Response to Referee #2

This is an interesting and very well written study on solar 11-year signatures in different atmospheric parameters (in the troposphere, stratosphere and lower thermosphere) based on model simulations with the UM-UKCA model. The paper does not present any spectacular new results on atmospheric effects of solar variability at the 11-year scale, but it is an interesting contribution to the field and should eventually be published in my opinion. An important aspect of the study is the fact that two different analysis techniques (i.e., a composite analysis and multi-linear regression) are applied and the differences in the results are studied and discussed.

We thank the reviewer for their positive review and the constructive and helpful comments for improving the manuscript. We reply to their comments below in blue.

I ask the authors to consider the following comments:

Page 2, line 15: "The spectral distribution of solar irradiance is commonly referred to as the spectral solar irradiance (SSI)." I disagree, this is not the correct meaning of solar spectral irradiance. SSI has the units W / m2 / nm, i.e. spectral irradiance. It is the power of electromagnetic radiation per unit area and per spectral interval. SSI at a certain wavelength can also be determined or calculated without considering the spectral distribution of the entire spectrum.

This sentence has been removed and replaced by: "The variation in solar spectral irradiance (SSI) as a function of wavelength is important for determining the atmospheric response to the solar cycle."

Page 5, line 29: "In the Fast-JX photolysis scheme, the change in partitioning of solar irradiance . . ." I find the phase "change in partitioning" a little misleading, because it's not only the partitioning that's changing. The overall TSI changes as well.

We have carefully chosen the phrase "change in partitioning of solar irradiance into wavelength bins" in light of the fact that the Fast-JX bins cover wavelengths from 177-850 nm and, thus, do not encompass the full solar spectrum. We therefore defer from referring to TSI changes in Fast-JX as this excludes a part of the full spectrum.

Page 5, last line: "At pressures less than 0.2 hPa, i.e. where photolysis rates are calculated using the look up tables, the 11-year solar cycle variability is reflected in the TSI change only, with no modulation of SSI." I don't really understand this statement. If TSI is changed, then SSI (in a given spectral interval) must change as well. You probably mean that the spectral distribution of the solar irradiance spectrum is not changed, right? See also my comment on the meaning of SSI above.

The reviewer is correct. We have clarified the text accordingly.

Page 6, line 22: ".. but a sparse horizontal sampling (Soukharev and Hood, 2006; Hood et al., 2015; Tummon et al., 2015)." I think it's more appropriate here to cite one of the original instrument or algorithm papers, rather than papers that "only" use the data. Sparse geographical coverage was always known to be a disadvantage of solar occultation observations.

We have now added references to Damedeo et al. (2013, 2014).

Page 7, equation 5 (and the equation in the supplement): The choice of the offset and trend terms does not make sense to me. The offset is just a number, right? Why does it have to be represented by a product of two numbers. This is not necessary and only makes things more complicated. I doubt that the function is implemented in this way in your fitting routine – this would not lead to stable results. Also, the trend term trend(t) is simply "t", right? If yes, then it should be written that way.

The MLR code does indeed represent the offset and trend terms as products of two numbers, and this approach has been previously used in the literature (e.g. Chapter 8 in SPARC CCMVal, 2010; Kunze et al., 2016).

As discussed in Sect. 2.4 the calculation of the yearly mean response by our MLR model is carried out using monthly-mean input data, and the seasonal cycle is accounted for by expanding the regression coefficients (i.e. the 'b' terms) into pairs of sine and cosine functions.

Thus, the term "b(offset)·offset" accounts for the 12-month climatology (i.e. 12 different values depending on a month). Similarly, the "b(trend)·trend(t)" is the trend term modified the annual cycle.

The trend(t) term is indeed just "t" but we would rather stick to the former name for consistency with the other terms in the equation.

Page 7, line 16: "(here applied 5 times)" Is there a specific reason, why this filter was applied 5 times?

We have decided to apply the filter 5 times in order to smooth the ENSO timeseries. This choice was made arbitrarily and we believe it would not bear large impact on the diagnosed solar response.

Page 8, line 14: "However, unlike the yearly mean TSI timseries that forces the model, the timeseries chosen here is that originally recommended for the CMIP5 models" How does this choice affect the results? Ideally, the same solar proxy time series should be used. Please add a brief (qualitative) comment on the expected impact (probably very small).

This choice is made for consistency since the model is forced with annual mean TSI while the monthly mean observation/reanalysis data will be affected by the monthly variations in solar irradiance. However, given that the amplitude of the 11 year solar cycle is larger than typical month-to-month fluctuations, this signal dominates the analysis and hence the choice of slightly different proxies is unlikely to affect the results.

Same line: "timseries" -> "timeseries"

Thank you for spotting this typo. Corrected.

Section 3: It would be good to show a sample result of the MLR analysis (fit and residual). I have no reason to doubt that the method works well, but it's always good to see a fit example.

We have added an example of a fit and a residual to the Supplement (Fig. S2), and we refer to it the manuscript.

Page 10, line 17: "According to the postulated . . ." I think this sentence is incomplete.

This has been reworded to: "According to the mechanism postulated by Kodera and Kuroda (2002),…"

Page 11, section 3.2.3: This section focuses more on the (few) similarities between ERAI and the model simulations. However, looking at Figs. 3 and 4 the obvious aspects are the significant differences for both T and the zonal wind response. They should be mentioned/discussed as well.

As given by the heading to Sect. 3.2.3., this section is about the response in the mid-latitude troposphere. We discuss the broad similarity in the NH, and we also note the differences found in the SH. We do not think there are many other aspects regarding the solar response in the mid-latitude troposphere that needs to be discussed in that section.

Page 12, line 14: I suggest replacing "The lower altitude of the ozone response" by "The lower altitude of the maximum ozone response"

Corrected as suggested.

Page 17, line 10: "we find that the total column ozone responses derived in various regions are somewhat higher for MLR than for composites," Any ideas on the causes of this behaviour?

We have not investigated this feature in detail. However, it is plausible that the composites are more strongly affected by random dynamical variability, which is particularly important for the determining the total ozone response to solar forcing (Hood, 1997), whereas the MLR explicitly treats the noise as a separate term.

Page 17, line 28: "observational records such as ERAI" Can one really call ERAI an observational record? It's certainly different from the "pure" observational records such as the SAGE II O3 data set.

We agree and have changed this to "observational/reanalysis records"

Page 21, line 18: "Some differences (although not statistically significant) are found in the troposphere and in the tropical lower stratosphere." Did the paper really show that the differences are not statistically significant? Some signatures are statistically significant in one analysis, but not in the other. What does this imply in terms of the statistical significance of the differences?

We agree that we have not explicitly discussed the issue of statistical significance of the composite-MLR differences. However, in Fig. 2b,c (ozone and temperature), the confidence intervals associated with the individual MLR and composite responses overlap throughout the tropical troposphere and stratosphere, thereby illustrating the differences between the MLR and composite responses in this region are not statistically different. We have further tested the differences between the composite and MLR zonal wind and temperature responses in Fig. 3a-b and 4a-b by looking for regions where the confidence intervals (i.e. ±2 standard errors) around the individual composite and MLR responses do not overlap (and, hence, where the composite-MLR differences are statistically significant), not shown. The results show that the differences are indeed mostly not statistically significant.

Given the reviewer's concerns, we have changed the sentence in question to 'Some apparent differences (although mostly not highly statistically significant)…'

Figure 2, caption and title of panel a): "heating rates response" -> "heating rate response"

Corrected.

Page 43, Table 1, lines 3 and 4: Both lines list the same spectral interval (320 – 690 nm). Is this intended? If yes, the exact meaning of these two lines (and their difference) is not clear to me.

This is intended and relates to the design of the model shortwave radiation scheme. It includes one band to account for absorption by ozone and a separate band to treat the overlapping absorption between ozone and water. More details can be found in Zhong et al. (2008), Cusack et al. (1999) and Edwards and Slingo (1996).

References:

See the references list in the manuscript, as well as:

Cusack, S., Edwards, J. M., and Crowther, L. M.: Investigating k distribution methods for parametrizing gaseous absorption in the Hadley Centre Climate Model, J. Geophys. Res., 104(D2), 2051-2057, 1999.

Hood, L. L.: The solar cycle variation of total ozone: Dynamical forcing in the lower stratosphere, Journal of Geophysical Research-Atmospheres, 102, 1355-1370, 10.1029/96jd00210, 1997.