The manuscript developed an environmental meteorology index (EMI) and use this index to further quantify the contributions of meteorology and emission control to the air quality improvement in China from 2013 to 2019. A major concern raised by a previous reviewer is about the inclusion of emission in the definition of EMI. After reviewing the manuscript, review comments and the authors' response, I sided with the previous reviewer, and think it is a valid concern regarding the emission term in the calculation of EMI. This issue needs to be thoroughly addressed.

1. Including Emis (emission) in their eq. (3) is important for diagnosing accumulation potentials or dispersing potentials, as illustrated in Fig. 3 and discussed in the last paragraph of page 14 and the first paragraph of page 15. Without emis, it would be hard to see how pollutant accumulates or disperse, as the accumulation or dispersion depends not only on meteorology but also on emissions. So for the purpose of calculating accumulation or dispersing potentials, it is fine to include Emis in Eq. (3). In this regard, I agreed with the authors that the exact definition of EMI will depend on scientific objectives the index is used to address.

A: Thank you for agreeing with this point.

2. But for the current manuscript, the most important goal is to diagnose contributions of meteorology and emission control to the air quality improvement in China. In the manuscript, Eq. (6) is used to calculate the sole contribution from emission control. One critical term in Eq. (6) is PM (m0, e1), the PM concentration under conditions of unchanged meteorology at p0 but with new emission e1. To get this, the authors then used the ratio of EMI(p0)_bar to EMI(p1)_bar (Eq. (7)). And the authors claim that this ratio can be used to "reflect the impact ratio of sole meteorology variations on the concentrations between p0 and p1 with the same emission at p1" (page 16, lines 10-12). But this argument is flawed, as the formula of EMIS includes emission contribution (Emis in Eq. (1)), as pointed out by the previous reviewer. As emission differences also contribute to the difference in EMI between p0 and p1, the authors can not attribute the ratio EMI(p0)_bar/EMI(p1)_bar to the pure impact of meteorology. So the primary objective of this manuscript, I agreed the previous reviewer and do not think it is appropriate to include the emission term in EMI.

A: We agree partially with the reviewer's comments. Let's clarify each term in Eq. 7 to better explain the assumption used in the manuscript. PM(m1, e1) is the OBSERVED PM concentrations at p1 with emission e1 and meteorology m1. PM(m0, e1) is a hypothetically non-measurable quantity, indicating the PM concentration at p1 with emission e1 and meteorology m0, that does not exist in reality but needs to assess the sole impact of emission changes. EMI(p0) and EMI(p1) are the simulated quantity of equivalent PM concentrations at p0 and p1, respectively, with a fixed emission (Emis in Eq. 3) but with separate meteorology of m0 and m1. If we re-write the Eq. 7 as follows:

$$\frac{EMI(p0)_{2.5}}{EMI(p1)_{2.5}} = \frac{PM(m0, e1)}{PM(m1, e1)}$$

which assumes that under the same emissions, the ratio of EMIs under two meteorology (m1, m0) equals to the ratio of PM concentrations under the same two meteorology (m1, m0). This is another reason that the emission term (Emis) is needed in the EMIs, otherwise the above equation (assumption) cannot be derived. We agreed with the reviewer's comment that "emission differences also contribute to the difference in EMI between p0 and p1". It is true that, in reality, the changes in PM concentrations or EMIs are not a linear addition of changes in emission and meteorology as assumed in Eqs. 6 and 7. This limitation has been included in Section 2.4 and Conclusions.

3. It is also important for the authors to quantitatively evaluate the effectiveness of the proposed EMI index for the attribution. For example, it would significantly strengthen this manuscript if the authors can use their CTM to calculate the contribution of emission control to the air quality improvement by perturbing emissions, and then use the CTM results to evaluate the results diagnosed from EMI.

A: Yes. We have done this evaluation and have added the results in the manuscript. Following texts and a Table was added to the manuscript.

The applicability of EMI to assess the meteorology and emission changes is also evaluated by results from a full chemical transport model (MM5/CUACE) and observational data for $PM_{2.5}$ in China for Novembers of 2017 and 2018. The averaged $EMI_{2.5}$ and observational data for the two months were used to estimate the emission change ratio (E-Ratio in Table 2) by Equations 6-7 from 2017 to 2018. In order to evaluate the correctness of this emission change estimate, the E-Ratio was used to adjust the emissions for November 2018 from the base emissions of the same month for 2017, which were then implemented in the MM5/CUACE to simulate the $PM_{2.5}$ concentrations for the two months, respectively. If the simulated concentration differences (M-Ratio) for the two months were comparable with the observed concentration differences (O-Ratio), it can be concluded that the emission change estimated by the EMI framework was reliable and could approximately represent the actual emission changes. Table 2 summarizes the analysis results of this evaluation for six typical cities. It is clear that the O-Ratios for the six cities are very comparable with M-Ratios, indicating that the EMI framework can be reasonably used to estimate the emission changes over time.

Table 2: Comparison of $PM_{2.5}$ Concentrations in Novembers of 2017 and 2018 from Ambient Observations and from CTM Simulations by EMI-derived Estimated Emission Changes

City	**$EMI_{2.5}$**		**Observations**			**Emission Changed**	**CTM Simulated**		
	2017	**2018**	**2017**	**2018**	**O-Ratio**	**E-Ratio**	**2017**	**2018**	**M-Ratio**
Beijing	1.8	3.6	45.7	72.8	1.59	**0.80**	42.3	67.5	1.59
Shanghai	2.7	2.6	42.0	40.1	0.95	**1.00**	52.7	51.2	0.97
Jinan	3.3	4.9	57.1	85.8	1.50	**1.02**	62.4	90.9	1.46
Xian	2.4	2.7	94.8	84.7	0.89	**0.79**	95.1	86.9	0.91
Zhengzhou	4.3	6.2	73.9	100.4	1.36	**0.96**	80.4	91.1	1.13
Shenyang	1.8	2.7	40.2	48.9	1.21	**0.82**	73.3	120.1	1.63