Atmos. Chem. Phys. Discuss., 7, S4857–S4864, 2007
www.atmos-chem-phys-discuss.net/7/S4857/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evidence of gravity waves into the atmosphere during the March 2006 total solar eclipse" *by* C. S. Zerefos et al.

C. S. Zerefos et al.

Received and published: 11 September 2007

Reply to Referee's #2 comments

The authors would like to thank the anonymous reviewer for his insightful comments and his valuable suggestions that have helped as improve the final version of our paper. All comments are hereby answered one by one:

1. Although a very interesting atmospheric phenomenon in its own right, the results of this work could benefit from being placed in context of their wider chemical or climatic impact. I would suspect that the momentum and energy transfer due to GWs generated by this mechanism would not rank highly in the budget of global momentum transfer by GWs and that any impact might be very small, even during such a transitory event. This is not to degrade the scientific interest in these results, which is clear, but the

reader would be well-served to be informed of the wider relevance of the results in atmospheric science. Some short discussion of such relevance should perhaps be included in the abstract, discussions and summary sections.

Following the reviewers suggestion we have expanded the first paragraph of the introduction to include short discussion on the important role of GWs on weather, climate and atmospheric chemistry.

2. Fig 1a shows a polynomial fit to total column ozone. Firstly, the total column ozone is clearly incorrect and the reasons for this are discussed on Page 5 and are quoted to be due to contamination by diffuse radiation in the instrument field of view. These reasons are referenced to Kazadzis et al., 2007, which is listed as "to be submitted" and cannot be found. Can you include a brief description of why diffuse radiation leads to strong negatively-biased ozone measurements only during the eclipse?

Kazadzis et al. (2007) is now online in ACPD for more details on the contamination of the measurements with the diffuse radiation that causes the underestimation of total ozone during the eclipse. This effect is negligible at other times because the signal of the direct irradiance is very strong compared to that of the diffuse. During the eclipse the direct irradiance continuously diminishes and in parallel the diffuse component becomes more significant.

3. Can ozone residuals calculated during the eclipse reliably be used in further spectral analysis due to the inherent and unquantified error of this diffuse radiation effect and its increasing relative error with decreasing solar irradiance?If this diffuse radiation effect is symmetric with totality, why is the polynomial fit used either side of totality in Fig 1a not symmetric? Wouldn't this polynomial fit also remove some of the real variation if you do not assume that the instrument response to the diffuse radiation effect is symmetric? Perhaps a polynomial should be fitted after a symmetric function is applied to remove the instrumental response error; if it is even possible to remove the instrumental response function.

ACPD

7, S4857–S4864, 2007

Interactive Comment

Although the total ozone data during the eclipse suffer from the diffuse component contamination, we believe that they can still be used in the spectral analysis, because the effect that causes the problem results in a smooth continuous reduction of the ozone values, which is removed by the applied polynomial fit. The eclipse induced 30-40 min oscillation in the total ozone data are added on top of this smooth reduction of total ozone. Therefore these oscillations will continue to exist in the residuals. Moreover, the existence of these oscillations has been independently confirmed by JO1D and UV-B measurements by different instruments independent from the diffuse effect, which relate directly to total ozone variability. For the same reason, the two branches of the data centered either side of the totality, are not necessarily symmetrical because the actual total ozone has changed during the 3 hours of the eclipse.

4. Much of the spectral power in the 30-minute period (Fig 2b) comes from the residual ozone across the period of the eclipse in Fig 1a, which is also the period of suspect ozone measurement and ultimately relies on the accuracy of the polynomial fit, which is suspect for the reason above. Are the authors then confident with their conclusions using those data?

As already mentioned in the previous question, the oscillations are added on top of ozone measurements which show a reduction during the eclipse, thus the identification of periodicities is independent. Moreover, the disturbance of the ozone layer or in particular total ozone has been additionally reconfirmed with independent measurements of JO1D and UV (305 nm). The fact that we see the same oscillations in all data enhances our confidence on the existence of GWs signals on the ozone layer and strengthens our results.

5. The evidence for any measurement of GWs in the troposphere is unclear and suspect. Although noted by the authors that “the identification of the GWs oscillation in the troposphere has been a more difficult task” in their summary, I would suggest that there is no clear evidence at all for the reasons outlined below and that the authors should revise the paper to state that no clear evidence for tropospheric influence could

ACPD

7, S4857–S4864, 2007

Interactive Comment

EGU

be derived from this study.

After the comments of both reviewers on the confidence of identifying GWs in the troposphere, and since we have also admitted from the very beginning that it has been difficult to draw safe conclusions, the whole section has been rearranged. We have removed those data that are strongly suspect for influence by local, transient processes (e.g. wind speed and PM10), we have tried to strengthen our analysis with temperature at various stations and RH and we finally conclude that even though oscillations are observed no clear evidence for tropospheric influence could be derived.

6. P. 7, Section 3.1.3: With reference to Figure 1c (I assume, but it is not stated, please correct), the authors suggest that the peak-to-peak amplitude of the temperature residual is about 1 percent of the temperature averaged over the eclipse period. Is this statement to illustrate the accuracy of the removal of the diurnal effect by the polynomial fit? By eye, I'd say the peak-to-peak amplitude for temperature in Fig. 1c is about 0.1 units (units are not given in the figure, please include). If the temperature on this day was 10 C then this 1 percent description would hold, but in Kelvin (standard) terms, the peak-to-peak difference was 0.1/283 K or 0.03 percent.

In all parameters used in this study we provide the peak-to-peak amplitude of their residuals, as a characteristic of the bow-wave like structure of GWs. The amplitude can be used for expressing the significance of GWs influence on each parameter and for making comparisons with past or future measurement and modeled predictions feasible. The unit used for temperature and its residuals is Celsius and not Kelvin (included in the graphs) so indeed the amplitude corresponds to about 0.5-1% of the average temperature during the eclipse.

7. Section 3.1.3: The magnitude of the temperature residuals (0.04 K) used for Fourier analysis are very small and well below the point accuracy of any temperature sensor, suggesting that the residual variations could perhaps be instrumental noise. There is no detail of the meteorological sensors in the text and we are earlier referred to

ACPD

7, S4857–S4864, 2007

Interactive Comment

Founda et al., 2007 for such details, but that paper is in the reference list as another “to be submitted” work and I could not find this paper on ACPD or elsewhere. Either way, even a 0.1 K temperature difference is very small and, if not instrumental noise, could be due to the cold downdraft during the passage of a cloud or manifold other transients in boundary layer temperature.

The temperature monitoring has been performed with a Pt-100 sensor (included in Rotronics MP 101A -T7-W4W) with accuracy ś0.1°C, which is the same order of magnitude as the peak-to-peak amplitude of temperature residuals (Founda et al. paper is now on line in ACPD). Data was recorded every 20 sec and 1 min averages were automatically extracted and used for the analysis (error propagation on the average, results to an error of ś0.06 °C). In any case we agree that the temperature signal is at the limits of detection, and even though strengthened by signals in more than one stations and with RH data as well, we finally conclude that manifold rationale could be provided and thus no safe conclusions could be drawn for GWs influence in the troposphere.

8. Section 3.1.3: The apparent 30-40 minute power peak for temperature shown in Fig. 2d, relates to the very small temperature residuals in Fig. 1c which do show some evidence of such periodicity (after smoothing). However, I would like to see the polynomial fit to the measured temperature time-series and the accuracy of the sensor used before assessing the accuracy of the temperature residuals and the subsequent conclusions drawn from them

The temperature data and the polynomial fit used to extract the residuals has been included in Figure 6 (new revised version) according to the reviewer’s suggestion.

9. The use of PM10 aerosol measurements: Measurements of surface PM10 aerosol loading are used in the paper, but it is unclear to me how and why they were used. PM10 aerosols are mostly confined to the lowest few hundred metres of the boundary layer and their concentration with height is highly dependant on surface winds, the local surface environment and combustion processes. Why these measurements might be

ACPD

7, S4857–S4864, 2007

useful in this context is unclear? The paragraph relating the PM10 measurements to JNO2 (Page 7) is most unclear. I can't see how PM10 measurements might be used as a proxy for GW propagation since their nature is too transitory to be of any use.

For all the reasons stated by the reviewer we have removed the analysis for PM10, wind speed and JNO2. The section referred to the influence of GWs in the troposphere has been rearranged accordingly.

10. Abstract: There is much published work in the literature that show the existence of eclipse-induced GWs - a quick search of the Google Scholar search engine revealed at least 20 papers on this subject between 1970 and present, so I'm not sure that their existence is a hypothesis that needs to be tested, as is stated in the first line of the abstract. Although GWs in the lower atmosphere are discussed, they are not mentioned in the abstract, which is more weighted to the ionosphere. If the lower atmosphere results are to be kept in the paper, they should be discussed in the abstract.

The existence of eclipse induced GWs has been predicted in many theoretical works (Chimonas 1970; Fritts and Luo, 1993; Eckermann et al., 2007). Their experimental evidence however is still equivocal since a wide spread of inferred wave properties is revealed and enhanced ambiguity exists concerning their source (e.g., Farges et al., 2003, Eckermann et al., 2007). Davies (1982) questioned whether any definitive experimental evidence exists for a characteristic eclipse-generated gravity wave due to reduced ozone heating in the stratosphere. Taking also into account that most experimental studies are confined to specific atmospheric areas e.g. ionosphere, troposphere, then it becomes clear that the topic is still of great scientific interest. To conclude, our work does not claim to have proven the existence of eclipse induced GWs but to provide new "experimental" evidence using combined observations at the most interesting atmospheric layers namely ionosphere, the ozone layer and the troposphere, in an experiment putting insight into the source of the GWs. Appropriate changes in the manuscript to set this have been made. An addition to the abstract to include discussion on our results in the troposphere has been made.

ACPD

7, S4857–S4864, 2007

Interactive Comment

11. P. 6, Section 3.1, last paragraph: What is the additional noise that is referred to and what is the justification for substituting it for zeroes?

Firstly, we have chosen the maximum period during the eclipse when almost cloud free conditions were found at all sites. Then, as additional noise, we refer to very short cloud signals or even signals from aircraft contrails disturbing the solar radiation, that we had to remove individually at each site, so as to avoid bias added on the spectrum of the series. Since Fourier Analysis is applied on uninterrupted time series, then it is necessary and it is typical to fill in the gaps with the mean value of the series, without significantly influencing the spectrum itself, provided of course that the gaps are not extensive compared to the length of the time series. In our case, since residuals are used, then the mean value of the series is zero so a small number of zeroes to fill in the gaps would not alter the spectral analysis. We have rephrased this part in the manuscript in order to be clearer.

12. P. 6 Section 3.1, last paragraph: What is the need and nature of the "padding" of the data that is mentioned and what is meant by looking for "successive frequencies at smaller increments"? Do you rather mean zero-filling of the coarser time resolved data in the Fourier domain? This needs to be explained.

Two are the most common zero-padding applications in spectral analysis: a) speeding up fast Fourier transform (FFT) calculations by altering the length of the time series to a power of 2 and b) the perceived benefit of improved resolution in the results by just increasing the length of the time series. In the latter case, zeroes are added at the end of the time series so that the frequency resolution in the resulted spectrum is higher while no changes to the spectrum itself are induced. In our study, if no padding was applied, then the frequency resolution in the range of frequencies of interest would range between 5 and 20 min thus making the estimation of the signal's period much less accurate and the discrimination of different, close periodicities difficult. Manuscript has been changed accordingly.

ACPD

7, S4857–S4864, 2007

13. P 7 Section 3.1.3, First pragraph: A low frequency peak at 45 minutes in the wind spectrum is written off as being due to “interference with meteorological discontinuities”. Can you give some examples of such meteorological phenomena and why such phenomena could not also interfere with the 30 minute peak? The same is noted for temperature data at other sites, but is not shown.

The wind speed analysis has been removed for the reasons stated in question 5.

14. All figures except Fig. 5. have units missing from axes. Please include.

All missing units have been included in the figures. Those figures referring to spectral analysis present the spectral estimates (periodogram values) of each parameter, calculated as sums of square coefficients for each frequency.

15. References to work “to be submitted” should not be used where they are crucial to understanding the work presented, e.g. the diffuse radiation instrument problem and meteorological sensors discussed above. Please give details and discuss these issues in sufficient depth within this paper if the references used (Founda et al., 2007, Kazadzis et al., 2007) and are not now published or in press.

The two papers the reviewer refers to are now published in ACPD, so access to them is currently feasible. We have included the full references at the ACPD stage in the list and some more recent additional references. However, the Gerasopoulos et al. overview paper has not been yet submitted and we shall include full reference at a later stage.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 7603, 2007.