Review of “Sampling the diurnal and annual cycles of the Earth’s energy imbalance with constellations of satellite-borne radiometers” by Hocking et al.

This paper addresses a concept to derive Earth’s radiative energy imbalance from space-based observations. As the title suggests, the focus is on diurnal and annual sampling, the former being an important source of uncertainty in current Earth radiation budget observations. The authors’ propose to reduce diurnal sampling errors by choosing appropriate orbits and implementing a constellation of satellites, each member of which will fly a suit of instruments that will alternate between solar and terrestrial viewing.

On L. 70 the authors’ are careful to point out that “… we focus only on sampling errors using the Earth-facing radiometer, with the intention of prioritising the accuracy of the long-term global mean over spatial and temporal resolution.” Idealized instruments with perfect response are assumed along with idealized Lambertian scattering and emission – only the sampling biases due to orbital characteristics of the hypothetical constellation are addressed in this paper.

The heart of the paper, the results presented in section 3 that address the attributes of polar and precessing orbits for minimizing sampling errors in acquiring EEI are sound, if not necessarily surprising. The combination of particular precessing and polar orbits have intrinsic strengths that mitigate the weakness of the others. Perhaps I missed it: presumably, the authors were motivated to derive a concept with a just a small (three) number of orbits and that is fine. Perhaps they want to state their motivation for the number of orbits. Cost? With miniaturization of sensors and spacecraft, the opportunities to deploy larger (in number) constellations will increase in time. The paper demonstrates advantages in numbers, especially in beating down sampling error. Potentially flying even larger constellations could be discussed in addition to the specific three-orbit concept proposed for their mission.

Based on the emphasis on reducing diurnal sampling errors, I recommend the paper for publication. However, I have a list of issues below that should be addressed. One important issue is the authors’ target uncertainty that is the same magnitude of current EEI estimates, 1.0 Wm^{-2}. More than once they list that as the desired uncertainty. Granted, most of the sampling errors listed in Table 2 are on the order of 0.1 Wm^{-2} but this is only one contribution to total uncertainty, many more of which will be left to future publications. To achieve the goal of resolving a 1.0 Wm^{-2} difference between the incoming and outgoing radiative energy is a daunting task that requires exquisite accuracy across all elements of the measurements and analysis equations. Even in this paper the authors seemingly overlooked some error sources, for example, those due to deriving a global mean outgoing irradiance (section 2.5). I would like to see the authors address these issues below prior to publication. I selected “major revisions” but these are somewhere in between major and minor: important but not difficult to address.

Here are specific comments:

1. L. 47-48: “Current estimates of the EEI are based on both direct measurements by satellite radiometers and inventories from ocean heat content measurements.”

   I suggest removing the word “direct” since the authors later explain some of the modeling required to covert directly measured radiance from satellite radiometers to global-averaged irradiance. Those are far-removed from “direct”.

2. L. 65: "…for an absolute accuracy of the annual EEI within 1.0 Wm^{-2}."

   If the imbalance is estimated to be 1.0 Wm^{-2}, how is that uncertainty (and it is uncertainty, not accuracy) sufficient to resolve the EEI? To resolve that level of imbalance, far lower uncertainty is required.

3. L. 77-79: "Previous efforts have typically relied on a diurnal model to synthesise full sampling of the diurnal cycle, which requires that the model does not introduce additional errors."

   Please provide a reference.

4. L. 111: "…irradiance I…" should be "…radiance I…"

5. L. 112: "Taking into account the $1/d^2$ decrease of the intensity with distance d …"

   Change "intensity" to "irradiance" since intensity is undefined in the paper and it is no longer a standard term in radiative transfer.

6. Eq. 1 is an interesting form of the equation relating radiance to irradiance; you may want to point out that this form is necessary, with integration over area rather than solid angle, since you are using CERES results for *M* (irradiance).

7. L. 117: "For a non-perfect instrument response, the ideal cos(η) factor would be adjusted accordingly."

   Please explain; "would be adjusted accordingly" for what purpose? To provide a correction to measured *F*? That is impossible because it would require a priori knowledge of *M* (or equivalently, radiance).

8. L. 139: "… perfect cosine response to the flux"

   There is no cosine response to "flux" (and the correct term is flux density, or irradiance); irradiance is the integral over solid angle of cosine-weighted radiance.

9. Table 1: I do not think this table adds much to the paper since most the table entries are undefined. Is it possible to add at least a short description of the terms? If not, please consider removing.

10. Fig. 7 caption: "The shortwave component includes the correction described in Sect. 2.5."

    I must have missed it but I do not know what the referenced correction is; Fig. 7 is already in section 2.5

    Okay, I see that the correction comes two pages later, in equation (2). This is awkward and likely to be confusing to most readers. Please rearrange to introduce the correction prior to figure 7.

11. L. 182: "For this study, a 5° by 5° grid provided sufficiently good results …"

    Please quantify "sufficiently good".

12. L. 183: A fortnight is not a common term for all readers; please use days, weeks, etc.

13. L. 196-194: “Note that this correction requires a method to separate the full-spectrum radiometer measurements of outgoing radiation into longwave and shortwave components, such as the proposed ECO cameras”

    Since this correction relies on separation scattered sunlight from emitted terrestrial radiation, some estimate of the uncertainty using cameras to do this separation needs to be addressed.

14. L. 201-217: Similar to the previous comment, is not clear if the uncertainty due the processing required to obtain global means has been considered. The error due to the shortwave correction in particular should be estimated.
15. L. 304: “In order to remain within the nominal target uncertainty of 1.0 Wm−2 …”

    As in comment 1, it is confusing that the target uncertainty is 100% of the estimated imbalance.

16. L. 315-316: “As a result, satellite radiation measurements cannot currently independently verify EEI estimates from interior methods.”

    Do “interior measurements” refer to the Argo float network of ocean temperature measurements (more commonly called *in situ*)?