Comment on "A homogeneous database of sunspot areas covering more than 130 years" by L. A. Balmaceda et al.

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Received 20 January 2010; revised 20 June 2010; accepted 24 June 2010; published 10 September 2010.

Citation: Foukal, P. (2010), Comment on "A homogeneous database of sunspot areas covering more than 130 years" by L. A. Balmaceda et al., *J. Geophys. Res.*, *115*, A09102, doi:10.1029/2010JA015293.

1. Introduction

[1] In their recent paper, *Balmaceda et al.* [2009] make a number of statements that require correction.

[2] For one, the conclusion presented in their abstract [*Balmaceda et al.*, 2009, paragraph 1] is that "There is no basis for the claim [ascribed by the authors to me] that UV irradiance variations have a much smaller influence on climate than total solar irradiance variations." Over 25% of the main body of the paper is devoted to disproving this claim. I have never made such a claim.

[3] *Foukal* [2002] (cited by Balmaceda et al.) shows that 20th century global temperature variation *correlates* better with variation of total solar irradiance (TSI) than with UV flux variations. But it is stressed by *Foukal* [2002, paragraph 1] that "the [total] irradiance variation amplitude is insufficient to influence global warming in present-day climate models." I am not aware of any way to compare the likelihood of climate driving by a mechanism that lacks power relative to another that lacks correlation, and I have never made such a comparison. Despite their lengthy protestations, Balmaceda et al. are still unable to cite any instance where I made such a claim. In fact, my paper deals evenly with both TSI and UV flux as possible mechanisms of climate driving.

[4] Second, Balmaceda et al. claim that our 2002 reconstruction of TSI was "incorrect" because it was based on "uncritical" use of sunspot area data. Actually, the spot-blocking function, provided by J. Lean, was adopted after critical consideration of the analysis by *Fligge and Solanki* [1997]. They, one of whom is a coauthor of the Balmaceda et al. paper, reported a correction factor between Royal Greenwich Observatory (RGO) and U.S. Air Force (USAF) spot areas of 1.2, thus less than *half the size* now claimed by Balmaceda et al. This correction would not have changed the finding that TSI and UV flux variations differ, given the very uncertain magnitude and time variability of any such correction, discussed below.

[5] The correction factor is estimated to be 1.49 ± 0.19 via the Russian data used by Balmaceda et al. But if we estimate it using the RGO versus Rome and Rome versus USAF overlaps described by *Fligge and Solanki* [1997], we obtain (using the ratios for these overlaps from *Balmaceda et al.* [2009, Table 2]) the lower factor 1.30. It is puzzling why these lower values, 1.2–1.3, are *not even mentioned* by Balmaceda et al., although these estimates are based on almost *twice the length of overlaps* (between RGO and Rome and Rome versus USAF) as those available in the comparison via RGO versus Russia.

[6] The formal error estimates [from *Balmaceda et al.*, 2009, Table 2] applied to this lower correction of 1.2–1.3 would be ±0.16 if the ratio behaved even roughly according to Gaussian statistics. But the behavior is often far from Gaussian. For instance, Figure 3 of *Fligge and Solanki* [1997] shows that for 10 of the 17 years of overlap between RGO and Rome, the agreement was essentially perfect, so the factor would have been 1.0. Over most of the remainder it would have been ~1.15. Almost never did the factor assume its "average" value. It is then more accurate to describe the uncertainty as a range of a bimodal distribution and to combine the ranges linearly, rather than quadratically. In that case the range would be about ±0.22.

[7] In summary, the evidence supports a correction factor between approximately unity and 1.7, with about equal likelihood. It is questionable whether a TSI reconstruction based on such an uncertain and highly time-varying correction is better than no correction at all. It also calls into question whether the correction by Balmaceda et al. results in the "homogeneous" time series they are claiming.

[8] Third, the reader's confidence in the uncertain statistics might be strengthened if the authors were able to explain the reason for higher RGO (and Russian) areas. *Balmaceda et al.* [2009, paragraph 24] state that "This is mainly due to the significant difference in the minimum value of the counted sunspots (1 ppm…for Russian versus 10 ppm…for SOON observations)" (reiterated in their conclusions in paragraph 41). This explanation is not correct. Examination of the RGO spot group records shows that the contribution of spots of area <10 ppm is negligible at times of appreciable activity; it does not begin to explain the 40%–50% correction claimed by Balmaceda et al.

[9] The additional suggestion that differences between areas measured from photographs and drawings might contribute also does not match the evidence. The *Hathaway et al.* [2002] correction of 1.4 that Balmaceda et al. mention as agreeing with theirs is based on comparison of (umbral) areas measured at Mount Wilson and by RGO. Both are photographic records.

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[10] Fourth, the authors discuss (paragraph 39) the post-1970s rise in my 2002 reconstructed TSI as the only feature in comparing the correlation of variations in TSI and UV flux with the time series of global temperature. In fact, of at least equal importance [*Foukal*, 2002, paragraph 12] is "that our time series of S [TSI] reproduces the double maxima in T around 1940 and 1957 remarkably well." This is not affected by the uncertain correction the authors are proposing.

[11] Fifth, if the correction factor were as high as 1.4, then this work by Balmaceda et al. would indicate that the poor correlation between variations in global temperature and UV flux shown in my 2002 paper (and now supported by independent U.S. and Russian analyses [*Foukal et al.*, 2009; *Tlatov et al.*, 2009]) applies *also* to the correlation between variations of global temperature and TSI. That would only strengthen the main conclusion of my 2002 paper; that solar radiative driving of climate change is problematical, whether one considers UV or TSI variations as the forcing agent. Balmaceda et al. do not mention this.

[12] Finally, the comparisons with the sunspot number raise questions that are difficult to evaluate here. The comparison with sunspot number shown in Figure 3 is plotted for cycles 12–20 for the RGO data and for cycles 22–23 for the Solar Optical Observing Network (SOON) data. Given the large cycle-to-cycle dispersion in relation between spot areas and spot numbers, it is difficult to conclude anything from this plot. In summary, uncertainties in spot area scales certainly deserve consideration in irradiance reconstructions. Future work that deals properly with the time variability of the corrections and identifies their origin could improve TSI reconstructions. But Balmaceda et al.'s paper contains many errors of fact and representation that greatly weaken their case.

[13] **Acknowledgment.** Philippa Browning thanks the reviewer for his assistance in evaluating this paper.

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