

Interactive comment on “Re-construction of global solar radiation time series from 1933 to 2013 at the Izaña Atmospheric Observatory” by R. D. García et al.

Anonymous Referee #2

Received and published: 29 July 2014

The paper presents the development of a time series of global horizontal solar irradiance, for the period 1933–2013, by combining two types of data obtained at the Izaña Observatory. On the one hand, for the most recent years (after 2005), the authors use the high quality solar irradiance data measured by pyranometers belonging to global networks such as BSRN. For the period 1992–2005, pyranometer measurements are also used, but they must be previously checked and calibrated. A radiative transfer model (LibRadtran) is also used as a supporting tool for this process. On the other hand, for the years previous to 1991, sunshine duration measurements taken by a Campbell-Stokes heliograph are used as a proxy for solar irradiance. In this sense,

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Angstrom-Prescott formula is adjusted by using simultaneous measurements of both kinds for the period 1992–2001.

The subject of the paper is relevant within the current context of investigations about the long-term past evolution of solar radiation at the Earth's surface, including the broadly known phenomena of dimming and brightening. The approach is quite methodological, so it can be interesting for application to other sites where similar datasets are available. Both subject and approach seem adequate for a journal like Atmospheric Measurement Techniques. So I recommend publication, although several aspects of the paper must be improved previously.

Please note that I wrote my review before looking at the other comments (from the other reviewers and from the authors) that are already posted on the “discussion” of this paper. I usually proceed in this way to avoid being influenced by someone else's opinion. I am sorry if my comments are contradictory with some of the other comments, but I think that the authors would be able to manage all of them.

MAJOR POINT

The authors analyze the behavior of the Campbell-Stokes heliograph (CS) and also of the most modern Sunshine Duration sensor (CSD); in both cases, Angstrom-Prescott expressions are fit (by using several years of data) and then are tested (by applying them to other years of data). However, the only data that are actually used in the reconstruction of the solar radiation series is that from the CS device, since measurements from CSD are concurrent with solar radiation (shortwave downward radiation, SDR) measurements. Therefore, in order to make the paper simpler and clearer, and to avoid unnecessary digressions, I would remove all references to the second instrument and I would focus on the use of the Campbell-Stokes data.

MINOR POINTS

- Section 2.3, fifth paragraph. This paragraph presents a quality control of the CS

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measurements (i.e., of daily sunshine duration) by comparing with results from the LibRadtran model corresponding to cloud-free days. So, the indices of agreement (RMSE, SEM) should have units of hours (h) and not of MJm⁻² as are incorrectly written in the text. Similarly, these units (h) should appear in RMSE and Intercept in Fig. 1. In addition, the arguments used to explain the overestimation of the CS measurements seem quite weak. First, winter conditions (cool and wet) should reduce the CS sunshine duration so the overestimation should be less, not greater. Second, the effect of diffuse radiation being concentrated by the glass sphere onto the reading card is negligible compared with direct radiation. Contrarily, the authors do not mention that part of the differences between CS measurements and LibRadtran data could come from some deviation of the model results, which, regarding the direct component, are not validated (in Section 3 the authors mention some results of a previous study, but they do not mention if these results correspond to global or direct radiation).

- Section 4. The sentence “In addition to meteorological variables (temperature, humidity, . . .) the SD mainly depends on the fraction of clear sky (FCS)” is awkward. SD depends on solar direct irradiance, which is of course affected by clouds (and aerosols, and water vapor atmospheric column). The dependence on temperature and humidity is very minor and purely instrumental (it only affects Campbell-Stokes instruments): in other words, if we measure SD by pyrheliometric methods, no dependence on temperature or humidity should be found). Moreover, it sounds quite strange to affirm that SD depends on FCS, and then define FCS from SD measurements (Eq. 7). Moreover, Eq. 7 uses SD_{exp} and SD_{max}, which, if I understand correctly are exactly the same magnitudes that in Eq. 2 are written as n and N_d . In summary, this paragraph should be totally rewritten for further clarity and consistency.

TECHNICAL CORRECTIONS

Abstract:

- Should the location of Izaña in the Canary Islands be mentioned from the very begin-

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ning, in the abstract?

- The expression “when it was not possible” is unclear.

- It is unnecessary to give both the determination coefficient (0.92) and the correlation coefficient (0.96) of an agreement, since they are totally related with each other.

- Since all the CS data (not only those with FCS > 40%) will be used in the reconstruction, it seems to me that the improved performance of the Angstrom-Prescott expressions for this range of cloudiness is not so relevant.

- The word “discontinuities” should be changed by a more appropriate word. “Discontinuities” suggests inhomogeneity in the series, and I think this is not what the authors want to say.

Introduction:

- Although the reference Sanchez-Lorenzo et al. 2007 is already cited, I would say that it should be called in the introduction as well, since this paper shows the phenomena of dimming-brightening in the Iberian Peninsula (so relatively close to the Canary Islands) by using sunshine duration measurements (the same kind of measurements that the authors of the current study are using).

- The paper by Sanchez-Lorenzo and Wild (2012) could also be cited in the third paragraph.

- Can the affirmation that IZO is “representative of subtropical North Atlantic free atmosphere” be substantiated somehow? Similarly, in Section 2, when it says “IZO is a worldwide reference station”.

Section 2.1:

- In the description of the instruments (and/or in Table 1) it should be made clearer if the change of network meant also a change of instrument. In other words, the CM-21 of the NCR is the same one of the BSRN? Or is the instrumentation duplicated?

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- The sentence about the time resolution of measurements and its conversion to daily irradiation should come before the sentence about the agreements of daily data among the different instruments. In addition, Eq. (1) is unnecessary; saying that daily irradiation has been computed from 1 minute measured irradiances is enough.

- The last paragraph mention "short gaps" but at least one of these gaps is quite long: September 2003-july 2005 (i.e., almost 2 years).

Section 2.3:

- In the second sentence of first paragraph, "direct solar irradiance" should be specified again. In addition, the value of 120 Wm^{-2} is indeed the expected direct solar irradiance shortly after (before) sunrise (sunset) in cloud-free, but also in low aerosol load, conditions.

- In the fourth paragraph, the authors justify the analyses of the CSD measurements despite they are not used in the reconstruction. Exactly for this latter reason, and as I mentioned before, I would avoid mentioning and analyzing these measurements.

- The third paragraph, where some issues that affect CS measurements are mentioned, should come with appropriate references. In particular, point (4) states that different operators may get "very different" totals. This seems excessive, and a quantification or an adequate reference should be added.

Section 4:

- After Eq. (2), you do not need to repeat the units ($\text{MJ m}^{-2} \text{ day}^{-1}$) twice.

- Expressions needed to compute the extraterrestrial irradiance can be removed by using appropriate references.

Section 4.1:

- P. 4204. It is strange that RMSE in winter and summer have exactly the same values (3.1%), while the authors comment that conditions are less favorable in winter. In ad-

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dition, this latter affirmation seems contradictory with what is said in p. 4201 ("months from October to February to assure very stable atmospheric conditions"). In the last paragraph of this page, a systematic bias is given, but units are missing.

- A reference regarding the values provided for China should be added.

Section 4.2:

- Second paragraph: where Fig. 6a is mentioned, it should be Fig. 5a.

Section 5:

- Why do you compute new coefficients for the Angstrom-Precott formula (table 5) instead of using those coefficients (table 4) that have already been validated against independent measurements? In addition, how come coefficients in Table 5 are quite different from those in table 4?

Section 6:

- First sentence: I would add that some measurements taken at TO have also been used.

- Second paragraph: "CNR" should be "NRC"

Fig. 1

- Could you comment why there is a gap in days with sunshine duration around 12 h?

Fig. 5

- I do not understand the blue solid line. It does not seem to represent a linear trend, since it is not a straight line.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 4191, 2014.

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