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Interactive comment on "Understanding meteorological influences on PM$_{2.5}$ concentrations across China: a temporal and spatial perspective" *by* Ziyue Chen et al.

Anonymous Referee #2

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This paper attempts to investigate the meteorological influence on PM2.5 concentrations in China at the national scale using a convergent cross-mapping (CCM) method. This method is somewhat new to the atmospheric chemistry community, but the physical mechanism as discussed in this paper is very descriptive and already well-known. Overall I don't feel these results are significant enough to warrant publication in ACP. Here are my major concerns.

First, the authors just use the PM2.5 observations in one year, from Mar 2014 to Feb 2015, which is far from sufficient to draw any convincing conclusions. In Figure 2, they evaluate the influence of 8 different variables on PM2.5 in each season. This

means they make these conclusions using only ∼90 data values, which is far from enough. When the authors prepare this manuscript, observations in 2015 and 2016 should already be available. Why not include a longer time series of observations into this study?

Second, the discussion of the scientific significance of this work looks very superficial and unprofessional. Throughout Section 5.1, the authors made a lot of descriptive statements with little reference. For example in Line 410-413, the authors claim that rising PM2.5 concentrations prevents the occurrence of winds. Is this true? Can the authors list some references? In my understanding, the effect of aerosols on wind occurrence is much smaller than that from synoptic circulation patterns.