1) In general the discussion remains mostly on a technical leveland not much interpretation of the physical processes related to the intrinsic nonstationarity is offered. Also, while data collected in an urban boundary layer are used, specific considerations of the effect of the urban complexity are missing in the analysis. This could be possibly done by proposing the analysis also for data collected in a homogeneous site, when available: the authors might consider performing this additional work.

Response: The reviewer has proposed many important research directions. In this paper, we have demonstrated that the intrinsic non-stationarity (IN) is a small-scale inertial sub-range phenomenon and would be an intrinsic property of turbulence. The physical process of IN may be related to the non-Gaussian cascade process of small-scale turbulence. The understanding of this process relies on the in-depth and systematic knowledge of basic characteristics of IN.

This paper only focuses on the main characteristics of IN in the CO2 turbulent fluxes and its effect on the calculation of CO2 turbulent fluxes, due to the limited data collected in this work (only single-layer turbulent wind speed and CO2 concentrations are collected). The purpose of this paper is to arouse the attention of readers to the systematic errors of the eddy-covariance method caused by IN. About the physical process of the IN, the influence of the urban boundary layer on the IN, and the characteristics of IN on different terrains, we hope to discuss these problems in other papers after collecting more data and running numerical simulations.

Considering the importance of the problems proposed by the reviewer, we added them in the form of outlook in the Conclusion of the revised paper. In order to clarify the subject of this paper, we revised the title as ‘Characteristics of intrinsic non-stationarity and its effect on eddy-covariance measurements of CO2 fluxes’.

2) L11-16: the first part of the introduction is rather general and goes far beyond the specific topic and purpose of the work presented: a more focused introduction would be worth, with more specific references to the related literature.

Response: As the reviewer commented, the first paragraph goes far beyond the specific topic and purpose of the work. Thus, we decide to delete the first paragraph in the revised paper. The first two lines of the second paragraph are intended to briefly explain the significance of the research subject of this paper, that is, the CO2 vertical exchange. However, the sentences are too ambiguous to cause misunderstanding. We have rewritten these sentences and add the related literature in the revised paper.

3) L58-60: the improvement achieved using the DFA method and the reasons to choose it should be better introduced and explained, citing the original publication is not enough.

Response: The statements are indeed somewhat inaccurate and vague. The fluctuation analysis (FA) was firstly proposed to detect possible non-stationarity in the data. However, the intrinsic non-stationarity and the non-stationarity caused by external forcing always coexist in a real time series. The FA can't distinguish the two kinds of non-stationarity. The DFA method was then proposed to resolve this problem by detrending large-scale trends in the data. That's why we choose this method in this analysis. We have corrected the misleading statements in the revised paper.

4) L72: is there any specific reason why n=1 was chosen for this study?

Response: The n=1 was chosen because it uses the simplest interpolation (linear interpolation) and has the similar results as high-order interpolations (see Fig. 3a).

5) L84-90: besides the description of the instrumentation, given that the data cover a period of just one month in Summer time, some details on the typical meteorological conditions of the area and on how the urban geometries may interfere with and affect the incoming flow would be worth. Also, being Summer time, one can expect that the main pollutant emissions are due to traffic and possible

industries: some information on this could be added.

Response: In fact, the topics mentioned by the reviewer, such as the typical meteorological conditions of the observation site, the effect of the urban geometries and the carbon emissions of the observation, have been extensively studied in Cheng et al (2018) that has been cited in the paper. Cheng et al (2018) used about 4 years and multi-layer data in their analysis. We only collected one month single-layer data and some data have been discarded by the quality control. Besides, as we have mentioned in the first response, the research subject of this paper does not involve the impact of meteorological factors and emission sources on IN. Therefore, we intend to recommend the paper of Cheng et al (2018) and references therein to readers. We have added some information to make this clear in the revised paper.

Cheng, X. L., Liu, X. M., Liu, Y. J., and Hu, F.: Characteristics of CO2 Concentration and Flux in the Beijing Urban Area, Journal of Geophysical Research: Atmospheres, 123, 1785–1801, 2018.

6) L91-96: since the work of Vickers and Mahrt (1997) is used not only for their quality control method, but also later on as a reference for discussing the results and the approaches to diagnose the non-stationarity, a brief summary of their method and paper content would be worth.

Response： We have added a brief summary of their method and paper content in L94-96 in the revised paper.

7) L103-104: the averaging time for this type of analysis is generally chosen as 900, 300 and 60 seconds: the use of a 6-s averaging should be explained and supported; why didn't the authors consider a 60-s-order average? The transition from 900-300 s time averaging to 6s is rather sharp, and it is not surprising that the fluctuation functions differ. It would be interesting to consider an intermediate averaging time.

Response: We did not choose the averaging time shown in Fig. 3 according to the principle of equal intervals. The 900s and 300s are chosen because they are commonly used in the eddy-covariance method (for example, see Doran, 2004; Metzger et al, 2007; Li and Bou-Zeid, 2011; Donateo et al, 2017). We choose them to indicate that the IN exists for the commonly used averaging time.

The choice of 6s has two purposes. First, we note that the crossover scale is about 20-30 sec. Thus, one naturally guesses that if using a timescale much smaller than the crossover scale the crossover phenomenon would be changed. In order to show this phenomenon in sharp contrast, we choose a timescale of 6s far away from the crossover timescale and we indeed observed that the crossover moves to a smaller scale. Second, the fact that the inertial-subrange turbulence is also partly removed with the removal of IN can also be shown in sharp contrast (Fig. 3c). This finding clearly reveals the possible relationship between the IN and inertial-subrange turbulence. It can be seen from Fig. 3c that the lowest frequency of inertial subrange is about 0.02Hz-0.03Hz, and the corresponding maximum time scale is about 33s-50s. The 60s scale suggested by the reviewer is not in the inertial subrange, and the choice of 60s cannot achieve above two purposes.

Some discussions on the choice of timescales in Fig. 3 have been added in L105-108 in the revised paper.

Doran, J.C. (2004). Characteristics of Intermittent Turbulent Temperature Fluxes in Stable Conditions. Boundary-Layer Meteorology 112, 241–255.

Metzger M, McKeon B.J and Holmes H (2007) The near-neutral atmospheric surface layer: turbulence and non-stationarity. Phil. Trans. R. Soc. A.365, 859–876.

Li, D., Bou-Zeid, E. (2011). Coherent Structures and the Dissimilarity of Turbulent Transport of Momentum and Scalars in the Unstable Atmospheric Surface Layer. Boundary-Layer Meteorol 140, 243–262.

Donateo, A., Cava, D. and Contini, D (2017). A Case Study of the Performance of Different Detrending Methods in Turbulent-Flux Estimation. Boundary-Layer Meteorol 164, 19–37.

8) L118: an introductory sentence explaining why the OU model is used here would be worth.

Response: We used the OU model here for two reasons. First, it has the crossover characteristics. Similar to the data, it is non-stationary at small scales and stationary at large scales. Second, it is very simple. The statistics (including the fluctuation exponents) of this model can be solved analytically (Czechowski and Telesca, 2016). We have added above discussions in L126-128 in the revised paper.

Czechowski, Z. and Telesca, L. (2016). Detrended fluctuation analysis of the Ornstein-Uhlenbeck process: Stationarity versus nonstationarity, Chaos:An Interdisciplinary Journal of Nonlinear Science, 26, 113 109.

9) L130-131: why now a 5-minutes Reynolds average time is used?

Response: We here choose 5-min as an example to illustrate the fittings of the OU process and the effect of IN on the test of non-stationarity. The 5-min average time is commonly used in the eddy-covariance method, please see the references listed in the respond of 7). Besides, as we have shown in Fig.3b, as long as the Reynolds average time is not in the inertial subrange, the IN is kept intact in the time series and the results will not change significantly.