(1) General comments:

The expected stratospheric ozone recovery from the effect of halogenated ozone depleting substances (ODSs) has received much attention in recent years. Yet detecting the recovery of the ozone layer is complex due to a number of factors, including internal and external variability, that obscure the emerging signal associated with the slow decline in ODSs levels. The manuscript addresses this issue by investigating three stages of ozone recovery. To this end, the authors use total column ozone (TCO) changes based on experiments of the UM-UKCA and multiple linear regression (MLR) analysis. Although models are not perfect (e.g. often show significant disagreement compared to observations), they are a valuable mean to explore ozone changes due to specific factors (i.e. ODSs levels).

Overall, the manuscript addresses relevant issues with regard to the evolution of the stratospheric ozone layer and uses appropriate data and methods. The text is technically well written. I have minor specific comments (detailed below), which I hope will help the authors improve the paper. In general, I suggest more detailed description and evaluation, additional comparison with ozone measurements, and further discussion on existing literature. Therefore, the manuscript is recommended for publication after the specific and technical comments are addressed.

(2) Specific comments:

a. In the Introduction section, the authors clearly set out the stratospheric ozone depletion in the last decades associated with man-made emissions of ODSs. Due to international efforts banning the use of these substances, the ozone layer is expected to recover and the study aims to explore different stages. However, significant work has been done on detection and attribution of ozone recovery, hence it would be appropriate (and helpful for the broader audience) to briefly introduce key findings, remaining issues, and link it with the novelty of this work. Moreover, this will help relate and put into context the main findings here later in the manuscript.

b. In the Model configuration and simulations section (page 3, lines 25–26), the authors explain that the simulations used were performed in support of the CCMI activity, and that are described in more detail in Bednarz et al. (2016) and Keeble et al. (2017). Bednarz et al. (2016) described that the simulations included a future climatological solar cycle since 2009 based on the observed cycle 23, which is not consistent with the description given in the manuscript (page 3, lines 16–17). Please clarify.

c. In the Removing natural cycles section, the text describes a MLR analysis to identify the impacts of natural variability on TCO. Since the results of this study heavily rely on the MLR analysis, I think this section requires more detailed description of the statistical method. In particular, the $TO3_i$ and $N_{e,l,t}$ terms need better description (i.e. "… some constant value" and "Any noise…"). Also, an evaluation of the MLR analysis is important – i.e. How good is it? How much of the model raw data is captured by the MLR and how much "noise" is left? –. The manuscript already includes some references on MLR analysis that may help.

d. For the Modelled global column ozone and minimum values section, it may be appropriate a statement about the choice of not including the polar regions in this analysis (Figure 1), since, in other sections and figures these regions are included and also discussed in the last paragraph here (page 5, lines 18–25). In fact, the latter paragraph argues that minimum column ozone values are a poor indicator of ozone recovery by giving examples based on polar regions.

Is there any particular reason for not using the latest version (3.3) of the Bodeker Scientific database? The latest version, in addition to include some improvements on the methodology, could be expanded until 2016 in the inset of Fig. 1. Also it would be nice to include the Bodeker Scientific database in the acknowledgements, as recommended on the website.

e. Regional trends section. This section includes very interesting results. However, modelled results in Fig. 2, both "raw" and residual data, could be compared to observed trends. In turn, this may lead to some evaluation/discussion and to put into context these results with existing literature. Nevertheless, there is some discussion (outlook) on TCO trends between 2000–2017 in the Discussion and Conclusions section (page 8, lines 15–19).

Error bars representing the 95% confidence interval may need a line or two detailing how these are estimated and whether they account for autocorrelation. Are these confidence intervals calculated in the same way for all analyses?

f. Return to historic values section. Figure 4 shows that TCO values in the tropics (<30º) reach the "1980 last recovery" between ~2060s–2070s. However, the main text (page 7, lines 12–14) explains that "…, it is the only region in which total column ozone abundances are not greater than their 1980s values by the end of the simulation,…". Please clarify. Also, is there any particular reason for not showing(using) "ozone residuals" on Fig. 4 as in previous analyses? I understand the study aims to explore ozone recovery addressing natural cycles.

(3) Technical comments:

Page 1, lines 9–10. "This approach…". The approach or method has not really been introduced. I suggest rephrasing this sentence (e.g. Here internal atmospheric variability… is accounted for by…).

Page 1, line 28. Substitute "ODSs" for "ODS" for consistency throughout the text.

Page 1, line 34. Randel and Wu (1995) did not explore the effects of Mt Pinatubo eruption on stratospheric ozone.

Page 1, lines 37–38. References order.

Page 1, line 38. Delete "," between "other" and "non-chlorinated".

Page 2, line 2. References order.

Page 2, lines 13–15. "Good agreement…" This sentence is a bit confusing, rephrasing maybe?

Page 2, lines 22–23. "…, data from fully coupled chemistry-climate model…" is a bit misleading since you use imposed SSTs. I would clarify "fully coupled" (chemistry and radiation schemes?).

Page 2, line 26. Spell out "SSTs".

Page 2, line 37. Substitute "-" from "–".

Page3, line 23. Could use just "SSTs", as it was introduced before.

Page 5, line 33. Substitute "… DU year^{-1}…" for "… TCO (DU year^{-1})…"? I am aware that "for the column ozone" is mentioned later in the sentence, though it is somehow confusing.

Page 7, lines 9–10. "… 1980s values for the first time (light red)…" should be "blue".

Page 7, line 21. Typo: "airmasses".

Page 7, line 26. I would substitute "expected" for "projected" (e.g. acknowledging these are modelled results, which are model and scenario dependent).

Page 8, line 15. Typo: "… Unlike a recent analyses…"

Page 8, line 26. Typo: "… The tropics have too small a trend…"

Page 15, line 2; and Figure 2, legend. Please follow same consistency in the naming, both for the figure and the main text.