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Atmospheric Chemistry and Physics Discussions

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 #1

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General comments

This paper examines the issue of quantifying differences in satellite measurements sets based on overlapping measurement periods, addressing the question of how long overlap periods need to be to accurately estimate offsets and drifts between instruments. A few general formulas are presented to calculate required overlap periods for given desired precision requirements of offset and drift estimation, and examples are presented.

While the issues discussed are definitely relevant to the construction of long-term atmospheric data records, this paper does not actually directly deal with any atmospheric measurements, instead focusing on examples based on satellite measurements of

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Discussion paper

solar irradiance. As a result, the fit between this paper and ACP (or even AMT for that matter) is somewhat questionable. Readers of ACP would likely benefit greatly from examples using actual atmospheric data, and given the ready availability of multi-instrument data sets like stratospheric ozone (e.g., Tegtmeier et al., 2013), it wouldn't be hard to include such examples. Of course, atmospheric data, with the temporal and spatial variability that comes with it, may present some additional complications to the analysis (which is mentioned in passing in the manuscript), but a discussion of these complications seems warranted in such a paper if it truly wants to address the analysis (and merging) of atmospheric data.

The utility of this paper to atmospheric community could also be improved by a fuller description of the general implications of the analysis before descending to the focused solar irradiance example. For example, Fig 3 displays the detectable drifts in the solar irradiance data sets as a function of years of overlap, which suggests a general form of the solution, but won't provide any quantitative information to anyone working with other data sets. Instead, a plot of ratio of drift to variability as a function of n (perhaps for different sample values of autocorrelation) would be directly relevant to users of other data sets.

Stylistically, I found the paper repetitious in places, often returning to discussions of issues that aren't, in my opinion, of central importance. For example, the issue of requirements (or the desire) for self-calibrating, consistent systems for atmospheric measurements is often brought up, but this paper deals specifically with techniques to deal with situations where measurements are not self-calibrated. This point can be made succintly in the introduction, and thereafter neglected, at least until the discussion and conclusions.

Specific comments:

Pg 1, l27: "offset or a drift in the offsets": If "offset" is singular in the first case, then "drift in the offset" seems more appropriate. But the sentence was confusing to me at

first, and I wonder if just “drift” is easier to understand.

Pg 1, l37-38: “may also benefit. . .” this issue is not dealt with in any substantial way in the paper, so this statement’s inclusion in the abstract seems superfluous.

Pg 2, l16: “tying data to absolute reference standards with the intent of developing traceability to reference standards” sounds a little tautological.

Pg 2, l17: It’s not clear to me why reference standards are brought into the argument here, is the point that if one of the two overlapping measurement sets is a standard, then you can extend a standard through identification of an offset and drift in the second instrument?

Pg 2, l36: Do “wavelength scale corrections” etc. really help instrument scientists understand the fundamental observations? Or does an understanding of the fundamental observations allow for valid corrections?

Pg 3, l15: Does removing a bias affect the precision of the merged data set? And, does one really need to remove a bias to identify a drift? If you look at changes with time (time derivatives) the absolute value doesn’t matter.

Pg 3, l28: It would seem that the paper is of interest to a wider group than just the users of merged data sets, specifically to the creators of merged data sets.

Pg 3, ll40: The first two paragraphs of Sec 1.1 have no apparent specific connection to “Offsets”, and seem to set the scene for an analysis of ozone data which never arrives. Actually, there doesn’t seem to be much of any specific introduction of the issue of offsets in this subsection.

Pg 3, l32: “but this will not. . .” If two measurement sets were both traceable to a reference, why wouldn’t this fully address the challenge of merging the data sets? Is the calibration referred to here only at a single time, or could it be continuous?

Pg 3, l36: Temporal changes in sampling can also contribute to drifts.

Pg 3, l39: If drifts in ozone were up to 5%, but were statistically insignificant, then the case of ozone seems to be very different than that of solar irradiance.

Pg 5, l9: A clear definition of "jump" is needed: I assumed it to be the instantaneous addition of a constant offset, but if a jump "can last from less than a few hours to multiple years" it sounds more like a two-step process.

Pg 5, l15: Why is the requirement for a long-term stable record difficult to justify?

Pg 10, l7: "better behaved" is not very helpful: this sentence doesn't explain what monthly data is better than.

Pg 10, l12: Given that the example below gives a case in which "1.96" is not the valid multiplier, it would seem appropriate to replace "1.96" in equation 2 with a placeholder variable for the student-t distribution (as a function of n). Otherwise, a quick reader may overlook the fact that 1.96 holds only for large n.

Pg 11, l8: "we can increase our measurements per month" in this example case, but not in all circumstances. I think the point is that with enough measurements, the random measurement errors in the mean are small enough to ignore, the only source of variance is the natural variability.

Pg 13: l1: A short derivation of Eq. 4 would be useful here if possible, otherwise the term introduced to account for the jump is not intuitive.

Pg 13: l6-8: These sentences talk about fitting of the offset and drift, simultaneously and sequentially. However, to this point there has been no discussion of "fitting", only using the equations to estimate the length of time needed to estimate an offset or drift. How dos the concept of fitting, simultaneously and sequentially, affect the use of Equations 2-4?

Pg 16, l4: Is a reference really needed to support the statement that "Earth observations often invoke spatial and temporal variations"?

Pg16, l17: An error in the drift which is half the trend one is seeking to detect seems large: does it mean that in a worse-case, the detection of the trend might take twice as long (as the case with no drift in the measurement?). How was this threshold decided?

Pg 16, l42: Given only Fig 3, the optimal is obviously as many years as possible. The optimality issue only is apparent when you consider the costs (which are discussed below).

Pg 17, l16: First, a subjective evaluation ("nice") of the work of Morss et al. (2005) is probably not appropriate here, and secondly, there's not much in the sentence to really inform the reader of the relevance of this work to the present study: "a case study based on primarily hypothetical valuation estimates" doesn't help much.

Editorial comments

Pg 1, l33: delete "may"

Pg 1, l 36: either "Extensions . . . are" or "Extension . . . is"

Pg 2, l2: "assess the stability" relates to identifying and quantifying drift, but much of the paper deals also with identifying and quantifying offsets, so I wonder if this first sentence of the paper should be more general.

Pg 2, l8: The "sensitivity degradation mechanisms" described in the prior sentence will firstly impact the individual satellite record, not just the merged record referred to here.

Pg 2: l28: "Should an offset. . .", This sentence joins two statements with a semicolon, but it's not clear how or why the two statements are linked.

Pg 3, l43: why "potential"?

Pg 5, ll35-40: Some pretty general material here which seems repetitive to the introduction.

Pg 6, ll9-10: Intra-sentence repetition.

Pg 6, l23: deploy->deployment

Pg 6, l26: “Here we are. . .” has been explained before.

Pg 10, l10: Here and elsewhere, “years” are discussed in the text, while the equation is written in terms of months.

Pg 10, l36: why brackets around “%”?

Pg 10, l47: The sentence which includes the equation seems to not quite make sense.

Pg 11, l2: do you not specify the drift, rather than “estimate” it?

Tegtmeier, S., Hegglin, M. I., Anderson, J., Bourassa, A., Brohede, S., Degenstein, D., Froidevaux, L., Fuller, R., Funke, B., Gille, J., Jones, A., Kasai, Y., Krüger, K., Kyrölä, E., Lingenfelser, G., Lumpe, J., Nardi, B., Neu, J., Pendlebury, D., Remsberg, E., Rozanov, A., Smith, L., Toohey, M., Urban, J., von Clarmann, T., Walker, K. A. and Wang, R. H. J.: SPARC Data Initiative: A comparison of ozone climatologies from international satellite limb sounders, J. Geophys. Res. Atmos., 118(21), 12,229-12,247, doi:10.1002/2013JD019877, 2013.

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