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Atmospheric
Chemistry
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Discussions

Interactive comment on “The effect of the solar rotational irradiance variation on the middle and upper atmosphere calculated by a three-dimensionalchemistry-climate model” *by* A. N. Gruzdev et al.

Anonymous Referee #3

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This paper reports statistical analyses of the output of a 3D chemistry-climate model to estimate the model response to short-term (solar rotational) UV variations. The simulated solar forcing is assumed to occur at a constant period of 27 days and with a temporally constant amplitude. In addition to calculating time-averaged correlation and linear regression coefficients for model ozone and temperature vs. the 205 nm solar flux, the authors also calculate mean spectral density and squared coherence. Finally, sensitivities (ozone or temperature change for a 1% change in the 205 nm solar flux) are calculated from daily and zonal averages of the model data using both linear regression and spectral techniques. The estimated sensitivities and phase lags are then compared to previously published observational estimates based on filtered satellite observations obtained under different conditions (nadir-viewing, limb-scanning, etc.) and at a variety of local times (Figures 12 and 13).

A major theme of the paper is that high-resolution spectral and cross-spectral methods provide a means of identifying 27-day variations in the atmosphere that are actually caused by solar forcing whereas linear regression and simple spectral analysis methods do not. They argue that a real relation between 27-day solar forcing and a 27-day atmospheric signal can be considered as highly likely if (page 7): “(1) the spectral coherence between this response and the 27-day forcing is high (squared coherence between 0.7 and 1); and (2) the frequency dependence of the phase spectrum is smooth in the close neighborhood of the 27-day period.”

Overall, this is a good step toward an eventual objective comparison of previous observational estimates of the effect of short-term solar UV variations on ozone, other species, and temperature with that calculated in a state-of-the-art 3D chemistry-climate model. However, there are a number of problems that require a substantial revision prior to publication. Most importantly, the current Figures 12 and 13 do not provide a clear and fair comparison of the model results to previously published observations for reasons given below. Second, the authors’ argument that a true identification of atmospheric variations that are highly likely to be caused by solar forcing is possible only by using high-resolution spectral and cross-spectral methods is not really true. If done carefully and thoroughly, analyses in the time domain can also provide such a probable identification with the added advantage of reduced complexity and easier interpretation. Since almost all observational estimates of the 27-day atmospheric response have been done using analyses in the time domain, a one-to-one comparison of these estimates to the model results is best accomplished by applying identical (time domain) techniques to the model output.

Main Comments:

(1) Besides being too busy, Figures 12 and 13 do not provide an objective or clear comparison of the model-derived sensitivities and phase lags to previously published observational estimates. First, the model-derived values are estimated from daily and zonal averages of model output with a mean vertical resolution of $<$ 4 km while the satellite measurements are normally available only for certain local times with vertical resolution of 5-8 km in the case of SBUV (Bhartia et al., JGR, 1996). Because of the short ozone lifetime in the upper stratosphere and lower mesosphere, a significant ozone diurnal cycle occurs which must be taken into account when making comparisons of the type done here. Moreover, ideally, the same vertical weighting functions used in obtaining the satellite ozone and temperature measurements should be applied to the model data if a one-to-one comparison is desired. Second, the same analytic techniques applied to the satellite measurements should be applied to the model data before making a direct comparison such as that in Figures 12 and 13. Since virtually all satellite data measurements were carried out using correlative and linear regression methods, these same methods should be used to estimate model sensitivities for a fair comparison. Even the filtering methods should be replicated as closely as possible to avoid any associated biases. For example, many analyses were performed on deviations from 35-day running averages rather than on data that had been band-pass filtered to remove all periods longer than 35 days. The original data were also usually processed using a 5-day or 7-day running average and linear interpolation procedure (made necessary by the existence of gaps as large as 5-7 days in the available time series). While imperfect relative to the Kaiser-Bessel band-pass filters employed by the authors, this simplified procedure should be replicated in the model data analysis to avoid any possible biases. The only alternative would be to repeat the observational analyses using the methods applied to the model data. The authors may wish to do this but it would require a major increase in the scope of the paper.

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Here are some suggested changes to Figures 12 and 13: First, choose a selected set of observations obtained at a single solar-zenith angle for ozone at a given altitude and the same for temperature. For ozone in the stratosphere, I would suggest choosing the Nimbus 7 SBUV measurements near local noon obtained near the 1980 solar maximum, which was characterized by especially strong and continuous 27-day UV variations (e.g., Hood, 1986; Keating et al., 1987; Hood and Cantrell, 1988). These measurements agree very well with one another and are verified by repeated analyses of separate time intervals and time-progressive cross correlation analyses (see below). Other measurements using, e.g., UARS MLS data are also valid but were obtained at different solar-zenith angles over a shorter time period and during a solar activity period that was less favorable for estimating the 27-day response. Adding them to the figure probably only complicates the comparison. They should definitely be referenced and discussed verbally in the text but it is not necessary to include them in Figures 12 and 13. For ozone in the mesosphere, I would suggest choosing the SME IR limb scanning measurements of Keating et al. (1987) and Hood et al. (1991). For temperature in both the stratosphere and mesosphere, I would suggest using the Nimbus 7 SAMS measurements of Hood (1986), Keating et al. (1987), Hood and Cantrell (1988), and Hood et al. (1991). These also agree well with one another and are verified by analyses of separate time intervals and time-progressive cross correlation analysis (see below). Second, re-calculate the model sensitivities using model data with appropriate vertical weighting and selected at appropriate local times using analytic techniques (i.e., correlation and linear regression of filtered time series) as close to those applied by the above authors as possible. Since the observational sensitivities are annually averaged and were obtained for normal solar forcing, only model sensitivities for the full year period and for normal forcing should be shown in the figures. If the authors wish to compare their model sensitivities for different seasons and for enhanced forcing and no forcing and using spectral analysis vs. linear regression, this should be done in a separate figure with no observations shown. In the case of the existing Figure 12, the seasonal sensitivities derived from spectral analyses are shown but the

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full year sensitivities are not shown. It is therefore not possible to compare directly the regression-derived sensitivities and the spectral-derived sensitivities. Please show full year sensitivities in any new figure intended to compare the various methods for calculating sensitivities.

(2) Contrary to the authors' arguments, identification of a given atmospheric signal as highly likely to be solar in origin can also be done (albeit less elegantly) using linear regression and cross correlation methods in the time domain. Specifically, the basic requirement is that the atmospheric response signal should have a phase lag relative to the solar forcing signal that tends to be constant regardless of the time interval that is analyzed. If the phase varies randomly, then the signal is likely not solar related. However, if the phase is found to be the same or nearly the same in different time intervals over a sufficiently long analysis period, then the signal is almost certainly caused mainly by external forcing rather than internal variability. One way to test this in the time domain for a given atmospheric time series and a given external forcing time series is by calculating a time-progressive cross-correlation function. This is the correlation coefficient calculated over a range of positive and negative lags within a sliding window that is moved progressively through the time series (see, e.g., Figures 7 and 9 of Hood, 1987). The time-progressive cross-correlation function provides much of the same information in the time domain that a calculation of spectral coherence provides in the frequency domain (although without determining the dependence on frequency). Another simple but effective empirical test is to divide the time series into two equal parts and calculate the time-averaged cross-correlation function for each half to verify that the preferred phase lags are similar in each half (see, e.g., Hood and Cantrell, 1988, for an application to two separate 22-month time intervals). In the revised paper, the authors should emphasize that this alternative approach does exist and that spectral analysis in the frequency domain is not the only way to identify a given atmospheric signal as likely to be solar in origin. The conclusions relating to this aspect also need to be revised.

(3) Solar rotational forcing is not constant with time and does not occur at a single (27-day) period. It occurs over a range of periods centered on (roughly) 27 days with a strongly temporally varying amplitude. There is also significant physical forcing at periods near 13 days. The model simulations reported here, at a constant 27-day period and with constant amplitude, are therefore not as realistic as would be desired. It is difficult to judge what effect this type of simulation would have on comparisons between the model sensitivities and those derived from observations. At a minimum, this difference should be noted when discussing the comparison of observations and model in Figures 12 and 13.

(4) In principle, if done thoroughly, an analysis in the time domain should be nearly equivalent to an analysis in the frequency domain. As the authors point out, one exception to this is that spectral analysis can isolate variations at a single frequency (or period) while correlation and linear regression analysis necessarily mixes variations occurring over a range of frequencies (and periods), depending on the filter that is applied. However, as discussed in comment (3), the actual solar forcing occurs over a range of frequencies so a time domain analysis may be an even better approach as long as the tests mentioned in comment (2) are applied.

Other Comments:

(5) The fact that the model response sensitivities tend to decrease with increasing amplitude of the forcing is puzzling. In observations, this does not appear to be the case: Time intervals when the solar UV forcing is strong at a nearly constant 27-day period produce corresponding ozone variations that are higher in amplitude (see, e.g., Figure 3 of Hood, 1986). Perhaps the explanation relates to the fact that the model simulations were done for 27-day forcing at constant amplitude over the entire time record. Some discussion of this is warranted in the Conclusions section.

(6) P. 4, first full para.: Here the aims of the present study are given. The stated

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aims are (1) to study the time and latitude dependence of the atmospheric response; and (2) to assess the respective merits of different analysis methods that are used to identify 27-day solar signals. However, another important aim (in this reviewer's opinion) should be to assess whether the HAMMONIA model produces ozone and temperature responses to solar rotational UV forcing that agree with observations. This can be done if the procedures outlined in comment (1) above are followed.

(7) P. 5, middle of page. Actual measurements of the solar flux near 205 nm (or a close proxy, the Mg II index) are readily available. Why not use these measurements as input to the model rather than assuming an artificial 27-day sinusoidal time variation with constant amplitude? This would make the simulations more realistic and the final comparisons of model sensitivities to observed sensitivities much more meaningful.

(8) P. 6, second para. As mentioned in comment (1), if direct comparisons with observed sensitivities and phase lags are desired, it may be preferable to use the same filtering techniques for the model data that were used for the observational data. At least a comparison of how the results would differ if those methods were applied rather than the high- and low-pass filters should be given.

(9) P. 7, last para. While this approach toward identifying real solar-induced signals is probably valid, the time-domain methods mentioned in comment (2) above should also be discussed here.

(10) P. 6-7. In general, the method of analysis should be revised to simulate as closely as possible the methods that were applied to observational time series (comment 1 above). Otherwise, any comparison of the model results to the observational results may not be meaningful.

(11) P. 9, line 4: "All model data used for the analysis are daily and zonally averaged." To allow direct comparisons with observations, this procedure needs to be revised (see

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comment 1).

(12) Page 10, last para. of section 5.1: As discussed in comment (2), it would be straightforward to apply correlation techniques in the time domain to test whether the simulated ozone and temperature signals are caused by 27-day solar forcing. This pargraph should therefore be revised.

(13) Page 10, bottom of page. The wavelet transform method is not defined and no references are provided. Please describe the method in more detail. In general, it is well-known that planetary wave activity produces internal variations with periods near 27 days (e.g., Chandra, 1985). But it is possible to distinguish solar-induced variations near this period using methods in the time domain as mentioned in comment (2). The wavelet transform method does not allow such a discrimination because no phase information is provided.

(14) P. 11, first sentence in section 5.3. This statement is not true because the previous sections did not consider the methods discussed in comment (2) above. Filter and correlation techniques, if carefully and thoroughly applied, can provide a sufficient method for analyzing the atmospheric response to 27-day solar forcing. Please delete or revise this sentence.

(15) P. 11, 11 lines from bottom: “A striking feature of the 27-day signal is its intermittent character.” The same characteristic can be seen in time-progressive cross-correlation functions of actual atmospheric data (e.g., Hood, 1987). A spectral-time analysis is not necessary to demonstrate this although it does show the dependence on frequency, which is useful. Please revise the text to note that similar characteristics can be seen in correlation analysis results.

(16) P. 12, section 5.4, para. 1: It is true that the key discriminant is that “atmospheric variations related to the 27-day solar forcing should be coherent with the forcing over

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long time periods." In the time domain, this means that the phase lag of the atmospheric variation should tend to remain constant over long time periods. Spectral coherence analysis is therefore not the only means for testing whether this is the case or not. This paragraph should be revised to note that this is true: Spectral coherence analysis is only one approach (in the frequency domain). Equally valid methods can be applied in the time domain, although the spectral method is certainly elegant and the dependence on frequency is determined. Also, relevant parts of the Conclusions section (section 6) should be revised accordingly.

(17) Section 5.7. Again, a separate figure comparing only the model sensitivities as derived using various techniques should be constructed. There is no need for a direct comparison to observations here because most of the techniques (seasonal, enhanced forcing, zero forcing, spectral methods, etc.) were not applied to observations.

(18) P. 18, last para. "Figure 12 shows that the linear regression technique can provide a response even in the case without applied 27-day solar forcing." This may be true but it would be straightforward to test whether the response is real with a sufficiently long time series (comment 2). Also, no error bars are provided so it is unclear whether the response is statistically significant. Please revise the paragraph.

(19) P. 19, Section 5.8. Again, Figures 12 and 13 should be completely reconstructed using model sensitivities derived from model data appropriately selected by solar-zenith angle and vertically weighted to allow direct comparisons with the observations. Only a selected subset of the observations should be shown as stated in comment (1). Only when this is done can a meaningful comparison be made of the HAMMONIA model results with the observations. Some real conclusions could then be drawn about whether the model agrees with the observations or not.

(20) P. 22, second para. of section 6: "Our analysis has revealed shortcomings of the correlation (and regression) analysis method. Apparent signals derived by this method

may not be a response to the solar forcing but just represent a part of the internal atmospheric variability." These statements are misleading and should be revised or deleted. If properly applied (comment 2), correlation and regression methods can discriminate between spurious signals caused by internal atmospheric variability and true solar-induced signals.

(21) P. 23. middle of page: "The sensitivity and phase of the ozone response in the tropical stratosphere and lower mesosphere are in satisfactory agreement with available observational results, ..." This statement is premature and may be wrong because the sampling of the model data (daily and zonal averages, $<$ 4 km vertical resolution) is not the same as that of the observational data (e.g., local noon, $>$ 5 km vertical resolution for SBUV) and because the analytic techniques (band-pass filtering) are somewhat different even for the regression with normal forcing analysis. Even for the plotted results, the thin green line in Figure 12a shows an amplitude that is somewhat lower than the SBUV observations between about 40 and 60 km (possibly due to diurnal averaging of the model data). So, the statement does not appear to be valid. "The simulated sensitivities for the stratospheric temperature response are at the lower edge of the range suggested by observations." A more accurate statement would be that the sensitivity for normal forcing (thin green line) is generally lower than that suggested by the observations. Also, the model simulated temperature phase lags disagree in altitude dependence with those estimated observationally (Figure 13b).

(22) P. 23, 11 lines from bottom: "An important deduction ... This statement is not true for tropical latitudes in the altitude range of 30-50 km. It also may not be true in the lower mesosphere where two independent studies have yielded comparable results for both ozone and temperature (Keating et al., 1987; Hood et al., 1991). Please delete or revise this sentence. While it is true that the observations could be improved, the analysis of the model data can also be improved. In particular, as emphasized already in comment (1), care must be taken to analyze the model data in the same

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way that the observations were obtained (i.e., same solar-zenith angle, same vertical weighting, correlation and linear regression to estimate sensitivities and phase lags) so that meaningful comparisons can be made. If this is done in the revision, the new versions of Figures 12 and 13 may help to answer the basic question of whether the HAMMONIA 27-day forcing model results agree with observations or not.

(23) The temperature sensitivities plotted in Figure 12b from the Hood et al. (1991) paper appear to be in error. The maximum sensitivity in the mesosphere was about 0.08 % near 68 km for a 1% change in the 205 nm flux according to Figure 5 of their paper. The measurements of Keating et al. (1987) and Hood et al. (1991) are therefore in better agreement than shown in Figure 12b.

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