Respond to Referees

Thanks for the opportunity imparted by Editor to resubmit the paper. Thanks very much for heartfelt comments, discussion and marked errors of Referees. We receive fully referees advising. We made major revisions point-by-point based on referees advising.

Report #1

Submitted on 28 Oct 2014
Anonymous Referee #1

The manuscript has certainly improved since the last version, particularly the introduction. The English is less problematic, but the structure in some sections (Results) still is. Many paragraphs still go in all directions.

MAJOR COMMENTS

1. I have one main concern about the paper in its current form. The authors are trying to draw some large-scale conclusions based on a very limited set of observations. Some sections of their work are still based on an analysis of 3 x 1h data segments at a single site. This is simply not representative enough!

2. I would really like to see the Ogive plots of some of the turbulent fluxes as I previously asked.

Respond:

1. To receive your opinion, we add a paragraph to analyze the Ogive characteristics with our data.
2. Two sets of data are used in the study. The data in ASL at the NSPCE/CAS are **from 23 July 2011 to 13 September 2011**. The data are divides into **continuous sections** of 5-hour, and the 1-hour high frequency signals are obtained by applying Eqs. (8) and (9) on each 5-hour data. This is a technique processing for necessary to test the ergodic theorem of stationary random processes. It does not only '*based on an analysis of 3 x 1h data segments at a single site.*' Of cause, such studies are preliminary, and many problems require further research. The attestation of more field experiments is necessary.

MINOR COMMENTS

l. 22-23: multi-station vs single station: explain your methodology in a few sentences.

l. 49: "experience" -> this is the first time this word is being used ("experience “a trajectory traverses all points on the energy hypersurface) points on the energy hypersurface.
Ehrenfests dubbed the quasi-ergodic hypothesis, namely, the assumption that the trajectory lies densely (i.e. passes arbitrarily close to every point) on the energy hypersurface.)

l. 74: replace "Many litteratures" by "Many authors". Also, cite your sources.

l. 84: the transition between these sentences is rough. Also, define lidar at the first occurrence only.

l. 111: what do you mean by "the whole layer atmospheric turbulent flux"?

l. 130+: but there are other techniques than the double rotation approach exactly for the reasons you are pointing out. Refer to the planar fit approach here (which you mention later in the paper).

l. 103-156: this paragraph is poorly organized.

l. 192: This is not the proper definition of autocorrelation. You need to substract the mean. (Here, it is a definition of autocorrelation function in the stationary random process.)

l. 391-429: this is a long discussion for a super local and punctual phenomenon.

Figure 1 is useless.

Respond: All are advised point-by-point. Figure 1 is also deleted.

Report #2
Submitted on 13 Aug 2014
Anonymous Referee #2

Respond： Thanks

Report #2
Submitted on 05 Nov 2014
Anonymous Referee #3

Review of the manuscript “Ergodicity test of the eddy correlation method” by Chen

et al.

General remarks
This manuscript deals with the ergodicity hypothesis for turbulence data, an often forgotten assumption behind the eddy covariance method. This manuscript proposes three new data analysis methods to evaluate the ergodic theorem for observational time series. These methods are applied to data from a more or less typical flux tower site in Nagqu, China and from the CASES-99 field campaign, where multiple turbulence towers were closely collocated. I completely agree that it is actually very important to check a data set for the ergodicity assumption if possible. However, I don't agree that this is usually not done at all. If turbulence is stationary and homogeneous, then it is also ergodic (Galanti and Tsinober 2004). At least for stationarity, there are some well-established test procedures available that are widely applied in the micrometeorological and eddy flux community (Foken and Wichura 1996; Vickers and Mahrt 1997). The homogeneity criterion plays and important role during the site selection process for eddy towers, hoping for as homogeneous turbulent conditions as possible. Nevertheless, it is well-known, that true homogeneity can hardly be met in the real world, and even for homogeneous surfaces the turbulence field can be inhomogeneous due to turbulent organized structures (Inagaki et al. 2006; Huang et al. 2008).
Although the manuscript certainly has scientific merit, it is hard to read because major problems with the English grammar. Particularly, the introduction section needs major revisions in order to improve the use of the English language. This section is also too long and lacks clarity. It could probably be trimmed to half of its current length by applying a more concise writing style and by avoiding unnecessary repetitions. Sometimes, almost identical text passages are repeated a few lines later (e.g. 71, 89 134 etc.). The following sections starting with "theories and methods" are much more readable. The figures and tables are instructive and the major conclusions are drawn correctly. In the discussion section, I would have liked to see that the authors relate their findings more to other studies from the literature with similar topics. In general, I would recommend that this manuscript can be accepted for publication in ACP after major revisions, particularly regarding the use of the English language (e.g. singular or plural forms, use of the article 'the', tenses, sentence structure), have been made.

Minor comments
L48-49: This sentence cannot be understood even when ignoring grammatical mistakes.
L51: recognized instead of was recognizing
L61: Do you perhaps mean spatial average instead of average square?
L61: The correct reference is Galanti and Tsinober (2004), and plural form should be used in the following sentences
L115, L367-368: In contrast to the authors' statement, it is NOT common practice anymore to apply linear de-trending to a time series before calculation covariances.

The high-pass filtering effect of such a procedure would cause an unwanted underestimation of the total flux (Finnigan et al. 2003; Moncrieff et al. 2004). The McMillen (1988) reference presented by the authors is outdated.
L208-209: please check this sentence for English grammar
L416: a positive buoyancy effect
L446: Such numbering of paragraphs is uncommon, maybe use third order headers, e.g. 4.1.1 Verifying average ergodic theorem of eddies in different scales etc.
Section 5, Discussion:
Could you please comment on the question to what extend an analysis of a time series alone (without spatial information), such as your "average ergodic function" and your "autocorrelation ergodic function" can really be useful to evaluate the ergodicity assumption. Can non-propagating structures (Mahrt 2010) be detected by the proposed test procedures?(Computing turbulent fluxes near the surface: Needed improvements L. Mahrt

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ABSTRACT *With the recognition that eddy flux measurements are relatively accurate for a variety of common situations, a number of issues leading to inaccurate flux estimates and/or ambiguous interpretation of flux values are surveyed. These issues include inadvertent conversion of random errors to systematic errors, ambiguous differentiation between turbulence and other motions, and omission of transport by stationary eddies. Correcting for sonic misalignment and flow distortion in the presence of real systematic vertical motions is also problematic. Special emphasis is placed on the need for spatial information, partly to include vertical transport by stationary circulations induced by small-scale surface features. While no categorical solutions to the above problems are offered, promising approaches worthy of more investigation are discussed.)*

It would interesting to see how the results of this ergodicity test relate to the steady state test of Foken and Wichura (1996) compares statistical moments of 5 min and 30 min averaging time, or the stationarity test by Vickers and Mahrt (1997), which looks at the trend of a time series?
L800: I completely agree that the eddy covariance method is based on the ergodic assumption. However, it does not make use of Monin-Obukhov similarity theory, as it directly measures the turbulent exchange of a scalar.
L830: I completely agree that a lack of ergodicity related to the presence of large-scale eddy transport can lead to a considerable error of a tower flux measurement. This has already been pointed out by Mauder et al. (2007) or Foken et al. (2011) for example. Particulary, airborne turbulence measurements can be quite useful to determine fluxes based on spatial averaging and compare them with tower-based flux estimates.
L840: Indeed, eddy fluxes based on multi-station observation data are more likely to fulfil the ergodic assumption and therefore are less prone to error. Obviously, such

spatial data sets are rare because of the big expense and the large logistical effort of such a measurement campaign, but virtual tower setups in a large-eddy simulation model can be readily employed to generate such data. Steinfeld et al. (2007) have published such a study and they came to interesting finding about the minimum required number of towers to obtain a representative flux estimate for a certain spatial domain.

References

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Respond:

Thanks very much for your heartfelt comments, discussion and marked errors**.** We receive fully your advising. We make major revisions point-by-point based on your advising.

1. The introduction section is revised to be reduced majorly.
2. To rewrite the discussion section and the abstract. In discussion we add possible application about *"average ergodic function" and your "autocorrelation ergodic function"*
3. We take notice of the steady state test of Foken and Wichura (1996) compares statistical moments of 5 min and 30 min averaging time, and the stationarity test by Vickers and Mahrt (1997), which looks at the trend of a time series. In this work, Table 1 and 2 give indirectly statistical moments and their trend of 5 min to 60 min averaging time for the eddies of different temporal scale, and that based on that to identify the eddy scale. But the results of Foken and Wichura (1996) and Vickers and Mahrt (1997) are in intact time serial to be different from our results which are filtered. So it is difficult to direct compare. At least these results are no inconsistency.

Foken T, Wichura B: Tools for quality assessment of surface-based flux measurements. Agric For Meteorol 78: 83-105, 1996.
Vickers D, Mahrt L: Quality control and flux sampling problems for tower and aircraft data. J Atmos Oceanic Technol 14: 512-526, 1997.