Reviewer 1:

General comments:

The paper of Mallick et al (2014) presents a simple and potentially interesting approach for estimating midday net radiation and net available energy at the Earth' surface (Rn-G) from satellite sounders. The method has been applied to data acquired twice daily by AIRS and MODIS sounders, obtained or aggregated at 1° resolution and monthly averaged, and extensively evaluated with the help of ground observations of turbulent heat fluxes from 30 FLUXNET stations. This validation allowed the authors to draw conclusions on the quality of the estimates for each biome considered.

The paper is within the scope of BG journal, globally well written, and the results well presented in a concise style that the readers may appreciate. However, the paper intents to address multiple objectives at the same time (a new methodology to compute surface heat capacity, application with satellite data and generation of a dataset for use in an evapotranspiration model presented in a second paper). This makes the paper not very focused on clear objectives, and sometimes do not address fully each of them properly.

The paper would probably gain in focusing on one objective only (the methodology, for example), or merge the two papers intended for the series. From my point of view, the method to derive Rn-G, and in particular surface heat capacity, is particularly interesting as such, although some assumptions need to be checked, as well as the extensive validation at FLUXNET sites. In that perspective, I think that the paper should be more focused on the method, which is the most innovative part of the study. I would therefore strongly recommend to first validate the new method at FLUXNET sites using ground observations of the surface radiation components as input, before applying it with satellite data, as the latter part has already, but partially, been done in other studies with a high degree of success (eg Verstraeten et al (2005) with NOAA-AVHRR for instantaneous net radiation). An alternative would be to integrate Paper 1 (this paper) into Paper 2 (paper on the Bowen ratio), as it would justify better some choices made in the methodology (monthly time scale, combination of AIRS and MODIS data).

We thank R1 for their helpful comments. We understand R1's position with respect to wanting to see a fuller evaluation of some of the various steps taken to produce the net available energy estimates. Although many of the steps have been evaluated elsewhere, novel aspects such as the derivation of G and surface heat capacity do require a fuller evaluation here. As a result, we have demonstrated the retrieval of Φ using in-situ surface temperature and day-night Rn observations of FLUXNET eddy covariance towers and evaluating the Φ retrievals against observations at the tower sites. The results of Φ retrieval over four different broad biome categories (grassland, cropland, forest and savanna) using in situ data is demonstrated in figure 6 (a to d). The associated Φ retrieval results are also described in Line 364 to 371.

We have also explored a single merged paper, but it becomes unmanageably long and so our preference is to keep the existing two (joint) paper format. Providing the links between the two are clear and they are published together, we believe this is the most efficient and effective means of presenting the overall framework.

1. Questions on the choice of specific satellite data for this study.

• After reading the paper, it is not clear to me why a combination of AIRS and MODIS data has been chosen. Is there any reason to prefer the use of AIRS data in this context compared to MODIS (as described in Peng et al, 2013)? Certainly, the use of AIRS data make the validation task much more difficult because of the huge difference in spatial footprint, and this choice imposes the results to be concentrated on 1 time slot per day, 13:30 local time. Other satellites could possibly have been used to have more passes per day.

One of the core objectives of the work is to explore the potential of atmospheric sounding data. AIRS is the only dedicated sounder available which can be explored to address the objectives in the paper. Although MODIS has soundings, it was not designed for this and only has low quality air temperature soundings. We fully appreciate this introduces many difficulties when it comes to the evaluation, but the most important aspect of these two joint papers is to introduce the possibility of using sounding data as a means of observing surface energy fluxes. We have restricted the net available energy derivation to (largely) AIRS data (we use MODIS albedo because AIRS does not contain any albedo field) in order to exploit a single platform for the entire framework, something we believe is important. These are clearly explained in section 2.1 (Line 161 to 173).

Please see comments below on handing the scale mis-match.

• A monthly time scale has been chosen for the study. The perspective of this choice is not exposed. One may wonder why daily or weekly time scale has not been selected, as it could be at least equally or more interesting to study the daily time scale for monitoring.

The daily 1 degree data contains orbital gaps and cloud contamination. In the 8-day data the co-incident land surface temperature in both day and night pass was missing and the atmospheric soundings were also missing in many places. It is the monthly dataset where the soundings as well as both the day-night land surface temperatures were available and the data has complete global coverage. This is the reason for using the monthly datasets. We have now mentioned this explicitly in section 2.1 (Line 136 to 141).

2. Questions on the assumptions

• I found the method to derive net available energy interesting, and certainly very promising for further applications. However, the paper would gain in credibility if all the assumptions were checked to be realistic in using ground observations. The assumptions to be checked are: the symmetry of the Ts difference over a month, Rn - G = 0 at 1:30 AM LT (Although there is already an indication of it in the paper, an information of the distribution of the Rn-G at night may be useful).

To address R1's first point we have shown figures (Figure 2a to 2c) of the 30 minute samples of Ts for a monthly averaged day highlighting the 13:30 hours and 1:30 hours Ts samples for three different seasons of year. In Figure 2, the saw-tooth pattern between noon (13:30 hour) and night (1:30 hour) Ts is evident. This clearly shows how well these two samples capture the dynamic range of the day and hence the discretisation is representative of the daily energy balance.

The diurnal distribution of Rn-G is shown in Figure 3 to support the hypothesis Rn-G ≅ 0 at 1:30 AM.

A detailed description on these figures (Fig. 2 and 3) are given around line 253 to 262.

• It is assumed that the estimates at 1 degree can be directly compared with point scale observations at the surface, which is an uncertain hypothesis. The authors themselves point out in the paper that the scale mismatch can be a source of discrepancy in their results, but without quantifying this effect. I would suggest to first verify the validity of the new methodology to compute Rn-G with ground observations only (if possible, or at least with satellite data at finer scale), and then to apply it at global scale with satellite soundings. This would certainly help in both acknowledging the accuracy of the new method and better understand the effects of scale mismatch.

Comparing these two very different scales is not a matter of choice, but necessity. It is important to keep in mind that our objective is to use AIRS soundings to estimate the surface latent heat flux (in a companion paper by Mallick et al., 2014) and that in using AIRS we are restricted to this scale. That said, we accept that we could derive some higher resolution Φ estimates using tower infrared radiometer measurements of surface temperature along with corresponding net radiation measurements as detailed above and compare these against tower data. Figure 6 illustrates the evaluation of the proposed Φ retrieval scheme using high temporal resolution data at the eddy covariance footprint scale where measurements of both Ts and R_N were available. The Φ retrieval results at the tower scale are also described around line 363 to 371.

<u>3. On the structure and text of the paper.</u>

• The time scale should be mentioned in the title, the abstract and the introduction.

Good point, although it is important to realise the estimates themselves are not monthly averages, but 13:30 and 1:30 values computed from monthly data. This is made clear in the title, abstract and introduction.

• The satellite datasets should be described first to interpret correctly the methods as they imposes constrains, or the methods should be reformulated to avoid relying on AIRS and MODIS specific products, leaving their description for after.

The satellite datasets are described first (section 2.1) as suggested.

• In the discussion section, the authors give a series of references to support the quality of their estimations. From my point of view, I found difficult to state the quality of the proposed estimations compared to the other studies as such: some references concern instantaneous values, others daily averages, some of them on very limited samples, but all with satellites soundings at finer scale (from MODIS, GOES). If I add three other references I found (Verstraeten et al, 2005; Jin et al, 2011; Peng et al, 2013), it is still difficult to know how to compare to the RMSD found in this study with others (See Table below). The suggestion of point 2 would certainly help the readers to apprehend the effect of scale discrepancy with the error due to the methods, assumptions. In addition, the authors could explain how to compare the different results from other studies with theirs.

We agree our RMSD is not comparable to the previous studies and we have made it explicit in the text (line 395 to 415). RMSD is getting impacted in two ways, due to scale mismatch and due to the time integration. A list of relevant satellite based RN estimation studies and associated errors are also given in Table 4. We thank R1 for raising this.

• The last paragraph of the discussion section should be moved (and reformulated) to Introduction, as it justifies some choices, and make a link to the second paper.

Good point, we have reformulated the last paragraph of the discussion and moved this to the end of the introduction (line 107 to 120).

• The conclusion section should state the conclusion based on the results obtained, the potential applicability to other sensors and perspectives. A part of the conclusion section (first paragraph) should be moved to the discussion section, as limitations of the method are discussed. A clear conclusion should be stated here. Applicability to other sensors is briefly mentioned by citing other satellite missions, but sometimes sounds a bit far stretched, without the authors giving a clue on how to proceed (for example, how to apply the method initially tested for monthly averages once a day to get 30 min estimations of Rn-G ?). Therefore, I would suggest either to limit the sensors list to which the method can be applied, or give a short explanation on how to proceed (the option I would recommend).

Again, good point. The first paragraph of the conclusion is moved into the discussion section (line 529 to 541).

Part of the conclusion is re-written (line 542 to 555). Some explanations on the potential use of the method to generate Φ at moderate spatial resolution (5 km) using MODIS data and at high temporal resolution using geostationary satellite data are elaborated in line 556 to 564. This says,

“ With the availability of high spatial resolution (1 – 5 km) MODIS day-night optical and thermal data, our present approach could be extended to derive high spatial resolution Φ estimates at the global scale. This could be achieved by estimating MODIS-based day-night R_N and combining day-night R_N with day-night T_S observations. At the same time, the current methodology could also be used on high temporal frequency observations of geostationary satellite (e.g., GOES and METEOSAT). Having estimated surface heat uptake and heat capacity (through equation 10), hourly G and Φ could be determined from hourly R_N and T_S observations of geostationary satellite by assuming conservation of heat capacity over a particular day. ”

4. Typos and additional references

• p11828, eq 1: $\Phi = \lambda E + H$

• p11832, eq 8: $\lambda E(t)$

Both the typo errors are corrected.

• Verstraeten W., Veroustraete, F., and Feyen, J., 2005: Estimating evapotranspiration of European forests from NOAA-imagery at satellite overpass time: Towards an operational processing chain for integrated optical and thermal sensor data products, Remote Sensing of Environment, 96 (2), 256-276.

• Peng, J., Liu, Y., Zhao, X., and Loew, A. (2013): Estimation of evapotranspiration from MODIS TOA radiances in the Poyang Lake basin, China, Hydrol. Earth Syst. Sci., 17, 1431-1444.

• Jin, Y., Randerson, J. T., and Goulden, M. L. (2011): Continental-scale net radiation and evapotranspiration estimated using MODIS satellite observations, Remote Sensing of Environment, 115, 2302-2319.

These three references are added in R_N results intercomparison (Table 4).

Reviewer 3:

General comments:

The paper presents a simple promising approach for estimating monthly global fields of net available energy (Φ=Rn-G) for exchange of heat between the Earth surface and atmosphere. The method is based on a novel approach whereby Φ and the surface heat capacity (c) are solved analytically using inputs of the day-night temperature difference (dT) and Rn which are both estimated from satellite data. The method is directly applicable on a global scale and no empirical relationships between soil heat flux and vegetation indices or assumptions about G-Rn ratios are needed, as is usually the case. However the method is based on a number of other assumptions, and it would strengthen the paper to include a test of the method using higher spatial resolution data (field data or MODIS data) before its application to global low resolution (100 x 100 km2) AIRS data which is evaluated using FLUXNET data (with much smaller footprint). Currently, it seems like the estimated global radiation (Rs_in) could be the main reason for the (small) biases in Rn and Φ, and it would be valuable with further analysis and discussion of this problem.

We thank R3 for the supportive and helpful comments. We accept that to demonstrate the efficacy of the approach at higher spatial resolution would offer more reassurance to the reader (as also raised by R1). We have included a test of the proposed methodology over some representative biomes using tower measured Rn and radiometer measured land surface temperature only (Figure 6) (results are elaborated in section 3, line 363 to 371). However, given ultimately we have to use the AIRS sounding data for the overall method; we are constrained to this scale. As such, it is this scale that has to be ultimately evaluated and hence scaling will remain a recalcitrant issue.

Please see below for comments on Rs_in.

1) One assumption is that the system is approximately in equilibrium over 24 hours. In practice, all components of the diurnal energy balance are highly dynamic, however the method is applied to assess the mean monthly dynamics in which case the diurnal variability is smoothed out. Thus, it should be clear in title and abstract that the method is applicable only for estimation of average (eg. monthly) fields of Φ.

We agree, the monthly nature of the method is now made explicit throughout the manuscript including the figures and tables also.

2) Another assumption is that at 01:30 h, $\Phi \approx 0$. Even though this seems plausible, I suggest to use the field data to support this hypothesis. This would strengthen credibility of the method.

We have added additional figures (Figure 3a to 3d) covering four broad biomes to support the assumption of $\Phi \approx 0$ at 1.30 hours (please also see the response to R1).

3) As already noted by the authors, the assumptions lead to a dependency of daytime G on net long wave radiative energy balance which seems strange. Again, I suggest that the field observations are used to examine this relationship.

This is simply a product of the discretisation of the surface energy balance equations and stems directly from the $\Phi \approx 0$ at 1:30 assumption for which we have presented additional figures (Figure 3a to 3d). In order to further evaluate this assumption as suggested by R3, we have also presented Figure 4 that shows the two dimension scatters between midday G on nighttime net longwave radiation over four broad biome categories. Although we believe heat flux plate data does not provide an adequate evaluation data set because of the smaller footprint as compared to the relatively larger footprint of net the radiometers, the inverse relationship between the two variables in Figure 4 clearly indicates the dependence of midday G on the nighttime longwave radiation balance. A detail discussion in these figures will be found around line 253 to 275.

4) A MODIS global albedo product is available that could be used directly, but instead the broadband solar reflectance from the 7 MODIS spectral (bidirectional) surface reflectance bands are used for calculated the global albedo. Please explain the reasons for this.

The MODIS global albedo product contains black sky and white sky albedo. Look/up table based atmospheric information and parameters are needed to convert them into the blue sky albedo. But there are established formulations (Liang et al., 1999, Liang, 2001) to directly convert the narrowband reflectances into the broadband visible albedo that does not depend on any atmospheric variables and look-up tables. Detail explanation is included in the revised version around line 151 to 160.

5) A constant albedo for oceans (0.04) is assumed that do not consider sea ice. Why is surface albedo not assessed for oceans from the satellite data as it is for the land surface?

There is no ocean surface albedo data available in either AIRS or MODIS products and we shall mention this in the revised manuscript. Given the structure of the atmosphere and the very small energy fluxes involved, high latitude Φ and latent heat estimates from this method are likely to be problematic anyhow. We have emphasised this in the revised manuscript in line 202 to 203 and line 274 to 276.

6) For the evaluation of the results in Fig.'s 3 and 4 and Table 3, it is unclear whether the data-based Φ is derived as (Rn-G) or (λE+H). It makes sense to use (λE+H) for large-scale evaluation due to the larger foot print of atmospheric fluxes than (Rn-G), as mentioned earlier in the paper, but please clarify whether this is the case in Fig/table captions. Calculation of data-based Φ requires energy balance closure of data which is typically not the case (an analysis for all Fluxnet sites is seen in Stoy et al., 2013). Amiro (2009) show that the energy balance closure is better fulfilled when data are averaged over longer periods. Is this the case for the data used in the current study? Please report on the monthly energy balance closure of the data used.

In the present case Φ is derived as λE+H and this is stated explicitly in the figures and tables in the revised manuscript and also in line 293 to 298. We fully accept the issues around lack of local scale closure in tower data and highlighted this in the text. We have used the monthly averages of (AIRS overpass time) 13:30 hours net available energy from the eddy covariance tower. We have pointed this out in the text on the robustness of the closure assumption used to derive λE+H (line 293 to 296).

The monthly midday (13:30 hour) surface energy balance closure of presently used biome categories is listed in Table 5. Monthly averages of (AIRS overpass time) 13:30 hours surface energy balance closure of the 30 sites used here (Table 5) shows an average energy imbalance of ~20 percent (ranging from 8 to 34 percent). This is described in the discussion section (line 513 to 520) citing the reference of Stoy et al (2013) and Amiro (2009). Implication of the surface energy imbalance on the evaluation of satellite derived Φ is described in line 523 to 528.

7) If possible, please provide information about the area extension of the biomes that are represented by flux sites and show a biome map as background for the eddy covariance site map in Fig. 1.

The location of the eddy covariance sites and their corresponding biome categories are already given in Table 1. We believe this is sufficient given this is traditionally how such information is communicated. A biome map is shown in Figure 1 with the distribution of eddy covariance sites on the map.

8) The importance of the bias in Rs_in for Rn and Φ estimation can be tested using the field measurements of Rs_in as inputs. I suggest testing of the method using field data and discussion of possibilities to improve the satellite based estimation of Rs_in. A very simple method is used for Rs_in estimation in the paper which is based on a constant global clear-sky atmospheric transmissivity. Since Rs_in seems to be the main issue, what is the potential for improving clear-sky transmissivity and the global Rs_in estimates? (the problem seems to be largest at high Rs_in, so inaccurate clear sky transmissivity could be an important reason for model bias). Please compare with other studies and include discussion of this.

Good point, as R1 also pointed out. We accept that a more sophisticated treatment of the atmospheric transmissivity would improve the scheme, but rather than include this in the current manuscript (it represents a significant amount of additional work to develop and evaluate a model independent approach!), we have expanded the discussion on the potential avenues to improve the atmospheric transmissivity retrieval in line 450 to 477. An intercomparison of $R_{S\downarrow}$ retrieval results are described in line 477 to 487. A list of relevant $R_{S\downarrow}$ study characteristics and their associated errors are given in Table 4.

9) p. 15, l. 365-376. Regarding discussion about energy balance closure and its possible attribution, an extensive analysis of energy balance closure of all (173) FLUXNET sites were recently given by Stoy et al. (2013). Interestingly, the energy balance closure is generally best for savannahs and evergreen deciduous forests, and the results suggest that landscape heterogeneity (in addition to canopy heat storage) could be responsible for lack of energy closure. This hypothesis suggests that lack of energy balance closure is not attributed to systematic errors in Rn, G, H and λE. Amiro (2009) also indicates that energy balance closure is not an issue over longer time scales (only one site studied). In the current paper, it is quite obvious that the underestimation of Rs_in is a problem that will propagate to Rn. The importance of the bias in Rs_in for Rn and Φ estimation could be tested using the field measurements of Rs_in as inputs. References should also be included for comparison with Rs_in estimation results in other studies (as was done for discussion/evaluation of Rn

results), and perspectives to improve global Rs_in estimation discussed (see also comment 8).

We have added the reference of Stoy et al. (2013) and Amiro (2009) in the revised version of the manuscript and also added Table 5 to show the monthly midday energy balance closure of the biome types under study. Elaborated discussions on surface energy balance closure are also included from line 513 to 520.

The sensitivity analysis presented in Table 2 is already indicating the significant sensitivity of Rn and Φ to cloud cover fraction and atmospheric transmissivity. This shows the method presented in the manuscript to estimate Rs_in needs further improvements and the manuscript is clear in this regard. However, this is non-trivial and, given this is thrown up in the evaluation, as with all 'methods' papers, it is a matter of further work in our opinion. We have expanded the discussions on Rs_in (line 450 to 487) (Table 4) with suitable references. It says

''Clearly, the retrieval of atmospheric shortwave transmissivity (τ_A) using cloud cover fraction is the principal reason for $R_{S\downarrow}$(satellite) < $R_{S\downarrow}$(tower) (Figure 9a). The sensitivity analysis presented in Table 2 is also indicating the significant sensitivity of R_N and Φ to the cloud cover fraction and atmospheric transmissivity. This shows the method presented in the manuscript to estimate $R_{S\downarrow}$_needs further improvements. If we assume τ_A to be the principal reason for $R_{S\downarrow}$(satellite) < $R_{S\downarrow}$(tower) then a global value of 0.75 would be, on average, too low (Gueymard, 2003). A recent study of Longman et al. (2012) for the Mauna Loa Observatory (MLO) demonstrated the clear sky τ_A could go upto 0.90. Given the relatively well defined relationship between $R_{S\downarrow}$(satellite) and $R_{S\downarrow}$(tower) seen in Figure 3c one would imagine that a more sophisticated dynamic representation of τ_A would offer substantial improvements in $R_{S\downarrow}$(satellite). Retrieval of τ_A including other atmospheric (e.g., cloud optical depth, aerosol optical depth, total precipitable water etc.) and surface (for example, single scattering albedo) variables in addition to the cloud cover fraction would offer a potential possibi lity of refining the $R_{S\downarrow}$ estimates (Chen et al., 2014; Longman et al., 2012; Kim and Hogue, 2008). The exo-atmospheric shortwave radiation frequently interacts with the clouds, aerosols and water vapor during the transmission towards the Earth's surface. This interaction is wavelength-dependent over the entire shortwave spectrum (Chen et al., 2014; Kim and Hogue, 2008; Gueymard, 2003) and therefore spectrally resolved τ_A scheme will be valuable to accurately determine $R_{S\downarrow}$. Recalibration of τ_A using the tower data is also a possibility although we have avoided this given the AIRS cloud cover fraction and scale mismatch between the satellite and tower could also be implicated in the observed bias. For example, the diffuse fraction of $R_{S\downarrow}$(tower) can become enriched by surface reflected solar radiation, particularly in undulating terrain (Dubayah and Loechel, 1997; Sultan et al., 2014). Nonlinear scaling effects of surface albedo (*Oliphant et al., 2003;* Salomon et al, 2006) can also be implicated in this because surface albedo interacts nonlinearly with surface characteristics such as surface wetness and land surface temperature (Ryu et al., 2008) or the leaf area index (Hammerle et al., 2008). Although, the RMSD of instantaneous $R_{S\downarrow}$ obtained in the present study (110 W m^{-2}) is different to the other studies where $R_{S\downarrow}$ retrieval was based on either using parametric (radiative transfer) models or through look-up tables derived from high spatial resolution MODIS data, it is worth comparing it with the statistics of some of those studies. Table 4 summarizes the characteristics and associated errors of some of the recent $R_{S\downarrow}$ estimation studies, which shows RMSD of 36-89 W m^{-2} at flux tower footprint, 54 – 137 W m^{-2} at 5 km spatial resolution and 77 – 158 W m^{-2} at 1° spatial resolution for the instantaneous $R_{S\downarrow}$ estimates and 20 – 39 W m^{-2} for the daily $R_{S\downarrow}$

(and net shortwave, R_{NS}) estimates. Considering the simplicity of the current approach and the large spatial scale of the AIRS data, RMSD to the order of 110 W m^{-2} appears reasonable''.

Minor issues:

p. 7, l. 144. Please include unit for c in parenthesis following its presentation.

Included now.

P. 13, l. 301. "Table 2" should be changed to "Table 3".

Done as suggested.

Fig. 3. In figure caption, reference to Table 2 should be changed to Table 3.

Done as suggested.

Fig. 3: How were the longwave radiation component RL_in (tower) and RL_out (tower) estimated? Were they measured directly or calculated from air/surface temperature data?

Both the longwave radiation components were measured directly over the tower sites in question. Pyrgeometer measurements were available for a limited subset (14) of tower sites and this is mentioned in the manuscript in line 488 to 491.

Table 3. Please specify in caption that the statistics are based on monthly values. Please also provide the percentage errors to facilitate comparison with discussion on page 13.

Done as suggested.

P. 12, l. 295->. Please include description of results shown in Figures 3c, 3d and 3e (only 3a and 3b are described). In particular, Fig. 3c indicates that underestimation of Rs seems to be the reason for underestimation of Rn.

These are now Fig 9 (a, b and c). While probing into the detail behind the reasons of Rn and Φ underestimation, we had analysed the shortwave ($R_{S\downarrow}$) and longwave radiation components ($R_{L\downarrow}$ and $R_{L\uparrow}$). We have separated the radiation component figures (Figs 9a, 9b and 9c) to clear this confusion. The descriptions of the results related to Fig 9a are given in Line 441 to 449. Similarly the descriptions of Figs 9b and 9c are also given in line 488 to 495.

p. 13, l. 302. It should be specified that the error results of Bisht (for Southern Great Plains) are comparable to results for grassland in the current study. In fact, other biomes show larger errors for Rn.

We have mentioned this in the revised manuscript (line 397 to 398).

p. 13, l. 315. What was the surface type (or biome) in the study of Stisen et al. (2008)? And what was the time resolution of their results? Hourly, daily, monthly?

Stisen et al. (2008) conducted their study on grassland and used 15 minutes geostationary satellite data. This is now mentioned in the revised manuscript (line 418 to 420).

p. 14, l. 335. Even though there seem to be only a marginal difference between measurement and calculations, then the bias becomes important for radiation budgeting which should not be neglected.

Point taken and necessary corrections are incorporated in line 450 to 460.

References:

Liang, S. (2001), Narrowband to broadband conversions of land surface albedo, Remote Sens. Environ., 76, 213–238.

Liang, S., Strahler, A., and Walthall C. (1999), Retrieval of land surface albedo from satellite observations: A simulation study, J. Appl. Meteorol., 38, 712–725.