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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.

Anonymous Referee #2

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General Comments:

This paper discusses interesting evidence for the generation of atmospheric gravity waves at different atmospheric levels during the passage of the Moon's shadow during solar eclipse. The work is well introduced, but requires a more careful discussion and possibly some major revisions. I would recommend publication as an appropriate article for ACP, subject to a satisfactory answer to the following general and minor comments, with special attention to questionable conclusions relating to the detection of GWs in the troposphere and the nature of ozone residuals (see major comments below). The ionospheric measurements and conclusions seem sound, but to my mind the detection of GWs elsewhere is suspect. If a discussion of GW detection in the lower atmosphere is retained in the revised paper, I would like to see this before recommending

publication.

Specific comments:

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.

2/ Conclusions derived from ozone residuals:

2a) 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?

2b) 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?

2c) 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

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possible to remove the instrumental response function.

2d) 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?

3/ Tropospheric influence:

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 be derived from this study.

3a/ 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.

3b/ 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 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,

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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.

3c/ 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.

4/ 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 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.

Minor comments:

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.

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?

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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.

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.

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

References to work “to be submitted” should not be used where they are crucial to understanding the work presented, eg. 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.

In summary, at present, I think the only clear evidence for GWs in this work are those found in the ionosphere.

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

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