

Interactive comment on “Intermittent turbulence contributes to vertical diffusion of PM_{2.5} in the North China Plain” by Wei Wei et al.

Anonymous Referee #1

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General comments:

In this paper an index, called intermittency factor (IF), is proposed to quantify the strength of intermittent turbulence in the atmospheric boundary layer (ABL) and applied to study the contribution of intermittent turbulence to vertical diffusion of PM_{2.5} in the North China Plain (NCP). The observational data are all from Tianjin, one of the megacities in the NCP. These data include high-resolution (10 Hz) 3D wind and virtual temperature data collected at 3 levels (40m, 120m and 200m) as well as temperature and RH at 15 levels on a meteorological tower, hourly measurements of PM_{2.5} at 3m, and horizontal and vertical wind data from a wind profiler radar. The strength of turbulence in the ABL and vertical profiles of wind and temperature are obtained and used in the analysis of variations in the PM_{2.5} concentration. The authors have not made

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systematic investigations instead they have focused on two cases containing pollution cumulative stage (CS) and transport stage (TS). A new technique, the so-called arbitrary-order Hilbert spectral analysis (HSA) is used for the derivation of IF. The validity of arbitrary-order HSA method in the identification of turbulent intermittency in the ABL was confirmed by the authors in their previous studies. It is shown that intermittent turbulence generated by low-level jets (LLJs) and downwards transported is a key driver for the diffusion of PM_{2.5} near the ground.

The IF index proposed in this paper seems to be a good scale to describe turbulence intermittency and can be useful in the analysis of accumulation and dispersion of pollutants in the ABL. The results of this paper are generally sound and can better our understanding of the role of thermodynamic processes in the variations of PM in the ABL. This paper is within the scope of ACP and generally well written. I recommend publication of this paper in ACP after addressing some issues.

Major comments:

(1) There are substantial differences between the CS and TS in TKE, u^* , and even W and U (Fig. 2). One can use these quantities in the explanation of accumulation and diffusion of PM_{2.5}. Why do we need an IF index? In other words, what is the advantage or superiority of using IF compared with other quantities? This point should be discussed in the paper.

(2) Are there any significant correlations between IF and TKE as well as other parameters? If there are, they should be presented and discussed.

(3) Your measurements are from Tianjin, which is just west of the Bohai Bay. The emission and formation of PM_{2.5} over the land areas are much stronger than over the sea. Therefore, I guess air from the Bohai Bay was much cleaner. During each TS the prevailing wind was either southeasterly or northeasterly, different from that during the CSs. Did the change in horizontal air flow contribute also to the decrease in the PM_{2.5} concentration? And how significant?

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(4) You show in Fig. 3 vertical profiles of changes in potential temperature during the CSs. I think similar results for the TSs should be presented and discussed as well. You may show how rapidly the stable condition formed during the CSs was broken by intermittent turbulence. In addition, some discussions about the evolution of the PBL height may be also good for a more complete picture.

(5) Your results and conclusions are based on cases study. I think it is better to add “cases from Tianjin” or similar subtitle. And “vertical diffusion” in the title can be questionable if you cannot prove that the decrease in PM_{2.5} was solely due to the vertical diffusion.

Minor points:

Page 1 line 21: What do you mean by “wind filed”? Wind profile?

Page 2 line 21: Define “FI”.

Page 2 line 32: Delete “respectively”.

Page 3 line 4: Change “east” to “southeast”.

Page 3 line 11: I think “HMP45C” is the type name of the probe and should be put in the brackets.

Page 3 line 16: Was the TEOM system installed near the tower or the WPR? Please make it clear.

Table 1: “c: 300-366 m s⁻¹”. Is this the range of wind speed that the sonic anemometer can measure? 300 is a very strange number here.

Page 4 line 16: Change “poor data” to “poor quality of data”.

Page 4 lines 19-21: What are the criteria for data that are suitable for this study?

Page 5 line 2: “local standard time” or “Beijing Time”?

Page 5 line 9: “On this basis”? It is not clear what is denoted.

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Page 5 lines 11-12: Delete “(CSAT3, CAMPBELL Inc., USA)” because the same information is given on page 3.

Page 5 line 19: Do you mean the local maxima that are found within every 30-min periods?

Page 5 line 9, page 6 lines 12-13, and page 11 lines 18-19: You are proposing or defining IF at these three places. This is redundant. I think you should define the IF index at a suitable place and use it elsewhere.

Page 6 line 31 and page 7 line 1: “. . .increased to a maximum of 412 ug m⁻³ for PM_{2.5} and then dropped to a low level within a few hours no matter for Case-1 and Case-2”. Please check you expression. I do believe the maximum values in Fig. 2a and Fig. 2b are all 412.

Page 9 Fig. 2: Please add some ticks on the Y-axes of Figs. 2a and 2b.

Page 11 line 2: CSs or TSs?

Page 11 Fig. 5: Does each concave curve represent a 30-min result? Please make it clear.

Page 11 line 24: Delete “site”.

Page 13 line 2: “under stable conditions”? Are you not talking about the TS?

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