Anonymous Referee #2 (RC, 7, C1927-C1932, 2014)

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.

Referee #2:

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, Ångström-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.

> **Authors:** We appreciate the positive and constructive suggestions. Most of them have been incorporated in the final version of the manuscript. Please, find below a detailed response to each of the major, minor points and technical corrections.

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.

Authors: Following your recommendation, the authors have decided to remove everything related to CSD records in the final manuscript. We agree this will make the manuscript clearer and simpler.

MINOR POINTS

1. **Section 2.3, fifth paragraph. This paragraph presents a quality control of the CS 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).**

Authors:

1. In relation to the units of the indices of agreement, the units have been modified in the final manuscript.

2. Regarding the overestimation, we fully agree with the referee's comment. There was a mistake in the presentation of the comparison. Also, we would like to clarify in this section that the selected days to perform the intercomparison between SD records and LibRadtran simulations were cloud-free and low aerosols content days in order to ensure very stable atmospheric conditions at IZO (WMO, 2008). Days with low aerosol content are mainly observed in winter because during the rest of the year Saharan dust events can affect the station (especially in summer).

3. Indeed, part of the differences may be attributed to the model uncertainties. García et al. (2014) reported that the theoretical total random error due to input model uncertainties is 0.04 MJm^{-2} (0.12%) for direct radiation estimates. These values agree with the results obtained with the comparison between observations and simulations in which it has been obtained a mean bias (simulations-observations) of -0.16±0.34 MJm^{-2} (-0.4±0.9%). These results were added and referenced in the final manuscript. In order to support the use of the LibRadtran model at IZO and the quality of the simulations performed, in the framework of

routine pyrheliometer and pyranometer calibration works, we have computed the relative difference between simulations with *LibRadtran* model and measurements with PMO-6 (http://www.pmodwrc.ch/pmod.php?topic=pmo6) absolute cavity radiometer system. The median relative difference is -0.19% (RMSE 0.49%) and -0.29% (RMSE 0.28%) for 6th and 10th June 2014, respectively (see Figure 1). These simulations were performed in the framework of calibration routines for pyrheliometers installed at Izaña Observatory.

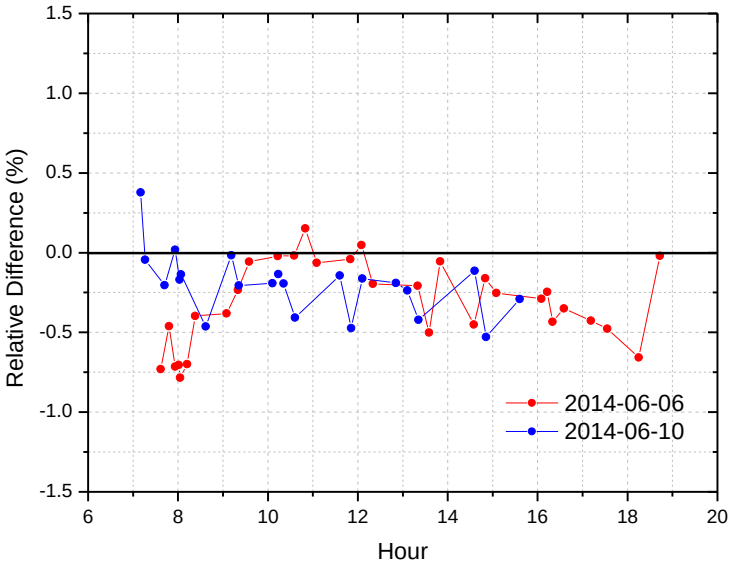

Figure 1.- Relative differences between direct solar radiation measurements with PMO-6 radiometer and simulations with LibRadtran model at Izaña Observatory on 6th (red line) and 10th (blue line) June 2014.

We added at the end of the Section 2.2 (Radiation transfer model and input parameters) the following paragraph:

"García et al. (2014) reported that LibRadtran GSR and DSR can theoretically be estimated with an uncertainty of 0.09 MJm^{-2} (0.31 %) and 0.04 MJm^{-2} (0.12 %) by comparing with solar observations, the mean bias (simulations-observations) is -0.30±0.24 MJm^{-2} (-1.1±0.9 %) for GSR and -0.16±0.34 MJm^{-2} (-0.4±0.9 %) for DSR"

In section 2.3 (Sunshine duration data), the paragraphs have been clarified in the final manuscript as follows:

"In order to document the precision of the IZO SD measurements as observed by the CS, we have compared these measurements to those obtained from DSR simulated with LibRadtran when exceeding a threshold value of 120 Wm^{-2} *since DSR measurements are not available during the CS data series period (see Sect. 2.2 for details about the simulations). We have considered all the cloud-free days, selected by using the method of Long and Ackerman (2000), with low aerosol*

content because they ensure very stable atmospheric conditions at IZO, are mainly observed from October to February and between May and June (Rodríguez et al., 2011, and references therein)".

"…Nonetheless, we observe that CS records systematically overestimate the sunshine hours by 3.1 %, showing a seasonal dependence: the CS record tends to overestimate SD by 2.4 % from October to February, and 5.6 % in May and June. This seasonal variation, also found in other authors (e.g. Kerr and Tabony (2004); Hinssen and Knap (2007)), may partly be attributed to the different response of the CS recorder to atmospheric conditions in winter and summer months. For example, the card strip reacts in a different manner whether the ambient air is humid (typically in winter) or dry (typically in summer) (Wood et al., 2003) as well as the burning of the card strip is not well defined at sunrise and sunset, leading to differences through the year. Also, the uncertainties introduced by the model, about 1 %, should be taken into account (see Sect. 2.2)."

4. **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 Nd. In summary, this paragraph should be totally rewritten for further clarity and consistency.**

Authors: Following the referee's recommendations this paragraph has been changed as follows:

"The SD records and, thus, the fraction of clear sky (FCS) defined here by Eq. (7), depend on solar direct irradiance, cloudiness (amount, type and thickness), PWV and atmospheric aerosols (mainly mineral dust particles at IZO, (Rodríguez et al., 2011; García et al., 2012)).

$$FCS(\%) = \frac{n}{N_d} * 100 \qquad (7)$$

They also depend, in a lesser extent, on meteorological variables as temperature and humidity, although this dependence is very small and purely instrumental. All of these factors account for the stratification found in Fig. 3a, where five regions of the FCS values…"

TECHNICAL CORRECTIONS

Abstract:

1) **Should the location of Izaña in the Canary Islands be mentioned from the very beginning, in the abstract?**

> **Authors:** Following the referee's recommendation, we have added the following sentence in the abstract:
>
> "…at the subtropical high-mountain Izaña Atmospheric Observatory *(IZO), located in Tenerife (The Canary Islands, Spain)*…."

2) The expression "when it was not possible" is unclear.

> **Authors:** This expression has been replaced by:
>
> "…Since GSR measurements have been used as a reference a strict quality control has been applied *based on principals of physical limits and comparison with LibRadtran model…"*

3) 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.

> **Authors:** This has been modified following the referee's recommendations:
>
> "*We obtain an overall root mean square error (RMSE) of 9.2 % and an agreement between the variances of GSR estimations and GSR measurements within 92%"*

4) 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 Ångstrom-Prescott expressions for this range of cloudiness is not so relevant.

> **Authors:** Indeed, all of the FCS values have been taken into account for the reconstruction of the series. However, most of them have FCS>40% (~90% of days between 1933 and 2013) during the testing period. Therefore, we consider that the improvement observed by using the Ångström-Prescott expressions for FCS ranges is significant and should be emphasized in the Abstract.
>
> This point will be clarified in the Section 4.1

5) 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.

> **Authors:** This expression has been replaced by: "…The reconstructed IZO GSR time series between 1933 and 2013 confirms *change points* and periods of increases/decreases of solar radiation…"

Introduction:

1) **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).**

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

Authors: Both references have been included in the Introduction.

3) **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".**

Authors:

1. Regarding IZO could be considered as representative of subtropical North Atlantic free atmosphere, we have added the following explanation to Section 2 (Site description, measurements and tool):

> "…IZO is a suitable site for in-situ and remote sensing observations and optimal for calibration and validation activities due to a high atmospheric stability, high frequency of clean and pristine skies, a stable total column ozone, very low column water content and low aerosols content. *IZO provides atmospheric measurements representative of free troposphere conditions of the subtropical North Atlantic region due to the quasi-permanent subsidence regime typical of the subtropical region (Cuevas et al., 2013, Gómez-Pelaez et al., 2013 and references herein*)…"

2. Likewise, we had added that "*IZO is a world-wide reference station*", because the Izaña station is a global GAW station according to WMO-GAW programme definition (http://www.wmo.int/pages/prog/arep/gaw/gaw_home_en.html) (see map hereafter), and it belongs to numerous international monitoring networks and research international programmes (detailed information in http://izana.aemet.es). Nonetheless, we have decided to remove this statement from the manuscript.

Figure 2.- GAW Global stations (http://www.wmo.int/pages/prog/arep/gaw/measurements.html)

Section 2.1:

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?**

Authors: Each network (NCR and BSRN) has its own instruments and data evaluation/calibration procedures. In fact, since January 2009 we have simultaneous measurements obtained by both BSRN and NCR.

This point will be clarified in Section 2.1 (please see comments 2 below).

2) **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.**

Authors: Following the referee's recommendations, this paragraph has been changed as follows:

"…Note that the different pyranometers acquire GSR records on a 1-minute basis. However, in this work, we use the daily GSR values, calculated by integrating the 1-minute measured GSR from sunrise to sunset (García et al., 2014)"

3) **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).**

Authors: The authors agree with the referee and this sentence has been modified as follows:

"To complete the gaps observed in the long-term GSR time series at IZO, we have used the GSR measurements taken at the Teide Observatory (OT, http://www.iac.es) managed by the Instituto de Astrofísica de Canarias (IAC)."

Section 2.3

1) **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.**

Authors: We appreciate this referee's comment. We have clarified this point in the revised manuscript as follows:

"SD is the time period that the ground surface is irradiated by direct solar radiation (i.e., sunlight reaching the earth's surface directly from the sun). In 1982, WMO defined it as the period during which direct solar irradiance exceeds a threshold value of 120Wm^{-2} (WMO, 1982).This value accounts for the expected direct solar irradiance shortly after (before) sunrise (sunset) in cloud-free, but also in low aerosol load conditions (WMO, 1984)".

2) **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 mentioned before, I would avoid mentioning and analyzing these measurements.**

Authors: See question MAJOR POINT (page 1)

3) **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.**

Authors: See question MINOR POINT (page 2-3)

Section 4:

1) **After Eq. (2), you do not need to repeat the units (MJ m-2 day-1) twice.**

2) **Expressions needed to compute the extraterrestrial irradiance can be removed by using appropriate references.**

Authors:

1. They have been modified in the final manuscript.

2. The solar literature contains a wide range of papers referring to the calculation of the Sun position (Blanco-Muriel et al., 2001). These calculations can be classified into two groups. The first one is a group of relative simple formulae and algorithms that, given the day of the year, estimate basic Sun-position parameters, such as the solar declination or the equation of time (Cooper, 1969; Lamm, 1981; Spencer, 1971; Swift, 1976). The second consists on more complex algorithms (Michalsky, 1988; Pitman and Vant-Hull, 1978; Walraven, 1978). To avoid any confusion, the authors have decided to maintain all the expressions and approximations used to evaluate the extraterrestrial solar radiation on a horizontal surface in this work.

Section 4.1:

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 addition, 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.**

Authors:

1. It was a *typographical error. Thereby it has been corrected in the final manuscript as follows:*

   "...The intra-annual bias reveals that the GSR estimations (Fig. 4a) are more accurate in summer (RMSE of 3.1%) than in winter (*RMSE of 9.3 %*)..."

2. Please, see question MINOR POINT (Page 2-3)

3. *The units for the bias values have been added.*

2) **A reference regarding the values provided for China should be added.**

Authors: The reference has been added in the final manuscript.

Section 4.2:

1) **Second paragraph: where Fig. 6a is mentioned, it should be Fig. 5a.**

Authors: This typo has been modified in the final manuscript.

Section 5:

1) **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?**

Authors: The coefficients *a* and *b* are very sensitive to the size of the data set used for evaluating them. Please note the differences between Table 4 and 5, as the referee suggested. Therefore, we have used the longest available period (1992-2000) to calculate the final coefficients *a* and *b* for the reconstruction of the long-time series, what redounds in lower SEM (standard error of the mean) in the calculated coefficients.

Section 6:

1) **First sentence: I would add that some measurements taken at TO have also been used.**
2) **Second paragraph: "CNR" should be "NRC"**

Authors: These comments have been modified in the final manuscript.

Fig. 1

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

Authors: The selected days for comparison between observations and simulations are characterized by cloud-free and low aerosols content (please recall the response to the Minor point). The gap around 12 h corresponds to the months of March, April, July, August and September when IZO is affected by high aerosols content due to Saharan dust events (see time series of SD records in Figure 3 of the manuscript).

Fig. 5

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

Authors: The Figure 5 has been replaced by the following one:

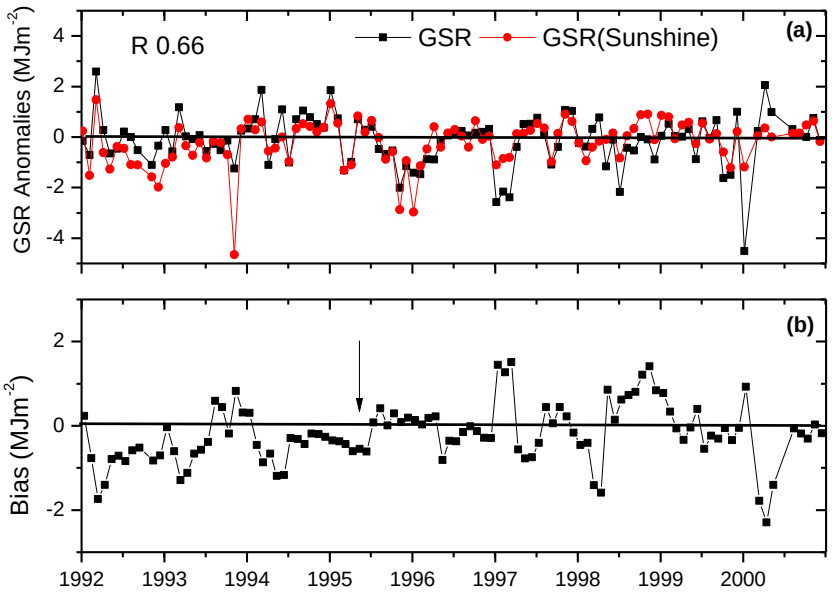

Fig. 5. Times series of monthly median of (a) the deseasonalized anomalies of GSR estimations (red line) and measurements (black line) and (b) monthly median bias between GSR estimations and measurements (MJm^{-2}) from 1992 to 2000 at IZO. The error bars indicate ± 1 SEM (standard error of the annual means) and the black arrow indicates the change point date.

By eliminating all references to CSD records from the final manuscript, long-term consistency of GSR estimations are computed for the period 1992-2000. So, as there are a lower number of years available, the authors have decided to work with monthly values.

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