the austral jet stream and Hadley cell to stratospheric ozone and greenhouse gases J. Clim. 27 5538–59

Seok-Woo Son et al 2018 Environ. Res. Lett. 13 054024, DOI 10.1088/1748-9326/aabf21
Morgenstern, O. (2021), JGRA, 126, https://doi.org/10.1029/2020JD034161

Maybe not all of these need to be discussed separately, but a more in-depth review of the literature would be in order. Morgenstern (2021) also reviewed some observational products for the SAM index and also found that R1 is an outlier, particularly during southern winter.

Thank you for the suggestion; we have added these and other papers to the literature review and have contextualized our work with respect to them in the discussion.

Proposed new background text:

Substantial modeling efforts have sought to understand the relative contributions of ozone depletion versus greenhouse gas forcing to the observed Southern Hemisphere wind changes. Past work, using several generations of CMIP (Coupled Model Intercomparison Project) and CCMI (Chemistry-Climate Model Initiative) models, has shown the recent Southern Hemisphere atmospheric circulation changes are primarily attributable to stratospheric ozone depletion (e.g. S. Son et al., 2009; S.-W. Son et al., 2018). However, inter-model spread is often large, and the quantitative responses of the winds and other atmospheric responses differ substantially between models (e.g. Gerber & Son, 2014). For example, the poleward migration of the westerly jet tends to be stronger in models whose mean jet position is biased toward lower latitudes (Son et al., 2010). The representation of ozone also plays a role in the character of the atmospheric response. For example, CMIP5 models with interactive ozone have a larger spread in the historical and future changes in jet position than those with prescribed ozone (Eyring et al., 2013). Simultaneously, CMIP6 models with interactive ozone tend to have a stronger role of ozone in the SAM strengthening than models with prescribed ozone (Morgenstern, 2021). Together, these results highlight the importance of correctly representing ozone loss and recovery for capturing circulation changes.

Ozone-driven shifts in the jet stream and wind intensification are expected to reverse as Southern Hemisphere ozone recovers (Polvani et al., 2011; Solomon et al., 2017). However, this recovery will be simultaneously opposed by greenhouse gas-driven warming, which acts in the opposite direction (Arblaster et al., 2011; McLandress et al., 2011; Zambri et al., 2021). The future evolution of the overall wind patterns thus depends on which forcing dominates. Barnes et al. (2014) showed that ozone recovery delays the effect of greenhouse gas driven climate change on multiple Southern Hemisphere climate indicators, including the position of the jet stream, and that the historical ozone-driven circulation changes are larger than those projected to the end of the twenty-first century. Other studies observe that under ozone recovery, the westerly jet that had previously moved poleward in response to ozone depletion remains largely stationary, as the effects of ozone recovery are opposed by greenhouse gas forcing (Gerber & Son, 2014; Son et al., 2010).

Considering trends in the SAM index, which can be taken as a proxy for wind speed, over the period ozone recovery is expected (~2000–2050) Simpkins and Karpechko (2012) show the effects of greenhouse gas forcing, pushing the SAM index to be more positive, opposes ozone recovery acting to make the SAM more negative. In the latter half of the century, they show that the evolution of the SAM is sensitive to the magnitude of greenhouse gas emissions. This sensitivity of atmospheric circulation features to the strength of greenhouse gas emissions is seen in multiple studies (e.g. Eyring et al., 2013, Barnes et al., 2014), highlighting that uncertainties in both future emissions and model response play a role in the uncertainty in future atmospheric circulation changes.

R1.3 Up to normalization, the SAM index presented here is the "Gong & Wang" index (Gong, D., & Wang, S. (1999). Definition of Antarctic oscillation index. Geophysical Research Letters, 26(4), 459–462. https://doi.org/10.1029/1999GL900003). It forms the basis for the Marshall index which as noted is derived from 6 stations, i.e. it is not a zonal mean. This should be acknowledged.

We have added this clarification into the text. We also add some justification for using the zonal mean based Gong and Wang index instead of subsampling the reanalyses to the Marshall stations: first, zonal means are commonly used for standard reanalysis-based SAM calculations (e.g. Morgenstern, 2021, Velasquez-Jimenez and Abram, 2024), likely because they better represent the large-scale circulation patterns that define the SAM, whereas point samples can be influenced by local topographic effects and small scale processes whose representation may differ between reanalyses. In addition, the zonal mean approach is likely more suited for comparison to ESMs (such as UKESM1) due to their relatively coarse spatial resolution.

Proposed new text:

    In the observational SAM index, these MSLP anomalies are calculated from the mean of six station records near each of the two latitudes for which good long-term records exist. In the reanalysis SAM index we simply use the zonal mean at both latitudes as in Gong and Wang (1999). We use the zonal mean rather than subsampling the reanalysis to the six station locations to maintain consistency with standard reanalysis-based SAM calculations (e.g. Morgenstern, 2021) and to avoid sampling biases introduced by point-location extractions from gridded fields. The zonal mean better represents the large-scale circulation patterns that define the SAM, whereas point samples can be influenced by local topographic effects and sub-grid-scale processes that are not fully resolved in reanalysis products.

R1.4 As for the behavior of the UKESM1 model, Morgenstern et al. (2020, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL088295, and 2022, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022JD037123) and Keeble et al. (2021) show that Antarctic ozone loss in this model is stronger than in observations and other CMIP6 models. That should be emphasized more strongly, I think. I'm not sure that the trends at 60-90 degreesS are more realistic than elsewhere (not according to Morgenstern et al. 2020). The amplitude of the trend is just larger than elsewhere.

Please see the response to R1.1 and the new section 3.3 comparing UKESM1 to other CMIP6 models. We now emphasize that UKESM1 has a higher TCO trend over 1980-1995 than some models (and observations) and this does impact SAM and wind trends, particularly for the 1980-1999 period. It is worth noting that the TCO trend in this period for 1980-1995 is not particularly large in DJF compared to other models; rather the bias is more in JJA season, but again not a complete outlier (see figure SF1 in the new section).

We also note that in Morgenstern (2022) that averaged over 75-90S UKESM1 TCO trends are not particularly larger than other models. The main issue highlighted in that paper is that the Antarctic ozone hole extends a bit too long into the summer in UKESM1 (along with most other models, see figure 3 in Morgenstern 2022).

Minor points:

R1.5 P2L41: I don't understand how the polar vortex can be "accelerated". How about "deepens"? There is evidence that the polar vortex is more long-lived with ozone depletion than without, with everythin else in a model remaining unchanged.

I've changed 'accelerates' to 'deepens' here.

Proposed new text:

This cooling strengthens the north-south temperature gradient, which in turn enhances the vertical wind shear and **deepens** the polar vortex,

R1.6 P2L70: There could be effects other than the wind-driven ocean circulation that can limit the usability of a model such as UKESM1 for simulating the uptake and transport into the abyss of CO_2 by the Southern Ocean, such as the quality of simulation of Antarctic bottom water formation. I'm not sure about UKESM1, but in other models this has certainly been found deficient, with impacts on the carbon cycle. So a more careful formulation here might be in order, such as that atmospheric drivers of ocean uptake in this model are fine.

We have modified this section to emphasize that one primary aim in the paper is to assess the Southern Ocean winds (mean, intensity distribution, jet position etc) in UKESM1 and, in particular, their sensitivity to variable ozone forcing. We agree with the reviewer that there are a multitude of other factors that could potentially reduce the quality of Southern Ocean projections, even if ozone and surface winds are well captured; e.g. Antarctic bottom water formation, representation of land and sea ice, connections between the Southern Ocean circulation and the AMOC, cloud amounts/optical properties/phase etc. Assessing all these phenomena is clearly beyond the scope of this study.

Proposed new text:

We have two main applications in mind. First, we want to understand how suitable UKESM1 is for studying the wider climate effects of changes in the wind distribution. For example, we recently used UKESM1 to study the role of wind changes in modifying the uptake of carbon dioxide by the Southern Ocean (Jarníková et al., 2025); here we aim to provide an evaluation of the robustness of the wind changes reported there. Second, we want to understand to what extent this model can be used to attribute the relative contribution of ozone and greenhouse gases to past and future changes in the wind distribution.

.

R1.7 P5L133: I suggest rather than comparing the Marshall index to the Gong & Wang index, to simply calculate the index in the same way as Marshall, i.e. using the 6 locations. Then you compare "oranges to oranges". Alternatively, you can show that the two derivations yield nearly the same results.

See response to 1.3: we have chosen to use zonal means both to be consistent with how other studies typically calculate the SAM from reanalyses, to better capture the overall circulation patterns, and to avoid sampling effects. We justify our choice in the proposed new text.

Proposed new text:

In the observational SAM index, these MSLP anomalies are calculated from the mean of six station records near each of the two latitudes for which good long-term records exist. In the reanalysis SAM index we simply use the zonal mean at both latitudes as in Gong and Wang (1999). We use the zonal mean rather than subsampling the reanalysis to the six station locations to maintain consistency with standard reanalysis-based SAM calculations (e.g. Morgenstern, 2021) and to avoid sampling biases introduced by point-location extractions from gridded fields. The zonal mean better represents the large-scale circulation patterns that define the SAM, whereas point samples can be influenced by local topographic effects and sub-grid-scale processes that are not fully resolved in reanalysis products.

R1.8 P5L161: Rather than discussing biases here (which is fine), the interesting question is how trends in TCO compare to observations, because trends in TCO are linked to climate change. Here I'm not convinced (following Morgenstern et al. 2020) that UKESM1 is more suitable because the bias over the Antarctic is smaller than elsewhere.

We have added a new section (3.3, see response to R1.1) where we compare UKESM1 to other CMIP6 models with interactive chemistry and make it clear that UKESM1 does overestimate the trend in Antarctic ozone (primarily from 1980 to 1995) and that this overestimate should be borne in mind when considering the near surface wind response. We state that this indicates the dynamical link between ozone loss/recovery and near surface winds in UKESM1 is likely weaker than in reality.

R1.9 P6L169: Surely you are not actually "modifying emissions" but rather surface mixing ratios of ODSs (which are globally uniform and follow a prescribed scenario). I would not use the word "emissions" in this context.

In the new manuscript, we would change "ODS emission scenarios" to "ODS surface mixing ratio scenarios" here and everywhere else it is relevant, for clarity:

We perform simulations for 1950 to 2100 using two ODS surface mixing ratio scenarios: (i) ODS use the standard CMIP6 surface mixing ratios (historical followed by a projection), and (ii) ODS are fixed at 1950 values.

R1.10 P6L175: In 1950 there were about 0.6 ppbv of Cl in the atmosphere due to CH3Cl, plus traces of CFCs that had been invented in the 1930s. So it's not quite correct to say that "no ODSs" reached the stratosphere. Just the large increases since 1950 do not reach the stratosphere in this experiment.

To be more precise in our language, we will change this sentence in the new manuscript to:

"OZONE-1950 minimizes stratospheric ozone loss throughout the simulation as only trace amounts of ODS, emitted or produced in the atmosphere before 1950, are available for ozone destruction in the stratosphere."

R1.11 P6L180: The assumption of linearity (needed to attribute any differences between ozone-hist and ozone-1950 to GHGs) is a little dangerous because of nonlinear couplings between GHGs and ODSs, such as reflected in the roles of NOx and HOx (both affected by GHGs) in interfering with halogen-catalyzed ozone depletion. It would be better to have a simulation GHG-1950 in which the leading GHGs (CO2, CH4, N2O) are held constant and ODSs follow their usual trajectory. Not sure why this does not exist in AerChemMIP, but if you can produce these simulations, that would certainly help allay this concern.

We agree that the assumption of linearity is an oversimplification and have added a sentence as a caveat to the proposed new manuscript:

Following McLandress et al. (2011), we assume that ozone-driven trends (resulting from the different prescribed ODS surface mixing ratios) and GHG-driven trends are additive; that is, the OZONE-HIST run demonstrates a linear addition of ODS-driven trends and GHG-driven trends, so [OZONE-HIST – OZONE-1950] will isolate the ODS-driven trend, while any trends in the OZONE-1950 runs are due to GHG emissions alone. ***We acknowledge the potential for some non-linear interactions that may weaken this linear additive assumption but suggest to a first order this approximation is suitable for attributing the primary differences identified either to ODS or GHG differences in the respective experiments.***

Furthermore, as a back of the envelope calculation that we do not include in the paper, we used the existing experiments to test the impact of the different ozone forcing scenarios under common GHG pathways, or equivalently the impact of different GHG forcing under common ozone scenarios. *By differencing output from different UKESM1 experiments we were able to isolate the non-linear interaction terms between ozone and GHG trends (on the evolution of each other) and could verify that such impacts were very small and generally not statistically significant. This supports the assumption of linear additivity.*

Using the following experiments:
1. ***OZONE-1950-SSP370***
2. ***OZONE-1950-SSP126***
3. ***OZONE-HIST-SSP370***
4. ***OZONE-HIST-SSP126***

Subtracting 1-2 isolates the GHG forcing effect (with ozone held constant across both scenarios). Similarly, differencing 3-4 isolates the same GHG forcing effect as in 1-2, but with ozone evolving historically (and identically) in each experiment). Subtracting the first difference from the second as (3−4) − (1−2) —removes the GHG component entirely, leaving only the influence of the ozone background state on GHG sensitivity, which tells us something about the nonlinearity. (3–1) – (4–2) shows the same thing. We did this experiment for all seasons and several timeperiods and found that trends attributable to this interaction were universally small (typically <|0.02| m/s/decade) and not statistically significant with the exception of the 1950-2100 decadal trend in DJF, which was

statistically significant but very small (-0.005 m/s/decade). Note that for 1950-2014, there is only one GHG scenario (historical) and so any interaction trends are identically 0.

	FY	DJF	JJA	MAM	SON
1950–2100	−0.00167	**−0.00474***	−0.00129	−0.00025	−0.00039
1950–2000	0	0	0	0	0
2000–2050	−0.01046	−0.01785	−0.00912	−0.00689	−0.00796
2050–2100	0.00917	0.01714	0.01325	−0.01724	0.02353

Trends attributable to ozone-GHG interaction. *= statistically significant
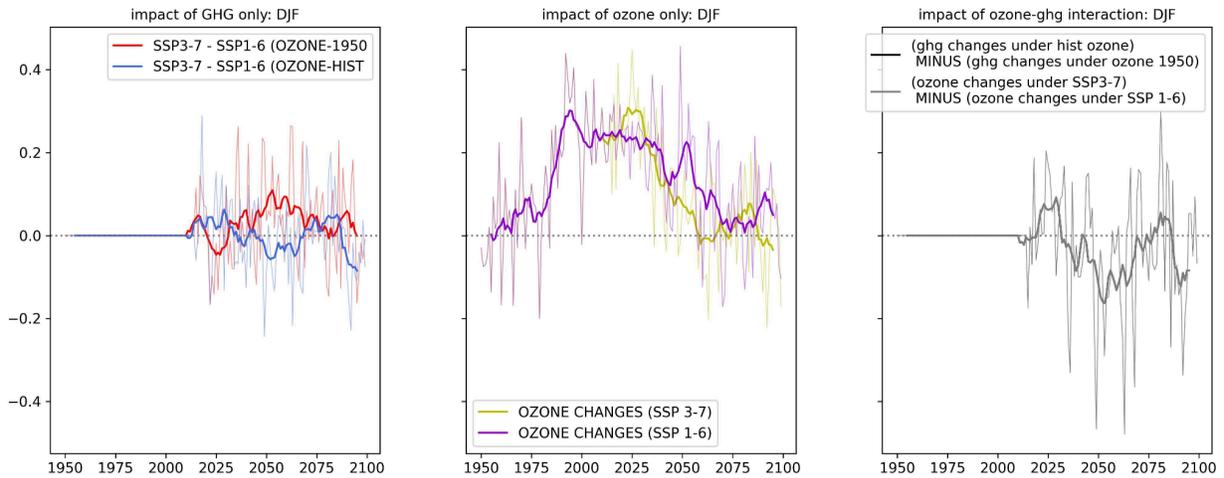
R1.12 P12L373: Reproducing the trend in winds is not sufficient to conclude that the model successfully reproduces ozone depletion. Based on other literature, I think it somewhat exaggerates ozone depletion, but then struggles to fully transport this dynamical driver into the troposphere. I suggest to rephrase this, allowing for this cancellation of errors.

Please see the response to R1.1 and the new proposed section comparing TCO and winds in UKESM1 to other CMIP6 models (included in the response to R1.1). We agree that UKESM1 has a large trend in ozone ~1980-1995 and this does lead to a somewhat stronger response in SAM and winds as well. We have rephrased this section to include a caveat that the UKESM1 ozone trends are strong and that this should be kept in mind when considering the impact of ozone on the model dynamical fields.

R1.13 P13L423: I would replace "poleward jet trend" with "poleward progression of the jet latitude" or similar.

I have replaced "poleward jet trend" with "poleward progression of the jet latitude", and the proposed new sentence would read:

However, the **poleward progression of the jet latitude** is stronger in UKESM1 than in any of the reanalyses,

R1.14 P14L429-430: Winds don't "accelerate" or "slow down", they "increase" or "decrease".

These phrases would now read:

In the first half of the 21st century, summer winds stagnate, as the decrease of wind speeds due to ozone recovery competes with wind speed increase due to GHG emissions. In contrast, wind speeds increase in austral winter and spring, as the GHG effect outcompetes the ozone recovery effect [...]

R1.15 P13L445: A more robust approach would use multiple models (there are more full-chemistry models in the CMIP6 archive) and use an emergent-constraint analysis (making use of historical biases in these models) to produce a multi-model projection of future winds over the Southern Ocean.

We agree this would be an interesting follow up study, though we note there are only 6 models in CMIP6 with interactive chemistry so the degree of multi-model spread/constraint might be limited. Furthermore, the spread in the multi-model representation of the recent-past relationship between TCO trends and wind speed trends (new Figure 6) suggests the model future projections may not easily offer an emergent constraint for this relationship. Our aim in this paper was to use observations and reanalyses to assess the realism of UKESM1 surface wind responses to simulated ozone loss and recovery and then to use this information to back-up our analysis and attribution of future wind changes to simulated ozone changes under different GHG scenarios. We used the observation-based SAM index to constrain wind speed trends in the reanalyses (Fig 5). This analysis also indicated that UKESM1 had an accurate representation of both wind and SAM trends over this period. In particular, this study aims to support earlier work (Jarnikova et al. 2025) where we used the UKESM1 future projections to investigate Southern Ocean carbon uptake and to inform potential future work that might use UKESM simulations to drive offline ocean models. In order to avoid confusion we have removed the word "constrained" at L445 and in the title.

Proposed new sentence:

~~Our work here, which focuses on the near-surface winds, extends this understanding by providing a constrained estimate of the changing surface wind speed trends in response to this shift, based 445 on a model with interactive chemistry whose past performance is validated against both reanalyses and the observational SAM index.~~

The ability of UKESM1 to represent these interactions, and how they shape surface winds over the Southern Ocean, was validated against both reanalyses and an observational SAM index and further contextualized against other CMIP6 models with interactive ozone chemistry.
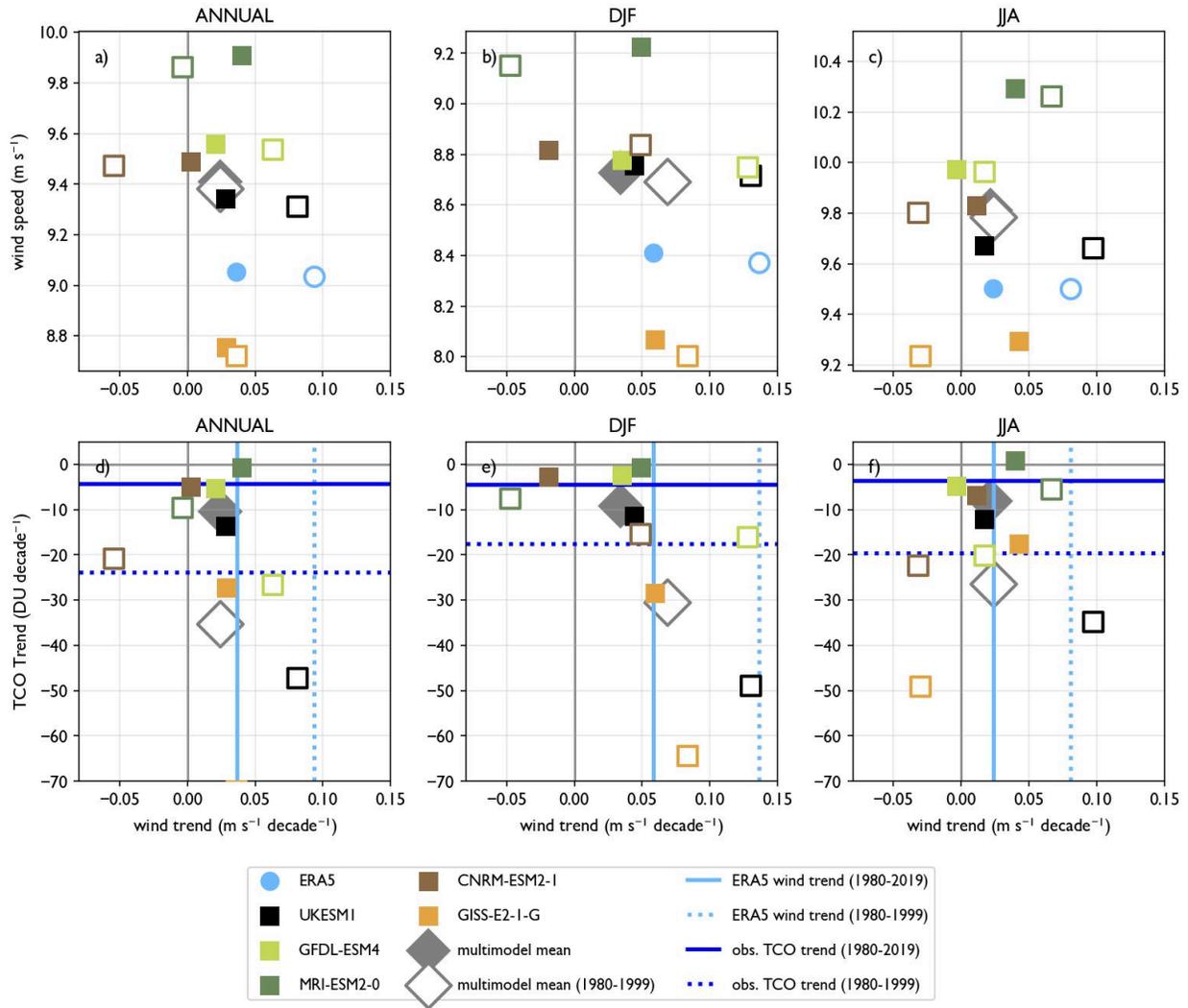
Fig. 6

Fig. 6: 10-m wind speed, decadal wind speed trend, and decadal TCO trend for CMIP models with interactive chemistry. *For each model, one ensemble member is used (see Table 2).*

.

R1.16 P14L448-457: This is a surprising, ocean-centric conclusion to this paper, given that the Southern Ocean plays almost no role in the rest of the paper. Perhaps you can refocus this to reflect more on the actual findings of this paper?

The original motivation for this paper was to support the work in Jarníková et al (2025) where we used the UKESM1 historical and future projection runs to understand the drivers of future Southern ocean carbon uptake. This was why this paragraph initially had an ocean-centric flavour. We agree with the reviewer that this reads somewhat oddly and have removed this paragraph and focus on a summary of the atmospheric findings of this study, with contextualization from past results.

Proposed new ending:

We show that ozone depletion overwhelmingly drove the observed summer wind acceleration in the latter half of the 20th century, and we expect this trend to reverse in the first half of the 21st century. By the second half of the 21st century, greenhouse gas loading is the main factor driving wind changes, with more emissions leading to more acceleration, but it is strongly active only in austral winter and spring. Thus, we demonstrate a shift in the dynamical controls on the wind behaviour over the Southern Ocean from ozone in the latter half of the 20th century to greenhouse gas emissions in the latter half of the 21st century.

This control shift is consistent with the general understanding. As in Barnes et al. (2014), we find historical wind speed trends due to ozone depletion are stronger than greenhouse gas driven trends seen in the twenty first century. Similar to Barnes et al (2014)., Gerber and Son (2014.), McLandress et al. (2011), and Simpkins and Karpechko (2012), we observe a cancellation of effects in the first half of the twenty first century, as ozone recovery competes with greenhouse gas emissions, leading to only weak wind trends in this period. We also reproduce the dependence of future trends on greenhouse gas emission strength seen in Simpkins and Karpechko (2012) and other studies. Our work, focusing on near-surface winds, extends this understanding by providing an estimate of the changing wind trends in response to the shift in dominant forcing. We do this using a coupled Earth system model (UKESM1), with full-atmosphere interactive chemistry, allowing a large set of interactions and feedbacks to be simulated; chemically between a range of GHGs and stratospheric ozone, between both sets of gases and atmospheric dynamics, and between the atmosphere and the underlying ocean and sea ice. The ability of UKESM1 to represent these interactions, and how they shape surface winds over the Southern Ocean, was validated against both reanalyses and an observational SAM index and further contextualized against other CMIP6 models with interactive ozone chemistry.

R1.17 Figure 1: Which period and which level do the plots refer to? This information should be in the caption. Ditto figures 2, 3, and table 2 (the level needs to be indicated).

I have added the specification that these are the 10-m wind speeds in the proposed new figures and tables (it is stated in the methods).

Fig. 1: Climatological wind speed for 1980-2019. For ERA5, the climatological wind speed for the full year and austral summer (DJF) and winter (JJA) is shown. For the other 3 reanalyses and UKESM1, differences from ERA5 are shown as [product x – ERA5]; i.e. positive (red) values indicate higher winds than ERA5. See also Table 2.

Fig. 2: Summary statistics for wind distributions in four reanalysis products and UKESM1. a-b): mean 10-m wind speed trends relative to climatology for a) full year and b) DJF only. Thick lines have been smoothed by a three-point running filter. c-d): windspeed frequency distribution (100 bins, 0-20 m s $^{-1}$) for c) full year and d) DJF, with high tails shown in inset. e-h) Trends in mean and extreme winds for full year and DJF vs. climatological means, with colours the same as in panel a). Filled symbols represent 1980-2019, while open symbols represent 1980-1999; circles represent reanalyses while squares represent UKESM1. Colours from panel a) are repeated throughout the figure. Summary statistics shown in panels e)-h) are also given in Tables 4 and 5. All figure statistics are calculated for daily winds at 1°x1° resolution, 40°S- 60°S.

Fig. 3: Decadal trends in 10-m wind speed (1980-2019). Hatching shows trends significant at the 95% confidence level. See also Table 5.

Table 4: Climatological 10-m wind speed (1980-2019). Seasonally subdivided open-ocean wind speed between 40°S and 60° S for ERA5 , and differences from ERA5 for the other reanalyses and UKESM1.

R1.18 Tables 2 and 3: Here it would be fairly straightforward to complement this with data for selected CMIP6 models that are comparable to UKESM1, such as CESM2-WACCM, EC-Earth-AerChem, CNRM-ESM, or more. Given the large ozone depletion in UKESM1, contrasting it with models with weaker ozone depletion (the weakest would be found in MRI-ESM2), perhaps something useful can be learnt about the role of ozone forcing.

Our new section 3.3 addresses this comment (see response to R1.1). We are grateful to the reviewer for suggesting this and agree it helps put the UKESM1 results into a wider context.

R1.19 Figure 4: Again here the definition of the mean jet position should be included, especially at which level the winds are evaluated.

I have added a clarifying sentence, as well as a reference to the methods, here.

Fig. 4: Summary statistics for the wind jet. *The wind jet is calculated as the location of the maximum of the u-component of the 10-m wind speed between 30°S and 70°S (see Methods). a): the climatological mean annual jet position, with one standard deviation of the annual mean shown for ERA5 and UKESM1. b): The zonal mean of the austral summer jet position. Thick lines indicate smoothing by a 3-point running mean filter. c-d): Trends in the annual (c) and summer (d) jet position mean vs. climatological means. Colours from panel a) are repeated throughout the figure. See also Sup. Tables ST3 and ST4.*