Biogeosciences Discuss., 8, C2906–C2912, 2011
www.biogeosciences-discuss.net/8/C2906/2011/

Biogeosciences Discussions

Interactive comment on "The variability of radiative balance elements and air temperature on the Asian region of Russia" *by* E. V. Kharyutkina et al.

Anonymous Referee #2

Received and published: 6 September 2011

Comments on Manuscript,

"The variability of radiative balance elements and air temperature on the Asian region of Russia" by E. V. Kharyutkina, I. I. Ippolitov, and S. V. Loginov (Manuscript Number: bgd-8-4331-2011)

I. Overall Comments The article is not well prepared. Some parts are unnecessarily lengthy while important parts are not sufficiently developed. The readers can hardly be convinced by their conclusions. The readability is below the standard of a typical scientific literature. The contents of the sections 4 and 5 may be developed to be more valuable and convincing. Therefore, I suggest that the manuscript be rewritten. The

following are more specific comments.

II. Specific Comments 1. Title The title may be revised to "The variability of surface radiative fluxes and air temperature for the Asian Region of Russia" or "The variability of cloud amount, surface energy fluxes and air temperature for the Asian Region of Russia" if Section 6 is the authors' most important one for clarity.

2. Abstract The Abstract should summarize main topics and conclusions based on your development through the contents of the article but it is not so.

(1) Delete "dynamics" (line 6) since you only use "cloud cover" and do not have much to do with cloud dynamics.

(2) There are no solid evidences from the contents to support "Annual averaged radiative balance values at the top are negative; it is consistent with negative annual averaged air temperature, averaged over territory" except the same sentence also appears in Section 7 (Conclusions). Here, "top" seems to be top of the atmosphere, or TOA. Also, do you really have "negative annual averaged air temperature"??

(3) In the last sentence, "The downward trend of radiative balance is the most obvious after the beginning of 90s of XX century, do you really mean the trend of the total net radiative flux that is the usual meaning of "balance" (=SWnet + LWnet = QâĘŞs - QâĘŚs – Eeff)? It seems not the case.

3. Section 1 (Introduction) The Section 1 has too many contents that are unnecessary for the article. You only need give a concise review for the subject you want to explore in the article and then introduce main concerns and topics. The volume can be cut to 1/2 to 1/3 of the current size.

4. Section 2 (Data etc.) It needs refinements to give more accurate descriptions, e.g., versions, uncertainties or error estimates of the datasets you want to use.

5. Section 3 The Section repeats too much the work by Ippolitov et al. (2008). You only need describe their most important finding and main conclusions that are necessary

for your development for this work.

(1) P. 4337, line 2. Use "global-mean surface air temperature" instead of "air surface global temperature".

(2) P. 4338. Line 9. For which datasets do you present correlation coefficient? Between JRA and AMIP?

6. Section 4 Both Sections 4 and 5 are what you should really focus on. They need some clarifications and more quantitative development to draw useful and convincing conclusions.

(1) P. 4338, line 15. You never say anything about surface albedo after this mentioning, so you don't need mention albedo here if you do not plan to say about it later.

(1) P. 4338, last paragraph (to next page). After mentioning downward shortwave radiation at the beginning of the paragraph, you then talk about "total radiation" without even defining it. "Total" can be ambiguous here (and you cannot expect majority of readers to read Ippolitov et al., 2009 in none-English) while Figures 2 and 3 only show TC, QâĘŞs and Eeff without total radiation. Actually, you have not shown any "total radiation" anywhere in the article. For overall reduction of 10-15% (for total radiation) and bias of 0.40 to 0.55 (for the northern hemispheric cloudiness) for JRA, you need supply their most important validation statistics, at least the total station number and their geographic distribution, time period, mean difference and standard deviation, from Ippolitov et al. (2009) and Chernokulsky et al. (2010). The word "impartial" should be replaced by "impartially" in line 4 from bottom.

(2) P. 4339, line 6. Here you first time introduce "total cloudiness" so the acronym, TC, should really be also introduced here, not later.

(3) Same page. Why do you suddenly introduce Eq. (1) while you focus on radiative fluxes in this section? It may be more suitable for Section 5.

(4) P. 4340, 2nd and 3rd paragraphs. You must supply more quantified evidence to

support your "good agreement" claim. One-station example in Fig. 3(b) does not mean much.

(5) Same page, 4th paragraph (line 12). Is it the tendency for global or ATR mean? Which dataset(s) are you referring to (since there are many on the web site)?

(6) Have you looked at aerosols effects on QâĘŞs in temporal variation trends during 1979 to 2008 period (since clouds are just half of the story)?

(7) Next paragraph (6th). Which period and which dataset are for Table 2? How do you explain the same "trend" between QâĘŞStr and TCtr for Jan.-Feb., Apr.-May, and Nov.-Dec.? As these six months are half of all the 12 months, how do you validate your conclusion on the relationship between TC and QâĘŞs?

(8) Last paragraph on P. 4340 and 1st paragraph on P. 4341. "Similar" and "connected" are not sufficient to draw any useful or valuable conclusions. QâĘŞ at TOA cannot vary with clouds since it is solely determined by the Sun. The statements "The spatial distribution of trend values of the upward longwave radiation $L\infty$ at the top and the downward solar radiation QâĘŞs at the surface are similar" is conflict with "the opposite behavior is observed for $L\infty$ at the top and QâĘŞs at the surface". More quantitative results must be supplied besides Fig. 4.

(9) P. 4341, line 2. It is not justified that (LW) "effective radiation variation is mainly connected with cloudiness variability". In general, surface skin and air temperature plays dominant role on the surface up and down LW fluxes, respectively.

7. Section 5 (1) P. 4341, 4th paragraph (line 14). You really should introduce the definition of Eeff on P. 4334 when you first use "effective radiation", which should really be "effective surface LW flux" for clarity. Are the numbers appearing here are for ATR? You may say "all the following are for ATR unless otherwise specified" to avoid any ambiguity as well as save text at the earliest.

(2) Eq. (2). It's better to define Ts as surface skin temperature and T as 2-m surface

air temperature.

(3) P. 4342, 2nd paragraph. The empirical LH and P formulas in (2) usually have large uncertainties that may not give correct interpretation.

8. Section 6 You need describe what your “regression model” is and how it is justified to use for your purpose with references supplied. Eq. (3) can hardly be justified since Qn (particularly QâĘŞs) is closely related to clouds (both amount and optical depth) while surface Eeff is mainly determined by surface skin and air temperature. Indeed, all the items in Eq. (3) are the incomplete components of a very complex climate system, within which, these components are interacting in complicate ways and form many feedback loops that make them internally correlated, closely or remotely. If the authors try to “predict” one of them using all the others, it only makes sense when using relatively more reliable and more easily obtainable ones to predict the others. As surface radiative fluxes are much less observed than surface air temperature, and turbulent fluxes are even not directly observed but calculated using bulk formulas with lower accuracy, these surface energy fluxes over an area (or whole globe) have to be calculated from the physical properties of the atmosphere (including clouds and aerosols, and temperature/humidity profiles) and the surface properties (including albedo, skin temperature and emissivity), most of which are routinely observed, e.g., Eq. (2) calculates turbulent fluxes from surface skin and air temperature/humidity. In other words, using the atmospheric and surface properties to “predict” energy fluxes makes more sense, and indeed, current radiative models can determine surface fluxes (as concerned here) quite accurately with larger errors coming from input properties, not models themselves. It remains unclear why the authors want to “predict” surface (air or skin) temperature anomaly from the energy fluxes (that are less ready) and cloud amount anomalies. Moreover, the authors first use surface skin and air temperature (and humidity) in Eq. (2) to calculate turbulent fluxes and then, in Eq. (3), use turbulent fluxes back to “predict” surface temperature that they just used in Eq. (2). Does it make any sense in such a loop?

9. Section 7 (Conclusions) This section has not well summarized what the authors developed in previous sections and it can hardly have convincing conclusions. In paragraph 1, it should specifically indicates which component of solar flux (up, down or net) and where the solar flux and air temperature are for (surface?). Using precise concepts and words in your text is crucial for people to understand your work. Also refer to the above comments to Abstract. You need distinguish ‘solar” and ‘radiative balance’, the latter is for total net flux from both SW and LW net fluxes. It is hard to know what the authors want to conclude in paragraph 2 and the authors need rephrase it. The last paragraph is from Section 6 so I do not repeat above comments.

10. Tables and Figures Table titles and figure captions are not clear or do not give sufficiently information, e.g., in Table 1, it should use “surface air temperature” (instead of Air surface temperature”) and give its source (from Ippolitov et al., 2008). Do you really need Table 1 that is copied from Ippolitov et al.(2008)? Table 2 should give the period during which the numbers are produced. Fig. 1’s caption should explicitly indicate if it is surface skin or air temperature, which are different but your whole article doesn’t distinguish the two quantities, and you should also indicate its source (from Ippolitov et al., 2008). Fig. 2 misses NCEP/DOE AMIP (only “by JRA-25”) so the readers cannot tell which curves are from AMIP or JRA .

11. Others Besides, some acronyms, e.g., NCEP, JRA and ERA, are not given their full name, or introduced with their full names after they are used several times later, e.g., TC. The convention is that if you intend to use an acronym to save text, you should introduce it at very first time with its full name expanded, and then you can systematically use the acronym through all the rest text (usually no longer using the full name again unless it is necessary). Some concepts are not given definition when they are first time introduced, e.g., “effective radiation” is not defined until it has been used several times later and it should be “effective surface LW” since radiation usually includes both SW and LW at any locations. Some concepts/words are not given clear or correct definition, e.g., “top” instead of TOA for top of the atmosphere. There are

numerous such examples that the authors should take care of and check twice before their submission to avoid ambiguity. If there is a conventional usage, it is better to follow it.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/8/C2906/2011/bgd-8-C2906-2011-supplement.pdf

---

Interactive comment on Biogeosciences Discuss., 8, 4331, 2011.