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Reply to comment by P. Foukal on "A homogeneous database of sunspot areas covering more than 130 years"

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[1] *Foukal*'s [2010] comment (hereinafter referred to as Fcom) on *Balmaceda et al.* [2009] makes a number of claims and assertions that require a response, since many of them are without a factual basis.

[2] 1. *Foukal* [2010, paragraph 2] writes the following:

> For one, the conclusion presented in the abstract 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.

First, the main goal of *Balmaceda et al.* [2009] (hereinafter BEA09) is the compilation of a homogeneous sunspot data set after proper calibration of areas provided by different observatories. Most of the abstract and the paper is devoted to the analysis and comparison of sunspot areas from different observatories. The irradiance reconstruction from the P_S index was included as an example on how the use of noncalibrated areas can lead to erroneous results. Only 2 pages out of 12 (1 section out of 7) deal with this topic.

[3] 2. Second, we thank Foukal for clarifying in Fcom the true aim of *Foukal* [2002] (hereinafter F02). The paper itself gave a very different impression. Thus, our statement was made on the basis of affirmations in the abstract of F02 [2002, paragraph 1] such as the following:

> Differences in time-variation between total and ultraviolet solar irradiance could help in separating their influence on climate…. Correlation of our time series of UV irradiance with global temperature, *T*, accounts for only 20% of the global temperature variance during the 20th century. Correlation of our total irradiance time series with *T* accounts statistically for 80% of the variance in global temperature over that period, although the irradiance variation amplitude is insufficient to influence global warming in present-day climate models.

[4] We understand this last caveat (Fcom, paragraph 3) to mean that Foukal believes that the problem lies with present-day climate models rather than with correlations per se. It would make little sense if the statement were to question the value of correlations, since the main focus of F02 lies on the difference between the *correlations* of total and spectral irradiance with temperature. This interpretation is further strengthened by a number of passages in the main body of F02 [2002] (only some of which are cited here):

> It is the difference in shape between the UV and total irradiance time series, not their amplitude, that interests us as a discriminator between their signatures in the climate record (paragraph 13).
>
> Our main finding is that the time-behavior of total and ultraviolet irradiances since 1915 are distinctly different (paragraph 16).
>
> The correlation we find between global temperature and F_{UV} over most of the 20th century is relatively low. But it seems sufficient to account for, e.g., the roughly 20% of climate variance seemingly associated with solar variability in the Holocene temperature record (paragraph 17).
>
> High correlation between total irradiance variation and *T* has been noted previously in empirical models.. But our study is the first to contrast this with the much lower correlation between *T* and F_{UV} (paragraph 18).

[5] Although F02 [2002, paragraph 20] discusses that the total irradiance amplitude is too low to give a significant contribution to global warming, it is then stated, "Nevertheless, the possibility of significant driving of 20th century climate by total irradiance variation cannot be dismissed [e.g., *North and Wu*, 2001]…. Interpretation of this high correlation between *S* and *T* awaits improved understanding of possible climate sensitivity to relatively small total irradiance variation."

[6] 3. Fcom [2010, paragraph 4] claim that a "critical consideration of the analysis by *Fligge and Solanki* [1997]" was carried out. However, F02 does not cite that paper nor provide any explanation why he ignored its quite relevant results.

[7] 4. *Foukal* [2010, paragraph 4] states that "They… 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." (with "They" he means *Fligge and Solanki* [1997]). This is *not* correct. The factor of 1.2 obtained by *Fligge and Solanki* [1997] was between RGO and Rome and between RGO and Yunnan data (using Rome as an intermediary). Since RGO and USAF (Solar

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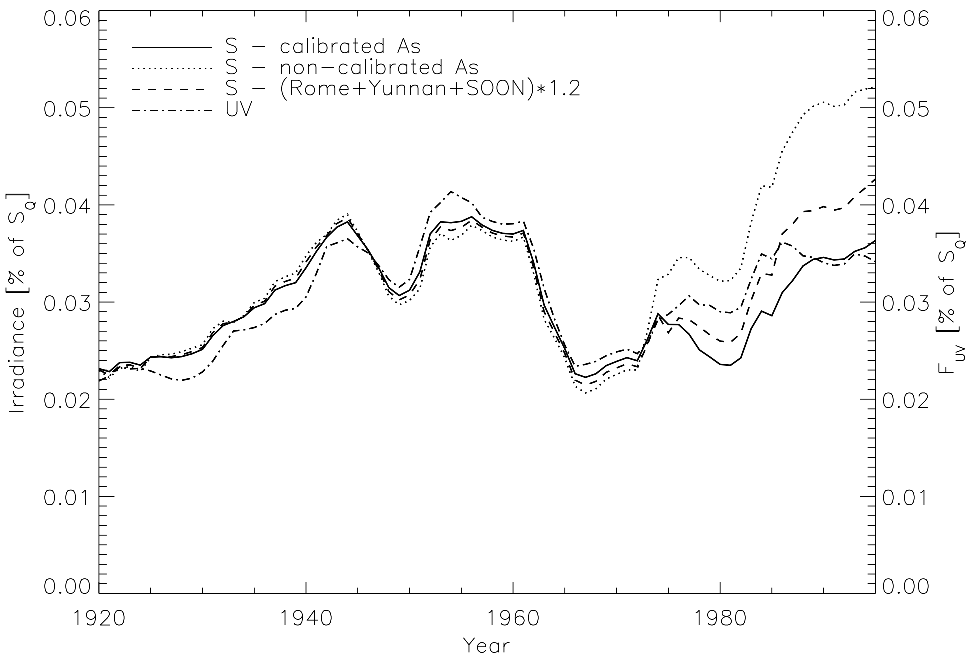

Figure 1. Eleven year running means of reconstructed total irradiance, S, from different sunspot area data sets (marked by curves) and UV irradiance, F_{UV}, represented by the A_{PN} time series. Both quantities are relative to the quiet Sun irradiance, S_Q = 1365.5 W/m^2 used by BEA09.

Optical Observing Network (SOON)) data do not overlap, their direct comparison is impossible and has to be done via another data set. If we take Rome as an intermediary between RGO and SOON, then the analysis of *Fligge and Solanki* [1997] returns a factor between ≈1.1 × 1.15 = 1.27 and ≈1.1 × 1.25 = 1.38, while that of BEA09 gives values between 1.3 and 1.46 (see BEA09, Tables 2 and 3), which is roughly consistent with the results obtained by using the Russian data as intermediary (as preferred by BEA09). Thus, the range of 1.2–1.3 quoted by Fcom for the factor between RGO and SOON (Fcom, paragraph 5) is largely below the lower limit allowed even if using the lower quality Rome data as an intermediary. In any case, by using the result of *Fligge and Solanki* [1997] to reconstruct total irradiance, we still get a result much closer to BEA09 than to F02, see our point 6 below.

[8] 5. Foukal insists on using Rome as an intermediary rather than the Russian data chosen by BEA09. There are good reasons for the choice made by BEA09: the Russian data (1) are more similar to RGO data (with a smaller correction factor and the same minimum spot size), (2) have an order of magnitude fewer gaps (BEA09, Table 1), and (3) do not display the inconstant behavior shown by the Rome data (which was noticed even by Fcom (paragraph 6), see also our point 7 below). Thus, e.g., using Rome data as an intermediary would have led to a very gappy data set between 1976 and 1981 that would have had to be filled by as many data points from Russian data as available from Rome data. Note that the Russian record was not available to *Fligge and Solanki* [1997].

[9] 6. *Foukal* [2010, paragraph 4] also claims that "This correction [factor of 1.2] would not have changed the finding that TSI and UV flux variations differ…" We have followed up this unsubstantiated statement and reconstructed the total irradiance, S, according to the method of F02 but now using the A_S series from the combination of Greenwich, Rome, and Yunnan data, the two latter multiplied by a factor of 1.2 as proposed by *Fligge and Solanki* [1997]. The resulting S curve is plotted in Figure 1 (dashed curve). Clearly, it lies considerably closer to F_{UV} (dot-dashed curve) and S from the sunspot areas A_S calibrated by BEA09 (solid curve) than to the reconstruction based on noncalibrated A_S (dotted curve), which is very similar to the S published by F02. This is supported by the RMS difference between the F_{UV} curve and the three reconstructions of S for the period after 1976: whereas the RMS difference to dashed and solid curves (*Fligge and Solanki* [1997] calibration and BEA09 calibration) is 0.00354 and 0.00314, respectively, and the difference to the dotted curve is 0.00931, i.e., significantly larger.

[10] 7. In paragraph 6, Fcom argues that the errors in the sunspot areas do not follow Gaussian statistics but display a bimodal distribution, in particular the ratio between Rome and RGO. In Figure 2, we plot the histogram of ratios between Rome and RGO, together with the best fit Gaussian. Clearly, the statistics are relatively close to being Gaussian (although there is an extended tail) and are far from bimodal. Note also that the ratio of Russian to RGO data follows Gaussian statistics far more closely than the plotted ratio. The more unusual statistics displayed by Rome than by Russian data when compared with RGO (and also with SOON) was another reason for BEA09 to concentrate on the Russian data.

[11] Hence, Fcom's main argument against the validity of our result, which is based on (1) his faulty claim about the statistics not being Gaussian, (2) his insistence on using Rome data in spite of their obviously lower quality, and (3) his use of the incorrect factor of 1.2 between RGO and SOON, is without any basis.

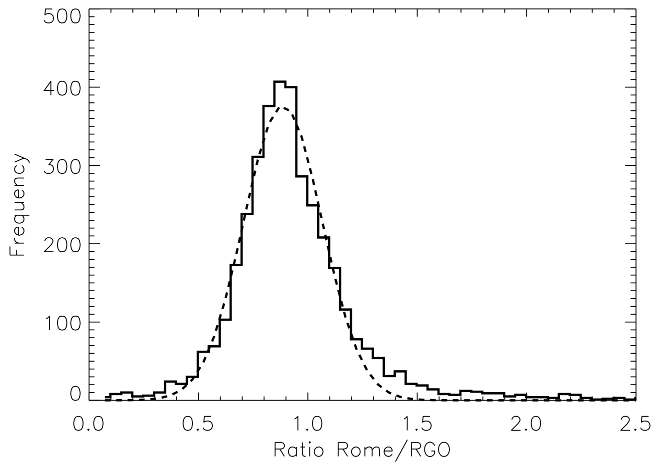

Figure 2. Distribution of the ratio between Rome and RGO sunspot areas and the best fit Gaussian.

[12] 8. In paragraphs 8 and 9 of Fcom, Foukal states that we cannot provide a satisfactory explanation for the difference between the various historic data sets. We repeat that the main aim of BEA09 was to create a consistent sunspot areas record, not to find an explanation why data records differ from each other. For historical data such an undertaking will remain difficult. Note that a far bigger and more fundamental question about the physics of the Sun is raised by the drastic change in the relationship between sunspot area and sunspot number between the time that RGO stopped recording and SOON started *if one uses uncorrected sunspot area data* (BEA09, Figure 3). Why, after displaying on average the same size over many cycles, do sunspots suddenly display only two thirds that average size once another data set is used? After applying the calibration provided by BEA09 (derived quite independently of sunspot numbers), the RGO and SOON relationships with sunspot number agree exceedingly well with each other. Foukal does not even attempt to provide a physical explanation for this sudden and huge change in average sunspot size. A well-founded physical explanation for such a massive effect would give at least some credence to his claims.

[13] 9. *Foukal* [2010, paragraph 12] also makes the claim that "The comparison with sunspot number shown in Figure 3 of *Fligge and Solanki* [1997] is plotted for cycles 12–20 for the RGO data and for cycles 22–23 for the 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." We are completely at a loss as to how Foukal reaches this conclusion. The difference in relationship is *far* bigger than the scatter. Consider the slopes to the sunpot area versus sunspot number regression lines: For RGO we find 18.0 ± 0.2; for SOON after correction by the factor of 1.49 we find 17.7 ± 0.3; but for SOON without correction we find 11.8 ± 0.2. Although the first two agree within $\sim$1–1.5σ with each other, the last differs by over 30σ.

[14] We note that recently *Hathaway* [2010], using an independent analysis, i.e., employing sunspot numbers instead of a third sunspot areas data set, finds a factor of 1.48 between RGO and SOON data, in close agreement with the BEA09 result of 1.49. This further strengthens the result of BEA09 and casts further doubt on the arguments put forward by Fcom for a factor of 1.

[15] 10. Nowhere do BEA09 claim that the reconstruction by F02 does not do a reasonable job prior to 1976, when Greenwich data were used. The statement in paragraph 10 of Fcom is consequently irrelevant.

[16] 11. We agree with Fcom [2010, paragraph 11] that "solar radiative driving of climate change is problematical," *as long as we consider the period since around 1970*. Regarding earlier times, we believe that a final answer is pending, awaiting improvements in climate models.

[17] 12. That the calibrated A_S record is the better one to use is also indicated by a comparison with independent reconstructions of total solar irradiance that do not make use of the sunspot area [*Steinhilber et al.*, 2009; *Schöll et al.*, 2007]. The first one is based on the relationship between the open flux and total solar irradiance derived by *Fröhlich* [2009] and makes use of cosmogenic radionuclides. The 11 year smoothed total irradiance curve [*Steinhilber et al.*, 2009, Figure 1d] shows a slightly higher peak around the 1990s compared to the 1960s (with the difference being <0.1 W/m^2). Foukal's *S* curve (F02, Figure 2a) shows for the same time interval, 1960–1990, an increase of $\approx$0.015% of S_O, which corresponds to 0.2 W/m^2, i.e., over twice the value found by *Steinhilber et al.* [2009]. *Schöll et al.* [2007] use the sunspot number for the short-term trend and neutron monitor measurements for the long-term reconstruction of irradiance variations [*Schöll et al.*, 2007, Figure 4]. For cycle 19, it exhibits a bigger amplitude than for cycles 21 and 22, very similar to the dotted curve in Figure 6 of BEA09 (see solid curve in Figure 1), based on calibrated A_S, but quite different from the reconstruction of F02, where cycles 21 and 22 are considerably stronger than 19. Note that on shorter time scales, neutron monitor measurements are considerably more reliable than cosmogenic isotopes, so that we trust more the result of *Schöll et al.* [2007].

[18] In conclusion, the criticisms voiced by Foukal in his comment do not hold up to closer scrutiny, as we have shown above. It is amply clear that the use of calibrated sunspot areas data, as proposed by BEA09 in agreement with *Hathaway et al.* [2002] and *Hathaway* [2010], is to be preferred over the use of noncalibrated data, as still supported by Foukal.

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

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