

Interactive comment on “Ambient measurement of shipping emissions in Shanghai port areas” by Xinning Wang et al.

Anonymous Referee #2

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General comment The paper regards an analysis of the impact of shipping to atmospheric pollutants measured in the area of Shanghai harbour (China). The approach used is based on the identification and characterisation of ship plumes using high temporal resolution measurements of gaseous pollutants and of particles using a SPAMS. The work is interesting and allowed to investigate the typical spectra of particles released by ships as well as to evaluate statistically the contribution of shipping to local air quality. The work is suitable for the Journal and generally well written (even if minor spell check is required), however, some aspects are not completely clear (see my specific comments) and an additional effort in the discussion of size distributions of the impacts should be included. In conclusion, I believe that the paper should be considered for publication after a major revision.

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Specific comments

Title. I think that it is not correct to speak of “measurements of shipping emissions” because emission factors or measurements of specific emission rates are not given. I would suggest to change the title to put in evidence the core of the work: contribution of shipping to atmospheric pollution.

Introduction. The adoption of a DECA (Domestic ECA) is quite interesting and it would be even more interesting if a more detailed discussion is included. For example, it would possible to comment on the efficacy of this measure in reducing the impact of shipping on local pollution levels. It is also worth to mention that a recent work (Contini et al., 2015 - Atmospheric Environment 102, 183-190) showed that application of “domestic” restrictions on the fuel quality could be effective in reducing not only local SO₂ concentrations but also primary emissions of particles from ships. I believe that a discussion on this aspect would be appreciated by the readers.

Sections 2.3 and 2.4. It is often mentioned the high temporal resolution of SPAMS measurements, I would suggest to explicitly report the numerical value.

Page 6 (lines 1-5). V-particles measured without the presence of SO₂ peaks are interpreted as due to the use of low-sulphur content fuel, however, it would not be possible that they are coming from other industrial (or anthropic in general) sources? Some words on this should be included.

Page 7, line 23. To speak at this level of BC is not really useful, likely authors mean EC.

Page 8, lines 2-3. This sentence is not clear and should be re-written. I believe that authors means that ultrafine particle concentrations could be a better metric compared to mass concentrations to investigate the impact of shipping to atmospheric aerosol.

Page 9, line 30. The approach based on this formula was originally developed in Contini et al (Journal of Environmental Management 92 (2011) 2119e2129) and suc-

cessively used by other authors. I believe that it would be fair to mention this aspect.

Looking at the size distributions reported in figures 4 and 6, it appears that V particles are especially relevant for ultrafine particles, however this aspect is not deeply investigated on the evaluation of the impacts. It would be possible to use the approach discussed on page 9 to investigate the size dependency of the impacts of shipping, eventually estimating the impacts for different size ranges. I believe that, if a sufficient statistics could be obtained, this will give very useful additional information compared to the impact on total particle number reported in Table 2.

Page 10, lines 13-23. The comparison with shipping impact measured in other ports is certainly interesting, however, it is done on relative impacts and not on absolute contributions due to shipping activities this means that it depends not only on ship traffic but also on the contributions of the other sources acting on the specific measurement site. This should be mentioned because it could explain some of the apparent discrepancy mentioned by the authors. In addition, I would suggest to expand the comparison to other ports analysed with the high temporal resolution approach (Merico et al Transportation Research Part D 50 (2017) 431–445) but also with other complementary approaches (see for example Viana et al 2014 Atmos. Environ. 90, 96–105).

Regarding the impacts reported in Table 2, it would be possible to estimate the uncertainties?

Page 11 line 5. This sentence is not clear. Authors likely mean that the impact of shipping is more relevant and clearly discernible on SO₂ and V particles compared to the other pollutant analysed. Could authors clarify?

In the supplementary material it is reported “...in present study the online single particle measurement was utilized to indicate the occurrence of shipping emission plumes...” however in the main text was mentioned that both particles and SO₂ concentrations were used. Please clarify this apparent contradiction.

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Minor corrections

Page 1, line 15. Better “particle size distributions”.

Page 1, line 28. Please eliminate the initial S.

Page 2, line 19. Subscript for SO₂. The same in page 4 (line 25).

Page 7, line 14. Better “different size distributions. . .”

Page 8, line 12. Better “by the dominant”. In addition, I would remove etc, if necessary please mention explicitly.

Page 8, line please remove etc. as above.

Page 8, line 25. Better “is therefore not attempted. . .”

Page 9, line 2. > 0.5 μm

Page 11, line 10 ozone without capital letter.

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