

Interactive comment on “Measurement report: Seasonal, distribution and sources of organophosphate esters in PM_{2.5} from an inland urban city in southwest China” by Hongling Yin et al.

Anonymous Referee #2

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Thanks for the invitation to review. I read the manuscript by Yin et al. with interest. The authors reported concentrations of seven OPEs in PM_{2.5} from Chengdu, China, tracked their possible sources, and conducted source apportionment using PCA and the PMF receptor model. My utmost concern is the data accuracy as some required QA/QC procedures were missing. Additionally, the manuscript is a little hard to read as it has a number of grammatical issues, and several statements lacked reference supports. Though this study provided a few useful information (e.g., difference in OPE profiles between inland and costal cities), its novelty and quality at current version

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may not be sufficient enough for the Atmospheric Chemistry and Physics. My specific comments are as follows:

Major concern: Novelty: There is a similar study previously conducted by the leading author here. What makes this manuscript distinct from that previous one? Authors should elaborate more the novelty of this study. QA/QC: 1) As no surrogate standards were spiked prior to sample treatment, how did authors evaluate OPE recoveries from the analytical procedures?; 2) How was the matrix effect assessed and compensated?; and 3) The data from field blanks were missing. PMF model: How were the uncertainties determined? Which references were referred to for identification of sources associated with each factor? I also want to see the source profile of each factor.

Minor concern: Line 8: “emerging contaminants” → “contaminant of emerging concern”. OPEs have been produced for decades. Line 9: “centers” → “areas” Line 13: “. . .which TOGETHER made up. . .” Line 18: OPEs can transfer from soil to air particles via suspension and volatilization as well. Actually, authors mentioned this at Lines 303-304. Lines 32-35: A weird sentence, please rephrase it. Line 35: Reference is needed for the “OPE restrictions”. Lines 38-39: Reference is needed. Line 45: Which type of matrix is referred to for “Concentrations of OPEs in most cities. . .” I looked at the references cited, but not all of them talked about PM_{2.5}. Lines 54-56: How about “Chengdu is an important city in Southwest China due to its role as a national high-tech industrial base, a commercial logistics center, and a comprehensive transportation hub”? Line 82: Sampling intervals? Line 86: Was the analytical method used here applied in any previous studies? Lines 93-94: How about “The latter eluate was collected and concentrated by vacuum-condensing. . .”? Lines 114-118: Could concisely say “detected in virtually all the samples”. Lines 120-121: Did “The average value. . .four seasons” mean “annual average level”? Line 141: Rephrase the first sentence. Lines 143-145: Explain the meaning of values in the parentheses. Lines: 165-167: References? Lines 182-184: A recent study measuring an extended list of OPEs in the Great Lakes atmosphere also found that alkyl OPEs dominated OPE compositional profiles of urban

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air collected from Chicago and Cleveland (Wu et al. 2020; 10.1021/acs.est.9b07755).
Line 208-210: OPE levels can be surely affected by temperature, so I suppose the authors would like to say “seasonal variations in OPE levels”. Additionally, would meteorological parameters other than temperature result in the seasonal variations found in the present study? Lines 236-238: Has been mentioned before. Lines 238-248: Out of place here. Could be moved to section 3.1. Line 257: Need reference to support “they tend to be adsorbed in PM 2.5”. Line 315: Other factors may lead to such difference between indoor and outdoor OPEs. For example, TBEP has the shortest atmospheric half-lives, which may explain why its dominance in indoor samples was not observed for the outdoor counterparts. Lines 350-356: References are required for identification of possible sources associated with each factor.

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