Response to 2nd comments by Forrest M. Mims III, posted on 21 Dec 2018

General remarks to comment by reviewer

We thank Mr. Mims for his additional comments but feel that many remarks are beyond the scope of the paper. Our paper is about the importance of applying corrections for solar limb darkening when observing a solar eclipse; the question of whether or not bow waves from the Moon's shadow may result in fluctuations in total ozone column (TOC); and the question of whether or not the ratio of direct-to-diffuse irradiance changes appreciably during the period of a solar eclipse (excluding the period near totality). The paper is NOT about the best, most accurate, or most precise method to measure TOC.

In our response to the first comments by the reviewer (posted on 24 November 2018), we provided new evidence that our method of measuring TOC during the eclipse is precise enough for detecting potential changes in TOC from bow waves. In brief, we concluded that the noise in our measurements is low enough for detecting relative changes in TOC of larger than 0.05 %. In addition, we calculated that changing aerosol concentrations during the time of our observations result in an additional uncertainty in TOC of 1.5 DU or 0.5 %. Even if our uncertainty estimates were too optimistic, for example due to an unknown systematic error in our TOC retrieval method, it would be highly unlikely that variations in our calculated TOC values would anti-correlate with real variations in TOC triggered by bow waves such that the resulting TOC measurements after the 3rd contact become basically flat, with a variation of only ±1 DU or ±0.33 % (see Fig. 9 of original manuscript). For comparison, the peak-to-peak amplitude attributed to bow waves reported by Zerefos et al. (2007) was 2.0–3.5 %, and the peak-to-peak amplitude reported by Mims and Mims (1993) was 1.7 %.

In conclusion, we cannot rule out that that the Moon's shadow led to variations of TOC in the order of ±0.3 % during the eclipse observed by us, but note that this upper limit is considerably lower than the fluctuations reported by Zerefos et al. (2007) and Mims and Mims (1993).

Changes to manuscript

In the abstract, the following sentence:

> "In contrast to results of observations from earlier solar eclipses, no fluctuations in TOC were observed that could be attributed to gravity waves."

will be replaced with:

> "In contrast to results of observations from earlier solar eclipses, no fluctuations in TOC were observed that could be unambiguously attributed to gravity waves."

In Sect. 8.3., the following sentence:

> "Our data do not support the observation by Zerefos (2000; 2007) and Mims and Mims (1993) that bow waves from the Moon's shadow lead to oscillations in TOC."

will be replaced with:

> "Our data do not support the observation by Zerefos (2007) and Mims and Mims (1993) that bow waves from the Moon's shadow may lead to oscillations in TOC with a peak-to-peak amplitude exceeding 1.5 %."

Comment by reviewer:

TWO KEY POINTS: The authors have made several important revisions to their paper, but they simply must remove their erroneous assertions to the effect that: (1) the signal at 300 nm is noisy, which suggests poor measurements by TOPS (which used 300 nm and 305 nm) and (2) full-sky measurements of the ozone layer are "similar" to the direct sun measurements employed by Dobsons (and TOPS, Microtops, Brewers and Pandoras). These inappropriate claims and their 2-minute measurement time support their general assertions that raises doubts about the papers by me and others.

Authors' Response

Regarding (1): We do not assert anywhere in the paper that signals of the TOPS instruments are noisy or that measurements of this instrument are of poor quality. In fact, we do not even mention "TOPS" in the manuscript. The discussion of the noise characteristics of the TOPS instrument is only part of the reviewer's first post and our response. It is and will not be part of the paper.

Regarding (2): We do not state in the manuscript that "full-sky measurements of the ozone layer are 'similar' to the direct sun measurements employed by Dobsons (and TOPS, Microtops, Brewers and Pandoras)". We do not compare the accuracy of our TOC measurements with that of other methods. Like in the case of (1), the discussion on the quality of the different methods of measuring ozone is only part of the reviewer's first post and our response.

Changes to manuscript

None.

Comment by reviewer:

1. OPTIMUM WAVELENGTH SELECTION FOR MEASURING TOTAL COLUMN OZONE DURING A SOLAR ECLIPSE: The authors claim the 300-nm minimum employed by TOPS provides a noisy signal. They erroneously observe that: “Of note, the shortest wavelength of a Dobson is 305.5 nm and if measurements at 300 nm would be of great advantage, these instruments would likely use a shorter wavelength.” The 305.5-nm Dobson minimum wavelength was selected due to the use of the instrument across a wide band of latitudes. However, there is ample signal at 300 nm at my site (29.9 N), as demonstrated by the instrument’s detection of an error in NASA’s Nimbus-7 TOMS ozone instrument (Mims, Nature, 1993). The 300 nm signal was especially strong at 22 N during the 1991 solar eclipse reported in my paper in GRL. The authors might be right about the 300-nm signal at their northerly location. But they cannot compare what might have been a noisy 300 nm signal at their northerly 44.36 N site to the much higher amplitude 300-nm signal at the 22 N site for the eclipse I measured. Furthermore, DeLuisi and others have demonstrated that wavelengths below 305 nm provide more accurate ozone measurements than higher wavelengths. (This explains why TOPS found the satellite error.)

Authors’ Response

Similar to the last comment, we do not discuss the TOPS instrument in our paper. The quote in the reviewer’s comment above is again from our response to the reviewer’s first post. Also, we do not discuss the best wavelengths to be used to calculate TOC for the location of “our” eclipse. We simply state in the manuscript that TOCs were calculated from the measurements of the GUVis-3511’s channels at 305 and 340 nm.

Changes to manuscript

None.

Comment by reviewer:

Of special concern is this from the author’s abstract: “The total ozone column (TOC) was derived from measurements of global irradiance at 306 and 340nm.” While the 306 nm minimum is appropriate for 44.36 N, the 340 nm upper wavelength is far too high, for it allows for significant aerosol errors. The closely-spaced 300 nm and 305 nm wavelengths of TOPS nearly eliminated the aerosol error, which explains this instrument's excellent accuracy when compared with hundreds of satellite measurements and an EPA Brewer for 60 days at my site. Consider this abstract by Saunders et al in High-Precision Atmospheric Ozone Measurements Using ... trial spectral irradiances between 290 and 305 nm (JGR Atmospheres 1984 https://doi.org/10.1029/JD089iD04p05215) "Abstract "It is shown theoretically that many errors are significantly less when determining atmospheric ozone thicknesses from measurements of solar terrestrial spectral irradiance in the wavelength region between 290 and 305 nm as compared to the to the 305- to 340-nm region employed by the Dobson spectrophotometer. In order to test this conclusion experimentally, an elaborate set of state-of-the-art measurements have been made in the shorter wavelength region in Gainesville, Florida, between June 13 and June 18, 1980. Details of these measurements, including an extensive error analysis, are presented and indicate that such short-wavelength measurements, particularly between 295 and 305 nm, can be used to detect long-term changes of atmospheric ozone with an uncertainty not exceeding 1%. Observing conditions restricted the Gainesville measurements to zenith angles of less than 35°. Further investigations are required to determine the shortest wavelength that can be used at significantly greater zenith angles."

Authors' Response

We agree with the reviewer that aerosols will lead to systematic errors in TOC measurements if the wavelengths used for the retrieval are far apart. However, we have estimated the uncertainty in our TOC retrievals for the period of interest and have concluded that variations in aerosols during the period of the eclipse cause an uncertainty in TOC of only 1.5 DU or 0.5 %. We note that this uncertainty refers to the precision of TOC measurements (the metric of relevance to the paper), not absolute accuracy, which could be worse. However, our TOC retrievals agree to within 3 DU (or 1%) with OMI, suggesting that also the accuracy of our data is within acceptable limits.

The discussion of whether or not TOC measurements should be based on wavelengths in the 290 to 305 nm range is of little relevance to the paper because the GUVis-3511 radiometer has no channels with wavelengths below 305 nm. In addition, while TOC measurements using wavelengths shorter than 305 nm could indeed be more accurate, as the JGR quoted by the reviewer suggests, we like to point out that a higher accuracy can only be achieved if the filters of the instrument in question are well characterized. No filter instrument measures at exactly a nominal wavelength and all real instruments (including the TOPS) use filters with a finite bandpass. If a hypothetical channel that is supposed to measure at 300 nm measures in fact at 300.5 nm, TOC errors will result. We are not implying that this is the case for the TOPS instrument, but just like to point out that performing ozone measurements with channels at 300 and 305 nm can potentially lead to errors that could be comparable in magnitude to those affecting measurements using channels that are farther apart, and as a result are more sensitive to aerosols. Again, we cannot, and do not want to, assess the quality of TOPS measurements because we do not use data of this instrument in the paper and are not familiar with its characteristics.

The sentence "The total ozone column (TOC) was derived from measurements of global irradiance at 306 and 340nm." in the abstract does not judge whether this wavelength selection is the most appropriate. It simply states what was done and we see no reason to change it.

Changes to manuscript

None.

Comment by reviewer:

2. FULL-SKY VS. DIRECT SUN TOTAL OZONE MEASUREMENTS: The authors state in their paper: "The method of calculating TOC from measurements of global irradiance (instead of direct irradiance as it is typically done for Dobson, Brewer, TOPS, and Microtops instruments) was first proposed by Stamnes et al. (1991). We found that the accuracy of TOCs derived from global irradiance is similar to that of data from Dobson instruments or satellite (TOMS, OMI) observations if the look-up table takes local conditions into account (ozone profile, albedo, elevation, etc.) (Bernhard et al., 2005b)." The authors suggest that global irradiance provides TOC data ". . . similar to that of data from Dobson instruments or satellite. . . ." But the authors provide no data or citations to support this assertion, while leaving open the counter suggestion that Dobsons (and TOPS, Brewers and Pandoras) could be replaced by much simpler instruments that measure global irradiance and require no tracking and pointing.

Authors' Response

We do not state in our paper:

> “The method of calculating TOC from measurements of global irradiance (instead of direct irradiance as it is typically done for Dobson, Brewer, TOPS, and Microtops instruments) was first proposed by Stamnes et al. (1991). We found that the accuracy of TOCs derived from global irradiance is similar to that of data from Dobson instruments or satellite (TOMS, OMI) observations if the look-up table takes local conditions into account (ozone profile, albedo, elevation, etc.) (Bernhard et al., 2005b).”

as asserted in the comment by the reviewer above. This quote is taken from our comment to the first post of the reviewer. Instead, we simply state in the paper:

> “This method was first proposed by Stamnes et al. (1991) and was validated for GUV instruments by Bernhard et al. (2005a).”

We are puzzled by the reviewer’s comment:

> “The authors suggest that global irradiance provides TOC data “. . . similar to that of data from Dobson instruments or satellite. . . .” But the authors provide no data or citations to support this assertion, while leaving open the counter suggestion that Dobsons (and TOPS, Brewers and Pandoras) could be replaced by much simpler instruments that measure global irradiance and require no tracking and pointing.”

because the paper and the response to the post of the reviewer does include two citations (i.e., Bernhard et al., 2005a, and Bernhard et al., 2005b) where TOC measurements from global irradiance are compared with Dobson direct measurements and satellite (TOMS, OMI) observations.

In Bernhard et al., 2005b, we conclude that:

> “When Dobson measurements are corrected for the temperature dependence of the ozone absorption cross section and accurate air mass calculations are implemented, data from the three instruments agree with each other to within ±2% on average and show no significant dependence on SZA or total ozone.”

The “three instruments” quoted above refer to the Dobson; our SUV-100 spectroradiometer, which measures global irradiance; and TOMS. The reviewer may not agree with this conclusion, but it is false to assert that “the authors provide no data or citations to support this assertion“.

We further like to point out that the method of retrieving TOC from spectra of global irradiance measured by SUV-100 radiometers has been published by Bernhard et al. (2003). This paper also includes an uncertainty estimate of the method. The abstract ends with:

> "On average, the new algorithm generates ozone values in spring 2.2 % lower than TOMS observations and 1.8 % higher than Dobson measurements. From the uncertainty budget and the comparison with TOMS and Dobson it can be concluded that ozone values retrieved from global UV spectra have a similar accuracy as observations with standard instrumentation used for ozone monitoring."

Changes to manuscript

In Sect. 5.2., we will replace

> "This method was first proposed by Stamnes et al. (1991) and was validated for GUV instruments by Bernhard et al. (2005a)"

with

> "The method of calculating TOC from measurements of global irradiance was first proposed by Stamnes et al. (1991) and was further validated by Bernhard et al. (2003; 2005b). The application of the method to GUV instruments was described by Bernhard et al. (2005a)."

Comment by reviewer:

Of course, this is not the case. The authors have gone much, much too far in suggesting their global, full-sky data is "similar" to direct sun measurements made by the recognized standard for nearly a century and all other ozone instruments. I have always been intrigued by the prospect of accurate ozone retrievals by pairs of global UV measurements at closely-space wavelengths. I urge the authors to prepare a detailed paper about their claim that global TOC measurements are "similar" to those by traditional direct sun instruments. An ideal comparison would be with Brewers, which measure both direct sun and global TOC. Meanwhile, it is inappropriate to make an unsupported claim about "similar" results.

Authors' Response

The reviewer suggests to prepare a detailed paper about our claim that global TOC measurements are "similar" to those by traditional direct sun instruments. This paper has already been written. In fact, there are two: Bernhard et al. (2003; 2005b) and we therefore don't agree with the reviewer's conclusion that it is inappropriate to make an unsupported claim about "similar" results.

We do not advocate in any of our papers that the established network of Dobson and Brewer instrument should be replaced with instruments measuring global irradiance. The “global irradiance method” is only sufficiently accurate if the ozone profile is known with sufficient accuracy, in particular for large solar zenith angles. So the method is not as independent as direct measurements that rely on Beer-Lambert’s law and hence do not require knowledge of the profile. However, when there is cloud cover, the direct method cannot be used and Stamnes et al. (1991) showed that the “global” method is equally accurate as the zenith sky method used by Dobsons when the Sun is obstructed by clouds.

As a side note, because TOC retrievals from global irradiance depend on the ozone profile, the profile can in fact be determined from such measurements using a variant of the Umkehr method. We have recently published this “Global-Umkehr method” (Bernhard et al., 2017). Again, we not propose that this method is superior to the standard Umkehr method, which relies on zenith sky observations. The paper is just a prove of concept and makes the Umkehr method available to locations with global irradiance measurements. The abstract of Bernhard et al. (2017) concludes with “Total ozone columns (TOCs) calculated from the retrieved profiles agree to within 0:7±2:0 % (±1σ) with TOCs measured by the Ozone Monitoring Instrument on board the Aura satellite.” This demonstrates again the good accuracy of the TOC retrievals from global measurements if its done right.

Changes to manuscript

None.

Finally, we like to conclude that most of the discussion above is of little relevance to our paper. The only important question is whether or not our TOC retrievals from global irradiance measurements of the GUVis-3511 are of sufficient precision to detect fluctuations in TOC that could be triggered by bow waves. Using a sensitivity analysis, we concluded that this is the case. Based on our results, we cannot rule out that there is an effect with an amplitude of ±1 DU or ±0.33 %. We simply conclude that we did not observe fluctuations in TOC that could be unambiguously attributed to gravity waves. It is possible that the magnitude of gravity wave effects varies from eclipse to eclipse, explaining the discrepancy in the data by us, Zerefos et al. (2007) and Mims and Mims (1993), but this is speculation.

Changes to manuscript

None.

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