

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.

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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. The authors use surface wind and temperature in their attempt to isolate the characteristics of GWs generated in the stratosphere. As they themselves admit the detection of such a signal is highly unlikely given the complexity of boundary layer processes and the multitude of signals that such processes produce on surface meteorology. The fact that the temperature residuals are extremely small and that no information on the accuracy of the temperature and wind measurements is provided (only a reference to a paper in preparation) makes it hard to obtain any confidence in the presented spectral analysis results. In any case, if the authors want to establish the downward propa-

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gation of the stratospheric GWs they could potentially search for some high temporal resolution data that would be a proxy of upper tropospheric activity rather than surface processes. The use of the photolysis rates by themselves does not appear to resolve this issue.

After the comments of both reviewers on the confidence of identifying GWs in the troposphere, 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 providing additional information on the accuracy of the sensors used and we finally conclude that even though oscillations are observed no clear evidence for tropospheric influence could be derived. Unfortunately no upper tropospheric data were available and concerning surface data there has been no problem with the temporal resolution but with the accuracy of the sensors which is explained in the text. We have included all necessary information about the instruments accuracy (Founda et al. is in the meanwhile available online in ACPD).

2. The present paper refers for some parts of the analysis to three papers that are presently in preparation and therefore cannot be accessed by the reader. If the authors insist on publishing the current paper first, they should include more details on the analysis parts currently detailed in the other three papers. This comment refers both to the accuracy of the meteorology measurements mentioned above but also to the method that is used to remove the eclipse and diurnal effects and derive the residuals from the ozone data. This last part is not clearly explained in the paper even though it forms a basic component of the overall analysis.

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 also some recent references. However, the Gerasopoulos et al. overview paper has not been yet submitted and we shall include full reference at a later stage. The reviewer may have misunderstood that the method used to remove the eclipse and

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diurnal effects and derive the residuals from the ozone data is described elsewhere. It is only the complete initial time series that can be found in more details in Gerasopoulos et al. (2007). However, to satisfy both reviewers' comments we have included, apart from the residuals, all initial data and polynomial fittings, in this manuscript as well and this way the methodology followed and control of the fitting accuracy is more clear.

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

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