

Interactive comment on “How long do satellites need to overlap? Evaluation of climate data stability from overlapping satellite records” by Elizabeth C. Weatherhead et al.

Anonymous Referee #2

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General comments

This manuscript examines some challenges affecting the creation and error assessment of long-term climate-related data records, and provides some guidance in terms of the number of overlapping years between two satellite measurement systems for a given overall stability verification goal. The authors are generally well-versed in the subject matter and have useful insights to offer in this domain. The methodology is sound, especially as they make it clear that there is no "one size fits all" approach either, with so many possible variables in terms of potential instrument issues (jumps, offsets, drifts,...) as well as geophysical parameters and variations (both spatial and temporal). In this sense, given the somewhat limited case that is discussed (as this is

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most directly relevant to solar irradiance type data), the results should be viewed really as applied to these very specific conditions. Would the authors agree/disagree or care to comment further (see also the next comment)?

However, I would argue that the example used is somewhat limited given the broader interest from the atmospheric community in terms of atmospheric data (temperatures as well as composition), and the larger range of variabilities (spatial and temporal) tied to such observations. Can anything useful not be mentioned in this regard, even for a somewhat specific case (such as ozone or water vapor data) using two "state-of-the-art" monthly zonal mean data sets? Is the feeling that the overlap period needs would be longer, shorter, or about the same, even if this is based on a semi-quantitative approximate analysis (or why can this not be "readily added"?). I suspect it is in fact somewhat complicated and (clearly) quite dependent on the data sets themselves, but it would be good to add some words to this effect, if at all possible, even if a "full-blown" analysis is not performed for this other case of even broader interest. The value of this work is limited because of the limited example, although this is clearly useful to the solar irradiance community.

I also believe that one should never lose track of the fact that even if one can consider it valid to require (as a minimum) two years of overlap between satellite measurements in order to verify and "guard against" (if possible) potential relative drifts or jumps, the overall (longer-term) satellite record still depends very much on the years prior to and following the overlap period, for which independent observations (or reference calibration) are still required to check stability over the long run. In other words, current stability does not guarantee future stability (or even past stability). The authors might consider adding such additional words of caution, although there are generally enough cautionary sentences in the manuscript that this criticism is a fairly soft one; possibly this is more a question of separating the cautionary portions in a slightly cleaner way, as there is some overall inefficiency in the presentation and clarity (too many words or sentences in some places).

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Overall, I would recommend publication of this work as useful knowledge for readers interested in careful planning and evaluation of long-term observational (and merged) data records. Whether this manuscript is appropriate enough for ACP, or possibly better suited for a different Journal, is something that the editors should probably ponder further, but I would not be strongly opposed to this manuscript finding a place in ACP (although AMT may be a better fit), after some further consideration of the reviewer comments (mostly the general comments). I also do agree broadly with the other review of this manuscript, submitted shortly before this one.

Specific comments (mostly minor comments for added clarification and a number of editorial details)

Page 1, Line 19 (P1-L1): maybe change slightly to "Satellite sensors can enable unprecedented understanding of the Earth's climate system by providing measurements of incoming solar radiation, as well as both passive and active...and temporal coverage." [the last part of this long sentence seems superfluous - this is clearly why satellites are outstanding in the aspects mentioned]

P1-L33: delete "may" before "occur".

P1-L36: change "are" to "is".

P2-L30: This paper presents techniques that can address...

P2-L44: "showed that ..."

P3-L43: While this manuscript focuses mostly on solar irradiance records, there are some selected refs. to other parameters and ozone, for example. For the more general reader, adding at least a WMO (2014) reference for such studies seems appropriate here (at least) [and maybe also "and references therein", given the non-negligible amount of work that has gone into merging various data records of atmospheric temperature and composition]. A more careful consideration of various other refs. could be considered, although this is not central to this manuscript's relevance, given what is

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discussed already with a few other refs mentioned.

P3-L49: I am not sure that polar ozone loss is generally viewed as such a "major environmental concern" for the decades ahead [but if the consensus exists among this small subset, no major problem in keeping such strong words I suppose].

P4-L26: probably requires the word "is" at the end of the sentence.

P4-L39: Another ref. worth mentioning, it would seem, is the Hubert et al. [AMT, 2016] work on relative drifts between a large number of ozone time series measurements, somewhere in this paragraph, for example. ["and references therein..." at the very least].

P4-L44: delete "same" before "problems".

P5-L3: add "this" before "is often beyond".

P5-L7: replace "efforts" by "effects" (?)

P5-L23: As mentioned in my general comments, the "final data products" cannot just depend on the overlap record... A "best check", maybe, but not if the overlap period is so short compared to the full record time period (and some records extend for more than a decade).

P5-L36: "are applicable to" could be reworded as "may be useful for".

P6-L25: "such criteria are impractical".

P7-L6: "contains all three uncertainty sources identified in this study."

P8-L27: I suggest deleting "as is".

P9-L3: change "difference" to "differences". Also on line 8.

P10-L12: To get to equation (2) from eq. (1), you should specify where the 1.96 comes from, as this is not just a simple inversion of (1). The text in the paragraph that follows carries some of the information needed, but a more crisp description (or sentence

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before equation (2)) would be much better.

P10-L29: “not appropriate”? Clarify what this means here please.

P10-L32: “users should never ignore the added uncertainty” - not very clear what this means here; is the “added uncertainty” not basically built into the merged datasets as a result of the merging process, so either the variability changes somewhere or there are small steps/jumps, or “built-in” trends that differ from any individual dataset to some extent. But when one looks into the trends using the merged dataset, one gets a result that carries an uncertainty, e.g. by using formulae or methodologies suggested by the authors (or other past work). Do you mean, following the text until the end of this section 3, that users should try their best to “reduce the uncertainty in the overlap adjustments”? This is also something that is often a “given”, with little recourse besides independent confirmation using another dataset, although there are cases where some other methods might be used, as the authors mention. Maybe one just needs to say something like “users should be aware of added uncertainties. . .” and “try to minimize such effects, whenever possible” [and give one or two specific examples where this can be done].

P10-L48: If I use a drift factor in equation (3) that changes by a factor of 2, the resulting ratio for number of months would seem to go as $(1/2)$ to the power $2/3$, or a factor of 0.63. If I use 0.63 times 3 years, I get 1.9 years, which is close to the ratio in Fig. 3, but not exactly a factor of $2./3$. (0.67). Could you clarify this further or point to equation (3) specifically when discussing Fig. 3 to give the reader a clearer explanation for how the curve changes. One needs 5 years or so to get another factor of two drop in the drift value (or drift error reduction).

P13-L7: delete “the” before “a drift”.

P14-L15: change “produces” to “produce”, as “the jumps” refers to the subject; also I suggest changing “on” to “of” before “Fig. 4”.

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P14-L30: add a space before “represented”.

P16-L8 to L11: Not only can ground-based and in situ observations help during the overlap period, but they can also help by checking the longer-term trends including those for the merged datasets; this is actually a chance to give a nice “plug” for the usefulness of such independent observations rather than just limiting them to an overlap period, in my view - not that the intent was to limit their usefulness, but why not be more inclusive?

P16-L12: “efforts have been put forth” sounds better (for example).

P16-L37: Delete the period after “standards”.

P16-L40/41: I would shorten the English here, e.g. “Figure 3 shows that drift detection accuracy improves as a function of the number of overlap years. Improvements in drift detection capability decrease as the number of overlap years increases, but the optimal overlap duration is difficult to identify.”

P16-L46: Instead of “may be proposed to improve”, I would say “have been proposed and implemented. . .” [this is the case].

P17-L1-L23: I found this discussion somewhat long, although it is not that long compared to the full text - at least it is of less interest to some readers for a publication intended for atmospheric chemistry and physics (with solar radiation included).

P17-L18: I would add a comma after “satellite data”.

P17-L29 to L34: Again here, I would prefer to see this as an opportunity to present ground-based observations as a cost-effective alternative, or certainly a complement, to satellite data (for certain applications, at least). The “potentially high cost” (line 34) comes more from the satellite observations, in comparison to ground-based networks (for atmospheric composition applications in particular).

P18-L10: “achieved due to drift . . .” needs some improvement, maybe “achieved by

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considering that drift is inversely proportional to the number. . .”.

P18-L17: Again, I would add something here regarding ground-based, not to consider satellites as a final word for everything. . . e.g. “as observational approaches, both from satellites and from the ground, are considered. . .” This is true even if your main goal here is to provide some quantifiable basis for satellite overlap goals and methodologies, regardless of the additional input potential from non-satellite data.

Appendix A: This one could be summarized in a few sentences since the results of monthly versus 27-day period averages etc. . . are so similar for both cases. If you insist on keeping it, it could be dramatically shortened (which is why just adding a sentence or two in the main text seems appropriate to me - if at all), there are minor typos. L5, change “word” to “world”, L6, delete “the” before “June”, and the solar SIM std. dev. number needs an exponential format type (superscript “4”) like the other numbers.

Appendix B: On L10, the “with” does not need to be capitalized. More importantly, the Fig. captions should describe the Fig. contents, y and x axes (unless it is plainly obvious), and not add lines of comments that really belong in the text/discussion (and might be duplicated unnecessarily). In Fig. B2, the caption should also mention what the dashed blue lines represent instead of mentioning what this Figure “supports” (do this in the text). For Fig. B3, in addition, the “theoretical quantities” are too much of a mystery, even with refs. that the reader could check out, so adding a sentence or two to actually describe what is being plotted would be very beneficial, or give examples if there are really too many different quantities. . .

Appendix C: L6, please correct the reference to the plot and Figure number (no third plot in Fig. 2); also, using the word “visually” seems unnecessary. On line 21, I would change “the” to “a” before “signal-to-noise”, and add “that” before “is slightly”. On L27, I would delete “we can imagine that the detection of” and just say “this drift, . . .”, and L29, I would add “it” before “would vary”. On Fig. C1 caption, delete “the” before “even small drifts”, on L4, add “the” before “width”, and on L5, change “time” to “times”, and add I

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suggest “and the probability is indicated. . .”. On L11, change “directed” to “detected”. L13-14, I suggest “of detection with more years of overlap.” On the question of scale for the shaded/striped blue or green areas, this scaling is arbitrary it seems, not something that can be checked against the y-axis scale; is the maximum close to 100%? It may be worth clarifying this scale range as the y-axis does not help (or one could add a separate axis on the right side of the plot).

Appendix D: this one is pretty nebulous for those not close enough to statistics from the economics side (most readers of ACP or AMT), and I suppose this could be shortened a bit as well, especially as it is not the main goal of the manuscript (which is why after all, this is in an Appendix). Also, it sounds quite vague given the comments on lines 33-34, and/as the actual specifics are tough to provide reliably for the case at hand, let alone the (little mentioned) range of possible applications in the atmospheric sounding domain. Minor detail, footnote (4), there is a closing parenthesis missing after “(see Morss et al., 2005. . .”. Also, the typesetting for the mathematical quantities seems off in some cases (notably for q on lines 16 and 20). On L4 of page 25, I would change “use along” to “use of” or “using”, or something else to clarify this.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1156, 2017.

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