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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Fifty-six years of Surface Solar Radiation and Sunshine Duration at the Surface in São Paulo, Brazil: 1961–2016" *by* Marcia Akemi Yamasoe et al.

Marcia Akemi Yamasoe et al.

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We would like to thank both anonymous referees for the devoted time evaluating and reviewing our manuscript, with constructive suggestions to improve the final version. Each comment is addressed as follows, with the answer provided bellow:

Referee #1 The paper by Akemi Yamasoe et al. presents the results of the analysis of a 56-year record of surface downward solar irradiation with respect to other atmospheric parameters for São Paulo, Brazil. The authors try to define the main drivers of changes in irradiation during the period of study. Although some of the findings are interesting, improvement is necessary prior to publication. More specific comments are provided

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Discussion paper

below.

1. L50: Since the two different trends are not a global phenomenon (e.g. even some of the referred studies show different results for China and India), I suggest adding “over wide regions of the world” or something similar after “documented”.

Authors response: The text was added as suggested.

2. L58: Zerefos et al. (2009) could be also cited at this point (in addition to Wild 2012): ZEREFOS, C.S., ELEFTHERATOS, K., MELETI, C., KAZADZIS, S., ROMANOU, A., ICHOKU, C., TSELIOUDIS, G. and BAIS, A. (2009), Solar dimming and brightening over Thessaloniki, Greece, and Beijing, China. Tellus B, 61: 657-665. doi:10.1111/j.1600-0889.2009.00425.x

Authors response: Yes, indeed the suggested reference complements the discussion about the geographical heterogeneity of the “brightening phase” comparing UV-A and total solar irradiances at Thessaloniki, in Greece, and Beijing, in China.

3. L65-66: Relative discussion (regarding the main drivers of the trends over particular areas) can be also found in: - Kazadzis, S., Founda, D., Psiloglou, B. E., Kambezidis, H., Mihalopoulos, N., Sanchez-Lorenzo, A., Meleti, C., Raptis, P. I., Pierros, F., and Nabat, P.: Long-term series and trends in surface solar radiation in Athens, Greece, Atmos. Chem. Phys., 18, 2395–2411, https://doi.org/10.5194/acp-18-2395-2018, 2018. - Manara, V., Brunetti, M., Celozzi, A., Maugeri, M., Sanchez-Lorenzo, A., and Wild, M.: Detection of dimming/brightening in Italy from homogenized all-sky and clear-sky surface solar radiation records and underlying causes (1959–2013), Atmos. Chem. Phys., 16, 11145–11161, https://doi.org/10.5194/acp-16-11145-2016, 2016. - Manara, V., Bassi, M., Brunetti, M. et al. 1990–2016 surface solar radiation variability and trend over the Piedmont region (northwest Italy). Theor Appl Climatol 136, 849– 862 (2019). https://doi.org/10.1007/s00704-018-2521-6

Authors response: We appreciated your suggestions and added the one below besides

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the suggested references: Yang, S., Wang, X. L. and Wild, M. Causes of Dimming and Brightening in China Inferred from Homogenized Daily Clear-Sky and All-Sky in situ Surface Solar Radiation Records (1958-2016). Journal of Climate 32, 5901-5913, doi: 10.1175/JCLI-D-18-0666.1, 2019.

4. L92: Delete “Thus,”

Authors response: Word deleted.

5. L118: Please define if this is the standard (k=1) or the expanded (k=2) uncertainty.

Authors response: It is the standard (k = 1) instrumental uncertainty. More information concerning SSR uncertainty and long-term shift of the actinograph calibration, please, see reply to item 22 for referee #2 below.

6. L135 – 147: I am very skeptical about the methodology used to study the effect of aerosols. The authors have used a very small number of cloud-free days for each year in the period July – October in order to study the effect of aerosol. I doubt that with such a small number of days (i.e. 9 days for some years) the authors can get safe conclusions. Furthermore, I do not think that the results can be generalized for the whole year.

Authors response: We agreed with both referees that the number of cloud-free days for each year is not enough for a robust statistical analysis. For this reason, we changed this part of the manuscript, also modifying the discussion on the aerosol effect. Instead, we replaced with a discussion on visibility, using it as a proxy for aerosol optical depth and the number of foggy days during the same period, i. e. from July to October. In this time of the year, the aerosol can have a stronger effect on SSR, due to reduced cloud fraction, higher aerosol loadings either because of more stable conditions and less precipitation allowing air pollution to build up or due to long range transport of smoke from vegetation fires in other parts of South America. Occurrence of fog is also more frequent.

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7. L172: The AOD from MODIS at which wavelength?

Authors response: The AOD analysis was removed. The aerosol impact on SSR deserves a careful analysis and with a more appropriate database.

8. L173 – 175: “Shortly : : : spectrum”. Please add the appropriate reference.

Authors response: This part of the text was removed.

9. L177 – 178: Again, I believe that the authors should analyze and discuss the AOD and the AAI for different seasons in the year, and for the whole year. This way they would also provide some evidence for what they claim, i.e. that the effect of aerosol is significant only in July – October.

Authors response: This part of the text was removed.

10. L168 – 183: Some discussion regarding the uncertainties in the AOD and AAI product would be useful.

Authors response: This part of the text was removed.

11. L211:” it is listed amongst the 24 strongest El Niño events”. The 24 strongest events during which time period?

Authors response: According to the Earth System Research Laboratory from the National Oceanic and Atmospheric Administration (ESRL/NOAA) the time period is from 1895 to 2015 (https://www.esrl.noaa.gov/psd/enso/climaterisks/years/top24enso.html). This period was included in the manuscript to make it clear.

12. L227 – 229: “After 1983, the trend behavior of all variables changed”. What does this phrase mean? How do the authors define the “change”? While it is acceptable to study the trends for two different periods, the authors cannot support that there is a change in the trends without any further statistical analysis. What I mean is that someone could argue that e.g. the trend in SSR did not change at all, or that the trend in SD changed in 1980. If the authors want to support their statement that “the

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trend changed" in a particular year, or period of years, they should use more robust statistical analysis. See for example the methodology used by Yang et al., 2006 in order to investigate whether there is a statistically significant change in the trends of stratospheric ozone: Yang, E.S., Cunnold, D. M., Salawitch, R. J., McCormick, M. P., Russell, J., Zawodny, J. M., Oltmans, S., and Newchurch, M. J. (2006), Attribution of recovery in lowerâËŸARËĞ stratospheric ozone, J. Geophys. Res., 111, D17309, doi:10.1029/2005JD006371.

Authors response: We agree with the referee comment, modifying and complementing the statement to (see further discussion below): "After 1983, the trend behaviour of some variables changed, consistent with the findings of Reid et al. (2016), who observed a regime shift in land surface temperature anomalies in South America in 1984".

Applying different statical analyses, we verified that Tmax presented a statistically significant (p = 8.5×10-7) regime shift in 1984, according to the method proposed by Rodionov (2004), based on mean values. Using the package "segmented" from R (Muggeo, 2003), only SD and DTR presented a shift, in 1982 (p = 0.008) and in 1979 (p = 0.017), respectively, indicating that depending on the variable and the methodology, the trend change can be detected in different years. The observed change in Tmax, in 1984, is consistent with the findings of Reid et al. (2016). The authors evaluated 72 time series around the world to analyse the 1980s regime shift. They observed that the shift was first observed in South America, in 1984, and spread toward North Pacific and North America (1985), to the North Atlantic Ocean (1986), Europe (1987) and Asia (1988). In the Southern Hemisphere, it extended eastwards to the Indian Ocean in 1986 and Australia in 1987. Shortly, one hypothesis is that it was a combination of factors, from a recovery of the cooling effect caused by El Chichón volcano eruption in 1982, with a natural warming, which intensified the anthropogenic warming due to greenhouse gas emissions.

The cited references are:

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Muggeo, V. M. R. Estimating regression models with unknown break-points. Statist. Med. 22, 3055–3071. doi: 10.1002/sim.1545, 2003.

Reid, P. C., Hari, R. E., Beaugrand, G., Livingstone, D. M., Marty, C., Straile, D., Barichivich, J., Goberville, E., Adrian, R., Aono, Yasuyuki, Brown, R., Foster, J. Groisman, P., Hélaouët, P., Hsu, H.-H., Kirby, R., Knight, J., Kraberg, A., Li, J., Lo, T.-T., Myneni, R. B., North, R. P., Pounds, J. A., Sparks, T., Stübi, R., Tian, Y., Wiltshire, K. H., Xiao, D. and Zhu, Z. Global impacts of the 1980s regime shift. Global Change Biology 22, 682-703, doi: 10.1111/gcb.13106, 2016.

Rodionov, S. N. A sequential algorithm for testing climate regime shifts. Geophysical Research Letters 31, L09204. doi: 10.1029/2004GL019448, 2004.

13. L242 and L243: “Period” instead of “P eriod”

Authors response: Corrected.

14. Section 3.2: Again, my main concern regarding the analysis for the effect of clouds is that the number of cloudless days is too small. So, the results may be misleading. I don’t know if making the analysis for a particular time in the day (e.g. local noon?) could give more reliable results.

Authors response: Section 3.2 was modified, excluding the analysis of cloud free days. Now, in that section we discuss the long-term trend of visibility, as a proxy for aerosol optical depth and the number of foggy days. To separate each effect, we used visibility data recorded from 10:00 AM to 03:00 PM, as fog is more frequent early in the morning and can impact visibility.

15. Section 3.3 Since AOD and SSA measurements from AERONET are available at Sao Paolo since 2000, I suggest that they should be also used in the analysis. The AOD measurements could be even used to evaluate the MODIS product. The AERONET data will give more information relative to the fact that: “In the case of diesel fueled vehicles, the number of new registered vehicles in the São Paulo city increased from

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about 5000 in 2000 to more than 25000 in 2010, the year with the highest number of registrations"

Authors response: As explained previously, this part of the manuscript was removed.

16. L385 – 390: As the number of cloud-free days analyzed for each year is small, I do not think that the authors can be sure that AOD did not change.

Authors response: We agree with the referee and for this reason, we removed the AOD analysis.

Referee # 2: 17. General comments The paper discusses the long-term trends of downwelling solar irradiance at earth's surface in Sao Paolo, Brazil, one of the longest periods of such observations worldwide (1964-2016). The authors have identified in this dataset the well-known from other studies global dimming period of surface irradiance up the end of the 1980s. However, the brightening period that has been found in other locations over the world has not been confirmed at this station. The authors using other ancillary information, such as sunshine duration, cloudiness, diurnal temperature range, and days of fog have attempted to explain, to a certain degree, this unexpected behavior. The scientific questions addressed in this manuscript are well within the scopes of ACP. The innovation of the paper lies in the uniqueness of the dataset as well as in the use of measurements of additional geophysical parameters in order to test and explain their findings. The methods of data analysis are quite standard for this type of studies but could be improved, as suggested in the specific comments below, especially as far as it concerns the use of deseasonalized data to derive the annual means for the calculation of the trends. Overall, the paper is well structured and presented with adequate clarity, although there is room for further improvements. Most of the conclusions drawn from the results are supported by appropriate references. Generally, the language of the paper is adequate, but some parts should be be further improved to enhance readability. I have provided suggestions for some cases in the Technical Comments section, but there are more sentences that need fixing. Particu-

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lar effort should be put to the Conclusions section which seems to have been written hastily with and several sentences are difficult to read.

Authors response: We appreciated and thank referee #2 for the comments and suggestions. For the deseasonalized analysis, we made some tests as explained below (item 23) and observed only negligible differences. The manuscript was revised and we believe that the readability was improved in the revised version. Thank you for calling our attention to this.

Specific comments 18. Title: I suggest rephrasing to: Fifty-six years of Surface Solar Radiation and Sunshine Duration over São Paulo, Brazil: 1961–2016 or Long term changes of Surface Solar Radiation and Sunshine Duration over São Paulo, Brazil (1961–2016)

Authors response: The first suggestion was accepted, and the title was changed accordingly.

19. Line 17: Please include in the abstract some quantitative estimates of the trends in the two periods, at least for solar irradiation. The abstract is quite generic expressing mainly the intentions and not so much the findings.

Authors response: As suggested, we included quantitative estimates of the trends for solar irradiation and cloud cover in lines 28, 29, 31 and 32.

20. 47: The acronym SSR is defined here as surface solar radiation while later in line 88 is defined as surface solar irradiation. Please fix this because it is important to have a clear distinction between the two quantities.

Authors response: We removed the acronym SSR from line 47 and used it only when referring to surface solar irradiation, which is the variable analysed in the manuscript.

21. 89: The term “cloud cover fraction” (CCF) is more common in literature, and in essence, than term “sky cover fraction”

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Authors response: We changed the term, as suggested.

22. 118: Has the calibration of the instrument been monitored during the 56 years of operation? From the cited reference (1988) I understand that the 5% uncertainty characterizes the type of this instrument and does not include the uncertainty of the long-term stability of the instrument's sensitivity. Please discuss this in more detail.

Authors response: Yes, the 5% refers to the instrumental uncertainty. To verify the stability of the instrument, in 2014, a one-year comparison experiment was conducted with a brand new Robitzsch-Fuess Actinograph, type 58dc. Monthly scatterplots comparing irradiation data from daily measurements are available at http://www.estacao.iag.usp.br/Relatorios/Relat_tecnico_3.pdf (in Portuguese). Figure 1 presents the scatterplot of all daily measurements performed in 2014, comparing both instruments data. Bars (vertical and horizontal) indicate the 5% instrumental uncertainty, as pointed out by the referee. Applying a least square fitting to the data, the resulting slope is 0.919 ± 0.006. Assuming that the operational actinograph suffered a linear degradation throughout the years, from 1961 to 2014, and that, in the first year, the slope should be equal 1, the difference in slope resulted in a long-term trend of about -1.5 % per decade, ranging from (-1.6 to -1.4) % per decade. This agrees with the result by Plana-Fattori and Ceballos (1988) indicating that the hypothesis of a linear degradation can be considered. In order to take this long-term shift in the instrument calibration, we applied this linear trend to SSR data. This changed the results as presented in the Table below, particularly in JJA, for which the trends are now outside the 5% significance level. Table 2 (Table 1 in the revised version) of the manuscript was updated taking this correction into account as well as SSR in the Figure 2. We added this discussion as supplementary information to the manuscript.

Figure 1 – Comparison of one year of irradiation measurements performed with two Actinographs, the operational, whose data is discussed in the manuscript, and a brand new one. The blue line represents the least square fit with coefficients: linear = $0.30 \pm 0{,}07$ and slope = 0.919 ± 0.006. The red line is the 1:1.

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Table 1 - Modified Mann-Kendall trend test results for Period 1, from 1961 to 1983, and Period 2, from 1984 to 2016, considering each season and in an annual basis for the surface solar radiation (SSR) in units of kJ mˆ-2 per decade. Period 1: 1961-1983 Period 2: 1984-2016 Time interval Trend Z p Trend Z p Annual -0.40 -1.64 0.101 -0.39 -3.02 0.003 DJF -0.64 -1.05 0.291 -0.53 -2.56 0.010 MAM -0.76 -2.48 0.013 -0.25 -1.66 0.097 JJA -0.47 -1.93 0.054 -0.17 -1.87 0.061 SON -0.24 -0.89 0.373 -0.57 -2.40 0.016

23. 126: Annual averages are biased by the high summer values therefore are not representative for the year. I suggest using monthly anomalies (deviations from the long term monthly mean) and from them to calculate the annual means and derive the trends. This approach will probably alter the significance level of the trends.

Authors response: Only negligible difference was observed at the significance level of the trends if using the monthly anomalies. Please, see the example for SSR annual trends at Table 2 and compare with the results presented at Table 1. We attributed this to the low number of missing values. For SSR, the total number of missing days was 59 (from a total of 20454 days), the maximum number of missing days was five per year and four per month. Sunshine duration, maximum and minimum air temperatures presented no missing data and only two days in the entire series was missing for the cloud cover fraction.

Table 2 – Modified Mann-Kendall trend results using monthly anomalies data for SSR Period 1: 1961-1983 Period 2: 1984-2016 Time interval Trend Z p Trend Z p Annual -0.40 -1.69 0.091 -0.39 -2.99 0.003

24. 146: It is not clear whether the 9-day limit refers to each month (July to October) or to the entire 4-month period.

Authors response: It referred to the entire 4-month period. For this reason, we agreed with both referees that it was too low for a robust statistical analysis and decided to remove the clear sky analysis in the revised version of the manuscript.

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25. 148: Please clarify whether in the calculation of the atmospheric transmittance the solar irradiance (TSR) been adjusted for the variation of sun-earth distance.

Authors response: Yes, and we adopted the empirical formulas proposed by Paltridge and Platt (1976) to take that into account. We added this information in the manuscript to make it clear.

26. 153: As the station is located about 800 m above sea level, I assume that in many cases fog may occur below this altitude and on these occasions it would not affect the solar radiation measured and the station. Are these conditions distinguishable in the dataset?

Authors response: When fog is reported at the meteorological station, it is observed at surface level. Although the site is located at 800 m above sea level, fog can form due to radiative cooling at night. In effect, the mean elevation of São Paulo Metropolitan Area is around 800 m due to its location on a plateau, the Brazilian Plateau.

27. 154-155: Please clarify whether days with fog have been excluded from the clear sky averages.

Authors response: The clear sky analysis was removed from the revised version of the manuscript.

28. 155: Is the “fraction of cloud free days with foggy conditions” the FFD used in figure 3? If not, please explain how this index has been calculated. Figure 3 suggests that FFD can be as high as 0.8. Would this mean that in the particular year 80% of clear days are foggy?

Authors response: Yes. It means that in the particular year, from July to October, 80% of clear days are foggy. But as pointed by both referees, the low number of clear sky days (from 9 to 23) can result in this kind of artificial result, motivating us to remove this analysis from the final version of the manuscript.

29. 159: Table 1 could be removed because it does not add any information that is

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used in the analysis.

Authors response: We removed the table as suggested.

30. 187: Are the annual averages of the different variables computed for the common days of data or for each variable all available data have been included? This might influence the results in case of a large number of missing observations.

Authors response: As mentioned previously, for SSR, the total number of missing days was 59 (from a total of 20454 days), the maximum number of missing days was five per year and four per month. Sunshine duration, maximum and minimum air temperatures presented no missing data and only two days in the entire series was missing for the cloud cover fraction.

31. 196: In Figure 3 the upward trend in cloud cover does not extend to 1988 and ends in 1983. Is 1988 a typographical error or there is really a difference between the total cloud cover (this paper) and the trend of the two cloud types reported in Rosas 2019?

Authors response: We believe that the referee meant in Figure 2. Rosas et al. (2019) conducted the cloud cover database analysis considering visual observations from 1958 to 2016, and they evaluated the trends for the first 29 (from 1958 to 1986) and the last 30 years (from 1987 to 2016), and also for the whole time series. The increasing trend for stratiform cloud fraction of 4.8 % per decade and of 1.4 % per decade, in the case of cirrus clouds, were observed in the period from 1958 to 1986. Thus, 1988 was a typographical error. As explained in comment 12 to referee # 1, we separated the series in the 1983-1984 for the reasons discussed in the manuscript and which coincided to the trend shift detected in South America by Reid et al. (2016).

32. 274-275: Please clarify whether the threshold of 0.1 for the cloud fraction refers to the average of all measurement during the day or to each measurement during the day.

Authors response: It refers to each measurement during the day.

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33. 274-275: Please state how the limit of 9 cloud free days per year has been determined. Isn't it too small, representing only 2.5% of the available days? Is it related to the 2nd percentile representing the absolute maximum of the data?

Authors response: Yes, it is indeed too small. Cloud free days are rare in São Paulo, particularly in the afternoon. For this reason, we removed the analysis of cloud free days from the manuscript.

34. 280: July-October: Fig 3 caption states July-September. Which of the two is correct?

Authors response: The correct is July-October.

35. 297: Figure 3: It would be interesting to show how the DTR is behaving for clear-sky conditions.

Authors response: Due to the low number of data, we excluded the analysis for clear sky conditions.

36. 306: Visibility in 1963 is also quite low (possibly related to the Agung eruption?), which may have partly contributed to the reduction of SSR in this year.

Authors response: Yes, we believe that the low visibility in 1963 also contributed to the reduction of SSR in that year, but we are not sure if it is also related to the Agung eruption.

37. 325: I don't understand what is meant by "the AOD exceeds 2 sunshine duration recorders". Please rephrase.

Authors response: This part of the manuscript was removed.

38. 328-329: I cannot understand why effects on sunshine duration will be stronger when most of radiation is in the diffuse component. I would expect the opposite, i.e. that under prevalence of diffuse radiation the sunshine recorder would be less sensitive and effects of fog would not make any difference.

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Authors response: This part of the manuscript was removed.

39. 341-350: This section discusses the heat island effect which is not relevant to trends in cloud free irradiation. It would better fit in the next section where it could be connected to temperature changes and DTR or in the introduction.

Authors response: The discussion was moved to the next section, in the context of temperature changes.

40. 370: The increasing trend in the daily minimum temperature is indeed qualitatively in line with increasing cloudiness, but the latter is very small and insignificant (Table 2). The heat island discussed for the fog trend should have also played a role in the temperature trend.

Authors response: As mentioned in the previous comment, we discussed the urban heat island effect in the context of temperature trends.

41. 381: Figure 4: It would be interesting to show how these variables behave for clear skies only. A second set of lines with clear-sky values could be added with different color or symbol.

Authors response: As the number of clear sky days was too low for a statistically significant analysis, it was removed from the manuscript as pointed by both referees.

42. 385-390: Please try to split this long sentence in to two. It is difficult to read.

Authors response: We removed this part of the discussion in the revised version of the manuscript. The effect of aerosol on SSR will be analysed in a future study, when more data related to aerosol properties could be gathered.

43. 428: Please state the wavelength of the aerosol optical depth data.

Authors response: Text removed from the revised manuscript.

44. 442: Does the SSA from AERONET show any trend after 2000? What about the

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AOD from this instrument?

Authors response: Text removed from the revised manuscript.

45. 433: The variability of the AAI and the AOD cannot be compared in absolute terms because these two qualities are not the same.

Authors response: Text removed from the revised manuscript.

46. 436: From Figure 5 is evident that there is an abrupt change in AAI between 1992 and 1994 which should not be neglected. The AAI after 1995 has been almost doubled and remained fairly constant. Considering the years 1984-2016 as one period for a trend is probably not a good choice since the trend is not linear.

Authors response: Text removed from the revised manuscript.

47. 470: “their distinct patterns”. If I understand correctly, the other factors may have affected the SSR and not the SD and DTR, thus it should be changed to “the distinct changes in SSR”.

Authors response: At this point we meant that other factors may have affected SSR (such as aerosol optical depth, analysed by the variability of visibility data), SD (number of foggy days) and DTR (urban heat island effect and anthropogenic greenhouse gas emissions).

48. 503-506: See my comment for line 436 above.

Authors response: Removed from the final version.

49. Technical 30: Delete “still” - OK 34: Replace “encouraged” by “planned” - OK 55: Insert “comprising” before “both”, and delete the two occurrences of “in” - OK 67: Replace “the” by “increasing” - OK 92: Replace “propose to answer” with “are addressing” - OK 95-97: Better use “section” instead of “part” - OK 148: Insert “by” after “estimated” - OK 150: Delete “also” - OK 182: Please define that the dry season is July-October. As previously discussed, the analysis of the aerosol effect was removed

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from the manuscript. 242-243: There is a long blank after the P in word Period (two occurences) - OK 270: Replace “solely” with “sole” – this sentence was removed from the revised version of the manuscript. 271: Replace “clue” by “quantitative estimate” – the text was removed from the revised version of the manuscript. 275: Please add after “spring” the months corresponding to winter and spring season, just to avoid confusion for the readers living in the northern hemisphere. – the text was removed from the revised version of the manuscript. 291: Delete “decade” – the text was removed from the revised version of the manuscript. 292: Replace “mention” by “mentioning” – the text was removed from the revised version of the manuscript. 293: Replace “n/N” by normalized sunshine duration” – the text was removed from the revised version of the manuscript. 331: Delete (FFD) as it is has been already defined – the text was removed from the revised version of the manuscript. 333: Please replace “scenarios” with “conditions” – the text was removed from the revised version of the manuscript. 339: Replace “decay” with “reduction” since decay usually implies a gradual decrease but here we have a rather abrupt change. – the text was removed from the revised version of the manuscript. 357: Replace “it” by “DTR” - OK 360: Please add after “space”, “during daytime” and after “surface”, “during the night” - OK 400: Delete “jumping” - OK 401: Add “and” before “decreasing” - OK 423: Replace “relative” with “relatively” – the text was removed from the revised version of the manuscript. 431,432: Something is missing in this sentence. – the text was removed from the revised version of the manuscript. 434: Please replace “1980 and 1990 decades” with “in the 1980s and 1990s”. – the text was removed from the revised version of the manuscript. 471: Delete “a restrict analysis of” – OK. 475-476: Delete “is a potential candidate to” - OK 477: Replace “Although” with “However,” – the text was removed from the revised version of the manuscript.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2020-848/acp-2020-848-AC1-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-848, 2020.

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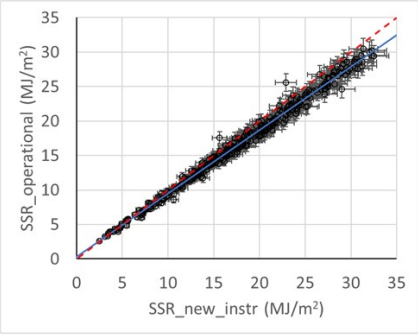

Fig. 1.

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