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Interactive comment on "Detection and measurement of total ozone from stellar spectra: Paper 2. Historic data from 1935–1942" *by* R. E. M. Griffin

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General comments

The paper describes the determination of total ozone from stellar spectra recorded 70 years ago. The procedure is relatively complex and was performed only for a limited number of observations in the sense of a pilot study. However, it offers a fascinating perspective for future work, as hundreds of thousands of stellar spectra have been recorded during the 20th century. Even more important, in my opinion, is the fact that this method is completely independent from the standard method used for measuring

total ozone, i.e., the Dobson spectrophotometer. There are almost no other opportunities for comparing independent total ozone data in the early decades of ozone measurements. In this sense, the paper is extremely valuable and certainly merits publication after some alterations. The author is to be congratulated on having successfully accomplished an extremely laborious task. Title and abstract are adequate, the paper is well structured and it is clearly within the scope of Atmospheric Chemistry and Physics. However, there are a number of technical points that raise some questions and should be considered in a revised manuscript.

Specific comments

Section 2 presents a precise account of the archive work done, including important details that will be helpful for future work in this area. The method sections (Sections 3 and 4) make clear that calculating total ozone from photographed spectra involves many steps, from the calibration of the characteristics of the photographic emulsion to the final data conversion. As to the first part of the method, leading from the photographic plates to the digitised spectra, my expertise is very limited. I don't know much about photographic emulsions, photometric fidelity of scanners, and stellar continui and hence I am not able to technically assess Section 3. I note that several steps require subjective decision or "hand work" (e.g., the photometric calibration or the placement of the stellar continuum). If such work is done by an experienced person such as Elizabeth Griffin, this procedure might yield the best achievable results. However, with regard to potential future applications, an automated and objective procedure is necessary. In this sense, the method presented in the paper is in an exploratory stage. I would like the author to comment on this aspect and on future developments of the method. But I am impressed by the highly resolved spectra that are obtained in this way.

I have some comments, though, on the second part of the method section (Section 4), which describes how total ozone was derived from the digitised spectra. Some details in this section are not clear. For instance, it is not clear to me why on-screen measuring

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was used to determine the equivalent width, as the spectra are available in digital form. Also, what does the author mean with “O3 profiles are sketched in with a smoothed line” (1st paragraph)? It seems that the reason for this procedure is the interference with the star’s own absorption lines. This brings me to the more general question of why ozone is calculated directly from the measured area under the curve rather than using a more general fitting procedure. Maybe this is a naïve question, but why is it not possible to simultaneously fit the ozone absorption spectrum and the absorption spectra of a few candidate gases that are suspected to cause the stellar absorption lines? This would lead to an approach similar as that used in the DOAS technique. Maybe this approach would not work after the placement of the continuum done in Section 3, but I would like the author to comment on this (or why this approach is not possible). It is also unclear to me what exactly the author did to the absorption coefficients (summing up, subtracting the smoothly varying Hartley continuum). "Summing up" appears several times where I would prefer "integrating" because otherwise the unit will be wrong. The term “equivalent width” (and how it is calculated) should be defined in the text. Also, units should be given for the quantities mentioned in the text, otherwise it is not clear why the slope is in Dobson units. Was the slope determined by the method of “Least Absolute Residuals” (LAR)? Also, it is not clear how the ozone absorption lines were defined. Is it the curve between two maxima that were used to fit the continuum?

I am surprised how well the ozone calculated from the individual ozone absorption lines correspond with each other. This is a good argument for the method. When looking in detail at plot 2, it also seems that there are systematic features that appear in both plots in the sense that a particular absorption lines gives higher or lower values in both plots. Is this the case for all spectra? I would be interested to see a plot of the residuals from the fitted lines, averaged over all plates (or all plates from a given star). This could be a sensible instrument to detect - and possibly correct - further sources of error.

For the calculation of the air mass the author uses the approximation secZ. While this is good enough in general, it may be too crude for high solar zenith angles and it could

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very easily be improved. It should be noted that the air mass relevant for ozone absorption is not the same as the geometrical air mass factor. It depends on the ozone profile and wavelength. Also, the station altitude must be considered. Air mass factors are important for ozone retrievals from satellite and many algorithms are around. In the handbook for Dobson spectrophotometers (Komhyr, 1980), a very simple approximation for the calculation of the relevant air mass for ozone absorption is used. If this equation is applied to the Mount Wilson data as given in the paper, the maximum error is 1.2%. While this is not very much compared to other errors, it might still be good to correct for this, especially with respect to future work that might involve stellar spectra taken under even higher solar zenith angles.

The author uses air mass factors that are averaged over the exposure time. For this one has to assume that the darkening of the photographic plate varies linearly with exposure time even over several hours (otherwise one would have to weigh the air mass factors accordingly, which would be a simple thing to do if the shape of the response function is known). Is this actually the case? Maybe one sentence on the problem of long exposure times, apart from the cloud problem already mentioned, would be appropriate.

The section on the possible errors is very valuable, but I did not understand exactly how the error bars were calculated. Also, errors are always given in percent and it is sometimes not clear whether this refers to percent of the final total ozone value or something else. I am wondering whether there could be an interference with SO2 absorption, which also has a banded structure in the 315-335 nm range. A comment on this would be helpful.

I have some important comments on Section 5, the comparison with Arosa total ozone. Arosa is very far away from Mount Wilson. Total ozone mean values are expected to be different, and anomalies are not expected to be correlated. It is true that Arosa is the most reliable total ozone series for this time period. One should consider, however, the expected difference in the mean values as documented, e.g., in the TOMS data. The

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TOMS overpass data for Table Mountain (which is very close from Mount Wilson) are significantly lower than for Arosa, especially from late winter to summer. The difference reaches up to 10%. Hence, if the two series agree well in the historical period, that could point to a systematic overestimation in the Mount Wilson data. Also, the seasona. Cycle looks a bit different at the two sites. A more detailed, but somewhat special comment on possible comparison with other data follows in a separate online comment. Also, I suggest using TOMS overpass data (perhaps from 1979 to 1981, when the anthropogenic effect was still small and prior to the eruption of El Chichon) to address the seasonal cycle and for further comparisons.

Technical comments

Section 1, paragraph 1: For the sake of completeness, the author could mention the Tromsø total ozone series that was recently re-evaluated and goes back to 1935 (Hansen and Svenøe, 2005).

It would be nice to cite Staehelin et al. (1998) for the Arosa data.

Caption of Fig. 2: Give units and explain “gf” and “equivalent width”.

Table 2, line 11: Zenithal O3 should be 329 DU.

Caption of Fig. 3: What are the crosses? What is the dashed line?

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