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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Solar cycle in current reanalyses: (non)linear attribution study" *by* A. Kuchar et al.

Anonymous Referee #3

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Overall, this is a useful study that contrasts with other recent efforts to detect and compare solar-induced variations in reanalysis meteorological datasets (e.g., Mitchell et al., Signatures of naturally induced variability in the atmosphere using multiple reanalysis datasets, QJRMS, in press, 2015). It differs from other similar works by examining in more detail the implied dynamical structure of the solar-induced response during the winter season in each hemisphere using EP-flux diagnostics in addition to zonal wind and temperature. In addition, it attempts to investigate solar-related variations of assimilated ozone, which has not been done previously to my knowledge.

On the negative side, the manuscript does an inadequate job of cautioning readers about the uncertainties associated with reanalysis datasets and the accuracy (or lack thereof) of the derived solar-induced variations. It can be argued that the reported

analysis is actually too detailed, given the limited quality and short duration of these assimilated datasets. An effort should be made to modify the ERA-Interim temperature data to correct for discontinuities occurring in the upper stratosphere at times of major changes in the input satellite radiance data. At a minimum, some assessment should be made of the likely errors resulting from such discontinuities. Also, at least some of the derived anomalies may not be entirely solar in origin, but could instead be caused by internal climate variability or the aliasing influence of volcanic eruptions during the short 35-year record.

(1) It is found that the 11-year ozone response in the tropical upper stratosphere differs greatly between the MERRA and ERA-Interim datasets (Figures 1m and 2m) and that neither response resembles that derived from observations, i.e., there is no double-peaked response. The annual mean upper stratospheric ozone response is decidedly negative for ERA and is slightly negative for MERRA, which is inconsistent with the effects of 11-year solar UV forcing. For ERA, the negative response is most pronounced (up to 5%) at polar latitudes. In contrast, analyses of merged SBUV ozone data yield a positive response in the upper stratosphere with more pronounced positive maxima at polar latitudes. Analysis of the seasonal dependence of the polar maxima show that they occur mainly in the summer season in each hemisphere (see, e.g., Figure 1 of Tourpali et al., JGR, v. 112, D12306, doi:10.1029/2006JD007760, 2007). Obviously, therefore, there is an issue with the assimilation of ozone in the reanalysis datasets. In the text (p. 30891), the pronounced negative polar ozone response is interpreted as "connected with a higher destruction of ozone during the solar maximum period and consequent heating of the region." This is possible since increases in temperature lead to increased ozone losses because of the temperature dependence of the reaction rates that control the ozone balance. Would this interpretation require that the assimilation model had interactive ozone chemistry? Please expand the discussion of this interpretation. In the case of MERRA, SBUV ozone profiles are assimilated while, in the case of ERA, no solar cycle variation of ozone is passed to the forecast model. There is also no solar cycle in irradiances passed to the radiative part of the

forecast model for any of the three reanalysis datasets considered here. So, no direct solar-induced increase in ozone production would be expected even if the assimilation model has interactive chemistry. The SBUV ozone profiles have very low vertical resolution and may yield an 11-year ozone response that is biased toward higher altitudes compared to SAGE observations, which have much better (1 km) vertical resolution. So, assimilation of the SBUV profiles in the MERRA system may not have produced a realistic 11-year ozone variation in that reanalysis dataset. How does the lack of a realistic upper stratospheric ozone variation affect the value of the reanalysis datasets for investigating 11-year dynamical responses? This should also be discussed somewhere in the manuscript.

(2) The derived upper stratospheric temperature response in all three reanalysis datasets (Figures 1a, 2a, and 3a) is less than accurate due to the existence of large offset errors occurring at times when the input satellite radiance data and/or the assimilation scheme changed (McLandress et al., 2014). This problem is briefly noted on p. 30884, line 9, but it is not considered to be a major issue. Also, no attempt is made to correct or adjust the reanalysis temperature data prior to the analysis. Such retrospective adjustments are probably next-to-impossible for MERRA and JRA but could have been attempted for ERA using the procedures developed by McLandress et al. However, the McLandress et al. study only considered discontinuities occurring in 1985 and 1998. As noted by them, another discontinuity occurred during 1979 that would also need an adjustment if the time series begins in that year. But, at a minimum, the offsets in 1985 and 1998 could have been corrected. In the revised paper, please (a) apply the necessary adjustments and repeat the analysis for the ERA data; and (b) add statements to the discussion and conclusion sections pointing out the likely errors in the temperature results resulting from these unphysical temperature discontinuities.

(3) There is no mention in the manuscript of the possibility that the calculated linear solar regression coefficients are affected by aliasing from the effects of strong volcanic aerosol injection events (El Chichon and Pinatubo) occurring following the cycle

21 and 22 maxima, respectively. The record is short (35 years) and these two fortuitously placed injection events are unique to this time period. They could have produced decadal-scale variations in the stratosphere that would not be entirely orthogonal to the solar forcing variable (the 10.7 cm radio flux). So, there could be some mixing of the volcanic and the solar regression coefficients. The most well-known possibility is that part or all of the 11-year lower stratospheric response of ozone and temperature derived from observations is a consequence of such aliasing (Solomon et al., JGR, 1996; Lee and Smith, JGR, 2003). Austin et al. (2008) concluded that this was not likely to be true for the chemistry climate models considered by them because the solar regression coefficients over the 1960-2005 period did not change much if an aerosol term was included or not in the regression model. However, Chiodo et al. (ACP, v. 14, p. 5251, 2014) have recently tested in more detail one such chemistry-climate model (WACCM 3.5) by carrying out simulations with and without volcanic aerosol forcing. They find that, at least for this specific model, the apparent solar-induced ozone and temperature responses in the lower stratosphere largely disappear in the simulation with no volcanic aerosol forcing. Thus, at least for WACCM 3.5, the solar-induced lower stratospheric response appears to be due almost entirely to the aliasing effects of the two eruptions. On the other hand, it is known that some CCMs overestimate ozone losses during high aerosol loading periods, causing a larger aliasing effect on the solar response than would occur when analyzing observations (Dhomse et al., ACP, 2011). At least some coupled climate models (e.g., MIROC-ESM-CHEM; Watanabe et al., Geoscientific Model Development, v. 4, p. 845, 2011) produce solar-induced lower stratospheric responses that are not strongly affected by aliasing from the El Chichon and Pinatubo eruptions. So, the answer to the question of whether or not the observationally estimated lower stratospheric response is strongly affected by volcanic aerosol aliasing unfortunately appears to depend on the model that is employed to simulate the climate system. Even the upper stratospheric solar response could be affected by such aliasing since the dynamical evolution of the entire stratosphere in winter was affected by these major eruptions. Further work is needed to resolve this issue. In the mean-

time, one should be careful to note the possibility that the lower stratospheric solar response derived from observational datasets could be affected by such aliasing. This should be done at appropriate places in the paper with appropriate added references.

(4) In the monthly analyses shown in Figures 4 and 5, by far the largest apparent solar response occurs in February at high northern latitudes in the form of a lower stratospheric warming, a mesospheric cooling, and an associated weakening of the zonal wind (polar vortex). This apparent response has been found in previous analyses of the ERA data (e.g., Frame and Gray, 2010). It is possible that this response is indeed solar-induced. For example, Gray et al. (J. Atmos. Sci., v. 61, p. 2777, 2004) suggests that the negative zonal wind response in late northern winter may be caused by an increased likelihood of major stratospheric warmings later in the winter under solar maximum conditions when the polar vortex in early winter is stronger, on average, and therefore less susceptible to disruption. In this manuscript (p. 30894), the February negative zonal wind response is regarded as real on statistical grounds alone: "In February, the intensive stratospheric warming and mesospheric cooling is associated with a more pronounced transition from winter to summer circulation attributed to the solar cycle (in relative impact methodology up to 30%)". However, one problem with this conclusion is that general circulation models have not yet successfully simulated the strong final warming in February under solar maximum conditions (e.g., Schmidt et al., JGR, v. 115, doi:10.1029/2009JD012542, 2010). Also, there is no similar observed response in late winter in the southern hemisphere. Given the short (35-year) record, it is possible that this response is not really solar but is instead a consequence of internal climate variability or aliasing from effects of the two major volcanic eruptions. Please revise the discussion to note this possibility.

(5) The Introduction does not really explain what will be done in this manuscript and why it is necessary. It consists of a general and rather lengthy review of the topical area of solar cycle forcing of the stratosphere, including observational and model results. This review includes some material that could be left out and is not written in a way that

explains what the outstanding questions / issues are. It never says what the objectives of the present work are and why they need to be addressed. Why is it necessary to consider non-linear methods in addition to linear multiple regression? Why is it necessary to investigate whether solar responses derived from assimilated reanalysis datasets are consistent with previous analyses of observations alone (e.g., whether a double-peaked response can be extracted from the reanalyses)? What will be done in this manuscript that is different from previous work? Please revise.

(6) Abstract, lines 17-20: "Furthermore, the seasonal dependence of the solar response was also discussed mainly as a source of dynamical causalities in the wave propagation characteristics in the zonal wind and the induced meridional circulation in the winter hemispheres." This sentence is not clear. Please re-write or leave out. Also, in the next sentence, please insert "at solar maximum" after "Brewer-Dobson circulation".

(7) P. 30881, lines 10-12. "Gray et al. (2009) have shown, with the fixed dynamical heating model, that the response of temperature in the photochemically controlled region of the upper stratosphere is approximately given 60% by direct solar heating and 40% due to indirect effect by the ozone changes." This statement is a simplification of what is shown in Figure 2 of Gray et al. (2009). In fact, the contribution from the indirect effect of the ozone changes varies from nearly zero in the equatorial middle stratosphere to 60% near the equatorial stratopause. It is a strong function of position, depending on what the solar-induced ozone change is, which can vary strongly with season.

(8) P. 30881, lines 20-22. This sentence refers to the confirmation of the double-peaked vertical structure in the simulations analyzed by Austin et al. (2008). Please revise based on Comment 3 above.

(9) P. 30882-83, lines 27-. This brief summary mentions the work of Ineson et al. 2009 and Harder et al. 2009. However, a more recent detailed review of solar spectral

irradiance variability has been given by Ermolli et al. (Atmos. Chem. Phys., v. 13, p. 3945, 2013). They discuss, for example, that the Harder et al. measurements from the SORCE satellite may have been affected by instrument degradation with time and so may be too large in the UV. They conclude that the SORCE measurements, which are currently being re-calibrated by the SORCE team, probably represent a liberal upper limit on the true SSI variation while proxy-based SSI models such as the NRL model represent a lower limit. Please revise to bring this up to date.

(10) P. 30883, lines 10-13. These two sentences should be combined into one.

(11) P. 30884, line 20. Please change to: ... were analyzed on a daily ... P. 30885, first line: Please insert “For example,” before “the Brewer-Dobson ...”. P. 30885, line 6: Please change “Except for” to “In addition to”.

(12) P. 30885, lines 6-14. This whole paragraph seems out of place in a section on Datasets. Please move it to either the Introduction or to section 4.2.

(13) P. 30885, Eq. 1. This seems to be a standard regression model except for the NAO term. Is the NAO really independent of the other terms? Or, does it depend partly on ENSO and on the solar cycle? If the latter, then this may introduce errors in the results since there will be mixing of coefficients. Please either remove this term from the model or discuss the issue of independence and whether an NAO term is needed.

(14) P. 30886. Use of the 10.7 cm flux is acceptable for the solar proxy. However, the results are presented as solar max minus min values in the figures. What is the corresponding difference in the 10.7 cm flux? Is it 100 flux units? Please state this or, otherwise, it is not possible to convert the coefficients in the figures to actual numbers per change in the solar flux. It should also be stated in this paragraph that the 10.7 cm flux is a proxy for solar ultraviolet variations at wavelengths (200-300 nm) that are important for ozone production and radiative heating in the stratosphere.

(15) P. 30887, line 13. Please define NWS, either here or in the reference list. Line 25:

Perception.

(16) P. 30888, line 2. feedforward should be feed-forward and backpropagation should be back-propagation.

(17) P. 30889, first line. change to: ... from purely practical ...

(18) P. 30889. Here, the figures are discussed for the first time. Looking at the figures, the small size makes them difficult to read. Also, the hatching to indicate statistical significance makes it difficult to determine exactly what the underlying color is. I am not sure what to do about this but the authors should consider a different presentation method. Would it be possible to enlarge by a factor of 2-3 the regression coefficient plots while leaving the RI plots (which are less illuminating) at a small size?

(19) P. 30891, lines 7-9. “The largest discrepancies can be seen in the upper stratosphere and especially in the temperature field ...”. It should be noted here that this could be at least partly because the discontinuities in the reanalysis temperature data are most pronounced in the upper stratosphere. It will be interesting to see how the ERA-Interim results change after the discontinuities are minimized using the McLandress procedure.

(20) P. 30895, lines 3-7. This sentence should be divided into two sentences. The second sentence should begin with: While, in the MERRA ...

(21) P. 30895-30898 - Dynamical effects discussion. Overall, this is a valuable and detailed description of the dynamical processes that are implied by the monthly linear regression results. In particular, as stated at the bottom of p. 30895, the coupled solar-induced anomalies of ozone, temperature, geopotential, and E-P flux divergence support the hypothesis of a weaker BDC near solar maxima, consistent with the previous interpretations of Kodera and Kuroda (2002) and Matthes et al. (2006). However, it is also stated on p. 30895, lines 7-9, that an effort is made in this section to “deduce the possible processes leading to the observed” solar-induced anomalies. I am not sure

that this section really achieves this goal. It is more a description of what is happening dynamically rather than why.

(22) At the end of section 5 (p. 30898), it is noted that the weakening of the BDC is apparently not as well established in the SH winter as in the NH winter. It is then stated that this could help explain why the temperature response in the equatorial lower stratosphere is larger in August during SH winter (about 1 C) than it is in December for NH winter (about 0.5 C). First of all, although the lower stratospheric temperature response in SH winter (Figure 5d) does appear to be larger than during NH winter (Figure 4d), it is quite impossible to read the amplitudes of the response from these figures (see comment 18 above). More importantly, if the slowing of the BDC is less in the SH winter, then why is the lower stratospheric temperature anomaly larger at that time?? Again, the discussion in this section is useful for describing what is happening but does not really address the why question. To address the why question, diagnostic analyses of model data are probably required.

(23) Conclusions section (p. 30898). Please add a caution to the reader who may otherwise think that the reanalysis datasets are free of errors and that it is straightforward to evaluate the 11-year solar response using these datasets. In particular, please note again the existence of large discontinuities in the temperature record occurring in 1979, 1985, and 1998 that will translate into errors in the derived solar coefficients.

(24) P. 30899, lines 8-9. Again, the statement that the Austin et al. (2008) results confirmed the double-peaked structure is a bit of an exaggeration. Please revise (see comment 3).

(25) P. 30900, some English corrections: Line 2: ... which investigated the solar ...; Line 15: ... show an ability to simulate the ...; Line 19: ... on northern winter conditions; nevertheless, southern winter ...

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