Authors response to prof. Eric A. Smith.

Overview

There is very little to recommend this paper -- it has three very fundamental problems: (I) there is little new scientific knowledge herein; (II) there are multiple points where the information presented by the authors is at best, imprecise, while at worst factually incorrect; and (III) there is an obvious lack of familiarity with the foundation literature that precludes the analysis and in which the most salient of this literature has already accomplished virtually all that the authors have unknowingly duplicated. (IV) In addition, even if there were something new, the scientific strategy behind the analysis is weak and ineffective. (V) Finally, the paper has numerous technical shortcomings in the way of stilted English usage, misuse of articles, spelling errors, sophomoric tutorial excess and inclusion of irrelevant diagrams. In essence, it is a carelessly (or hastily) prepared paper. It also might be presumed that this analysis was carried out as an excuse to exercise an orbit analysis software package [SGP4 embedded in PREDICT] rather than to make a serous attempt to understand some new aspect of precipitation retrieval error due to sampling limitations. In my opinion, a herculean effort would be required to transform this manuscript into a publishable paper. [Prof. Eric A. Smith]

We are thankful to prof. Eric A. Smith for the critical review. Many points he raises are valid and we do agree with the referee that the major criterion for any publication approval is the new scientific knowledge and surely any scientific research has to take into account all available knowledge on the subject.

However we do not agree that our study does not present any new scientific knowledge and we feel that the aim of our study was not fully clear to the referee. The scientific value of this study is in the comparison of sampling error estimates for a large number of satellites with different orbital parameters and viewing geometry under realistic conditions. We think it provides a reader with a useful overview of the differences in rainfall sampling ability of the various satellite constellations (more on this in response to specific comments).

We would also like to note that it was our intention to keep the number of references low thus not every paper that prof. Smith cites is mentioned in our manuscript, while we were aware of most of them. However many of the author names the referee cites appear in our list of references and many of the recommended papers can be easily found via the cited material.

The main motivation to use the SGP4 model was to minimize the effort in acquiring satellite overpass information. For DMSP satellites no complete record of orbital information is available, so we resolved to extracting sub-satellite tracks from the archived satellite records, which was a tedious task and should be avoided whenever possible. The SGP4 performance is well known (as indicated in section 4.3) and itself does not present any special interest for our analysis.

We believe that our methodology and results are sound and publishable after implementing corrections in the revised version of the manuscript. Our response to the specific comments is presented below.

Major shortcomings.

1. The topic of the paper has been examined extensively with almost nothing new being considered in the analysis. Moreover, whatever is new has not been identified.

The newness of the study is summarized in the introduction section, P.11680,L12-20.
The study aim is to compare sampling uncertainty for different satellite constellations with different instruments on-board relying on the precise satellite track information, e. g. NOAA and DMSP satellites. To our knowledge no such study has addressed this issue so far, except for the Iida et al (2006) which relies on a simplified orbital model that does not include effects of orbital drifts and considers only TRMM, AQUA and three DMSP satellites. Our study includes a larger group of satellites, in addition to DMSP, AQUA and TRMM satellites we also consider MetOP-A, 5 NOAA satellites as well as geostationary observations at synoptic times. The methodology we used also takes into account the orbital drift effect which may increase biases in the areas with strong diurnal cycle of precipitation. However we do not try to quantify the orbital drift effect separately. One example how our results may be helpful is that it provides a reader with additional information which data source is better (if different at all) in terms of capturing rainfall events.

We must mention that the main scope of our study was not to deliver new fundamental knowledge of the sampling uncertainty in global satellite rainfall estimates as such, but rather to identify the differences in performance of various satellite constellations under most realistic conditions and especially in regions with different precipitation regimes.

**2. The authors are apparently unaware of the salient literature addressing this topic. For starters the foundational studies of Salby (1982a-b, 1988/JAS; 1989/JCLIM) concerning asynoptic satellite observations and the review article by Arnold & Dey (1986/BAMS) concerning the history of how OSSEs were designed would have been essential citations in setting the stage for their analysis. Then, more up-to-date articles concerning satellite sampling strategies and OSSEs would have greatly helped them understand the critical issues in studying how incomplete satellite sampling introduces errors in mean values, e.g., Kirk-Davidoff et al. (2005/JCLIM), Lahoz et al. (2005/QJRMS), Stoffelen et al. (2006/QJRMS), Gelaro & Zhu (2009/Tellus), Sugimoto et al. (2009/MWR) and Matsutani et al. (2010/JGR). However, and most importantly, had the authors become knowledgeable of the studies of, e.g., McConnell & North (1987/JGR), Bell et al. (1990/JGR), Salby et al. (1991/BAMS), North et al. (1993/JAM), Bell & Kundu (1996/JCLIM), Morrissey & Janowiak (1996/JAM), Steiner (1996/WRR), Astin (1997/SG) and Huffman (1997/JAM), they would have fully understood how exhaustively their chosen topic had been examined -- even before the post-TRMM-launch rash of articles in the first decade of the new century coming from such authors as Arkin & co-authors (particularly Sapiano & Xie), Ebert, Gottschalck, Huffman and co-authors, Joyce, Levizzani, Soorooshian and co-authors, Turk and very recently Fischer and Wolff (2011/JAMC) that have essentially put to bed the topic of how multiple satellite sampling affects precipitation retrieval errors from space. They also demonstrated a lack of knowledge concerning the topic of rainfall diurnal sampling from satellite by ignoring such important studies as, e.g., Bell & Reid (1993/JAM), Soman et al. (1995/JAM), Negri et al. (2002/JAOT), Janowiak et al. (2005/JGR), and Yang & Smith (2006/JCLIM).
I have drawn attention to these papers (there are others) to help the authors understand that it**

was their responsibility to carry out a thorough literature examination before attempting to publish a paper in a reviewed journal -- for the specific purpose of determining, a priori, if their intended research was new or simply redundant. They would have found that what they were doing was essentially, NOT new, nor would it have led to new knowledge -- which is the fundamental test for publication approval.

We thank prof. Smith for the extensive list of literature. It is true that some of the papers are new to us (particularly those that consider observing system simulation experiments (OSSE), probably because only some of them are directly relevant to the satellite precipitation community). The major purpose of OSSEs in the cited studies is to estimate the effect of assimilating new parameters or brightness temperatures observed by particular satellites on the prediction skill of the numerical models. They typically consider satellite information to be global, gridded and available at synoptic times. It would be certainly interesting to compare the effect of assimilating the observations that come from different satellite constellations in NWP, however this would be far beyond the scope of the present paper. As it is stated in Lahoz et al (2005), setting up a proper OSSE may be considered as the task of the same difficulty as writing an assimilation framework itself, a task far too complicated for our purpose.

Concerning the rest of the papers that address the issue of sampling uncertainty directly – we were certainly aware of the most of the literature that prof. Smith cites, but did not include it to keep the number of references low. Wherever possible we tried to cite only recent publications by the same authors (e.g. Steiner et al (2003), Bell and Kundu (2000), Huffman et al (2007), Shin and North (1988), Wolff and Fischer (2009), Turk et al (2009), etc) and readers can easily find the rest of the literature from there. We also think that while it is true that satellite precipitation sampling uncertainty has been studied very extensively during the last two decades, most of the studies have somewhat different flavor compared to ours and are often based on idealized assumptions for the satellite sampling patterns. To name a few Shin and North (1988) and Steiner et al (2003) developed methodology to estimate sampling error based on number of observations as well as temporal and spatial aggregation scale, Bell and Kundu (2000) estimated sampling uncertainty on the monthly scale for three regions over ocean. Fischer and Wolff (2011) conducted an interesting study where they have derived sampling uncertainty for various satellites including DMSP and NOAA via disentangling the sampling term from the total rainfall product uncertainty using statistical error decomposition method described in Fischer (2007).

Our analysis provides an overview of the differences in rainfall sampling for various satellite systems and their combinations. We purposely conducted it for the regions with different precipitation diurnal cycle to illustrate at which temporal scale the differences in sampling uncertainty between these regions start to diminish. Our analysis is aimed at the people who need such an overview, e.g. members of hydrological community who work with rain-gauge observations at the regional scale and consider using satellite information for evaluating their results.

We will elaborate the topic of rainfall diurnal sampling from satellite using the references cited by prof. Smith. We did not do that until now since we do not try to estimate the effect of the orbital drift on the sampling uncertainty.

We will improve the introduction section of the manuscript to provide a reader with more comprehensive overview of existing knowledge on the topic and elaborate more clear the novelty of the present study.

3. Assuming there was something new in their study, by limiting their analysis to two small and arbitrary study areas within West Africa and Germany (for which no explanation or justification is provided) renders their conclusions of little value, especially outside the small study areas themselves where rainfall diurnal cycles, seasonal cycles and annual cycles undergo large variations, and for which those variations are greatly different from the counterpart variations within the two study areas. I also draw attention to the authors disregard of retrieval errors in their study, particularly those associated with geo-IR which are insensitive to rainfall altogether -- while at the same time implying that 2nd order orbit drift effects represent an important source of sampling error.

We picked the areas in West Africa and Germany for the following reasons:

a) as described in the section 3 (P. 11683, L.3-9) and illustrated in Fig. 3 there are significant differences in precipitation regimes of Germany and Benin. In Germany most of the rainfall falls in the form of light precipitation coming from the large scale long living precipitating systems while in Benin the largest contribution comes from the short-living convective cells during raining season which is only few months long.
b) another reason for choosing these areas is the availability of the ground-based observations. It is difficult to find original, not aggregated ground based observations required for our analysis which have appropriate temporal resolution (< 1h). So we have limited our research area to Germany and Benin, where data can be acquired relatively easily from the German weather service and AMMA-CATCH project. Since the data from these regions satisfy our criteria of different precipitation regimes (convective vs stratiform precipitation) we did not extend our focus area to any other location. This study should be therefore considered as a test case that could be extended to other regions where appropriate in situ data is available. We also did not want to resolve to using rainfall estimates that come from the NWP models in order to to avoid possibility of introducing any additional sources of uncertainty.

It is not completely clear to us why geo-IR retrieval errors would be important for our analysis which concentrates on the sampling uncertainty exclusively . The simulated GEO observations (the word “simulated” may be slightly misleading here as we simply used rain-gauge observations at the regular 3 hour intervals to reproduce geostationary measurements) are added to provide information of what amount of rainfall would be observed by geostationary satellites. We would appreciate if the reviewer could detail what he means.

Orbital drift effect may increase biases, especially when using rainfall estimates that come exclusively from the LEO observations. As illustrated in the Fig. 1, during two years the mean overpass time of a single LEO satellite may shift by ~90 minutes, which can be important for the areas with pronounced precipitation diurnal cycle. We did not look at the effect of the orbital drift separately, but we will address this issue in the future analysis.

4. There are numerous instances where the information provided by the authors is either imprecise or factually incorrect. A non-exhaustive set of examples are: (1) P-11679/~L19-15: although the GPCP, TMPA and CMORPH procedures use a great abundance of satellite precipitation observations, they do not use “all” available satellite precipitation observations;

We agree, the datasets mentioned utilize not “all” available satellite information but information from a “large variety” of satellites. We meant the latter however used a wrong wording. This is to be fixed in the revised version of the manuscript.

(2) P-11681/~L5: this discussion concerning point measurements is irrational in the context of beam filling error and 1- or 2-dimensional times series;

We accept this correction and will remove the sentence completely.

(3) P-11681/~L15-25: beyond the unnecessary tutorial aspects of this discussion and the fact that you now bring up retrieval error after immediately prior disregarding it as a component of your analysis, the equation on line 20 is incorrect since the covariant terms have been left out;

As it is very well described by Fischer and Wolff (2011), the covariant terms can be left out under the assumption that observed and estimated rainfall are independent of each other. We can think that in our case they are independent, but in perfect agreement, since we imagine our “retrieval” process as error-free.

We will provide a better explanation that includes reference to Fischer and Wolff in the revised version of the manuscript.

(4) P-11682/L11-12: these two sentences are self contradictory and you cannot just “think” yourself into use of hourly data without analyzing whether it serves the purpose, particularly after just claiming that 5-min data are essential;

As indicated by the referee #1 this issue is addressed in the paper by Villarini and Krajewski (2007). Their results show that the disagreement between the satellite and rain gauge observations is the smallest when the rain gauge observations are aggregated over the period from 90 to 120 minutes. Thus we think it is a sensible approach for our analysis to start with the hourly observations. We will clarify this point in a revised version of the manuscript.

(5) P-11683/L16: should say “approximately constant”; (6) P-11683/L18: “7” should be “6.9”; (7) P-11683/~L25: this statement is untrue -- there are multiple reasons associated with why MW measurements have mainly been made on satellites using LEO orbits, including cost limitations, antenna size allowances, use of real instead of synthetic aperture antennas, design of receiver gain electronics -- and other reasons associated with limited dwell times;

We agree and thank prof. Smith for this corrections 4.5, 4.6 and 4.7. We will modify the revised manuscript accordingly.

8) P-11684/~L15: definitions are not consistent as to use of term “LEO satellites” / it is not clear whether fourth group considers one or multiple GEO satellites / analysis should include group using only conical scanners;

It is not completely clear to us what does the referee mean by “definitions are not consistent as to use of term “LEO satellites”. As referee #1 mentions, there conical-scan imagers are erroneously called cross-track imagers. Indeed, we meant *conical-scan* imagers and will fix this error.

The number of geostationary satellites is not important in the context of our study since we consider only synoptic times as observational time, so any number of geostationary satellites would produce the same results for the given area. For the ease of formulations we consider it as one satellite called “GEO”.

(9) P-11685/L13: “regular” should be “irregular”;

(10) P-11686/L8 & L20: SE should be defined as a modified “index of dispersion” or “index of variation” rather than “sampling error”;

(11) P-11686/~L15: the discussion describing RMSE requires better clarification; (12) P-1168/L1: “field of view” is the incorrect term, it should be “swath” instead.

We agree with the points 4.9, 4.10 and 4.11, this is to be fixed in the revised version of the manuscript..

5. There are numerous technical deficiencies in the way of English usage and grammar, spelling, unneeded tutorial passages and inclusion of irrelevant diagrams. A few examples are: (1) Abstract: various improper uses of articles plus the inclusion of misleading phrase “of the common type”; (2) P-11683/L25: “orbits do” instead of “orbit does”; (3) P-11684/L25: “forth” is a misspelled word; (4) P-11687/L3-5: this last sentence is not needed; (5) Figures 1 and 4 are unnecessary.

We acknowledge these corrections and we will do our best to bring our spelling and grammar to the adequate level in the revised version of the manuscript by reviewing it by a native speaker. We will remove Fig. 4, however we believe Fig. 1 is necessary to provide reader with the sense of frequency of satellite overpasses and the order of the orbital drift.

Selected references

Villarini, G., & Krajewski, W. F. (2007). Evaluation of the research version TMPA three-hourly 0.25° × 0.25° rainfall estimates over Oklahoma. Geophysical Research Letters, 34(5), doi:10.1029/2006GL029147

Fischer, 2007: Statistical error decomposition of regional-scale cli- matological precipitation estimates from the Tropical Rainfall Measuring Mission (TRMM). J. Appl. Meteor. Climatol., 46, 791–813.

Fisher, Brad, David B. Wolff, 2011: Satellite Sampling and Retrieval Errors in Regional Monthly Rain Estimates from TMI, AMSR-E, SSM/I, AMSU-B, and the TRMM PR. J. Appl. Meteor. Climatol., 50, 994–1023. doi: http://dx.doi.org/10.1175/2010JAMC2487.1