Atmos. Chem. Phys. Discuss., 12, C10766–C10769, 2012
www.atmos-chem-phys-discuss.net/12/C10766/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigation of gaseous and particulate emissions from various marine vessel types measured on the banks of the Elbe in Northern Germany" *by* J.-M. Diesch et al.

Anonymous Referee #1

Received and published: 19 December 2012

As there are many factors that influence ship emissions and still few measurements of these under real conditions available the paper is a valuable contribution to investigating the influence of ship emissions on air quality. However, the presented results do not reflect all relevant influences properly. Thus, the improvements and the remaining lacks should be worked out more clearly. Without correction, proof or removal of the findings that are in my opinion unjustified (see below) I cannot recommend to publish the paper. Despite of this the paper is written clearly, well structured and readable.

Major: In section 2.5. the authors describe that they determined different emission factors discerning ships by type and size (which is proportional to the engine power as

they say). They do not take into account engine and propulsion type which has also an important influence on emissions. Considering this, there might be some restrictions in the applicability of the EFs which is confirmed by the (partly) large error ranges. This should be discussed in the paper.

292, section 3.2, 3.3, Table 2: It is in my opinion questionable to speak of a correlation if the coefficient is low and not having tested it. I would assume that a correlation coefficient below 0.3 means no significant correlation. Please do not speak of low correlations in such cases without a statistical proof. There's obviously no correlation between EFs and the engine power. I'm quite convinced that there would be a correlation with the load, i.e. the percentage of the power actually used. The load could be estimated by comparing the design speed of the vessel to its actual speed. However, in the optimum case also the engine type and type of propulsion should be considered. Some modern ships with diesel electrical engines, e.g., try to keep the load as constant as possible.

306: "A lower vessel speed implies a lower engine load and less complete combustion process for which reason BC is formed." Generally yes, but this depends on the engine/power/design speed. How do the authors come to their conclusion.

456: The point cloud in Fig. 5 doesn't justify this statement in my opinion. The authors should use an appropriate statistical test to underline their statement.

468: "The higher the gross tonnage levels the higher the NOx EFs [...]" This is for me another example for an unproved assertion.

483ff: what means "strongly"? There appears to be a difference between Type 1 and the other types, but no difference between Type 2 and 3. This at least is what I get from fig. 7 and table 1.

490: How do the authors derive a third mode above 100 nm for "high BC emitters" from fig. 7? I can only see that the curve flattens out, but this doesn't justify to assume an

ACPD

12, C10766–C10769, 2012

Interactive Comment

additional (number) mode.

In line 442 the authors write: “Higher black carbon concentrations imply a larger surface area where potential new particle formation precursors condense onto instead of nucleating. Additionally, high black carbon emissions inhibit the growth of freshly formed particles as they are scavenged while coagulating.” and in line 486: “[...] and “high BC emitters” emit particulate matter composed of freshly formed particles likely from sulfuric acid nucleating in the expanding plume [...]”. Isn’t this a contradiction?

526: “[...] also the engine type and operation additionally play important roles.” This is certainly true, not only for particulates but also for NOx as test bed measurements confirm. There are, however no convincing verifications of this statement in the paper. Maybe the distribution of the vessels into 3 type classes or the criteria by which they are discerned are not suitable.

559: “[...] the gross tonnage is an important parameter [...]” Considering the GT alone is not enough.

Section 5: My general impression is that the emission factors proposed by the authors reflect very general emission factors for average ships, useful e.g. for estimating global emissions or bulk emissions for ECAs. They are not suitable for detailed case studies, estimating emission changes when changing the fleet composition or the like. Provided the authors agree to my impression, this should be worked out in the conclusions. Further, the discussion and conclusions might be rethought after removing findings that cannot be verified from the data.

Minor:

The authors use frequently the word parameter for measured quantities. I’m aware that this has become common practice. It is, however, at least ambiguous. A parameter is by definition a fixed value that cannot be measured but calculated or estimated. The mean of a sample population is for example a parameter that is derived from a

ACPD

12, C10766–C10769, 2012

number of variable measurements. I'd suggest to use (measured) variable, quantity, concentrations, ...

Line 139: A table to summarize the the instrumentation and setup would be useful.

146: remove "respective". "relative small" relative to what? I wouldn't put the important information of this sentence into brackets.

175: Can it be excluded that part of the organics are also detected by the MAAP?

223: [. . .] and PC is assumed to be relatively small. On what is this assumption based?

246: achieved instead of reached.

249: Did the authors check this or can they give a reference?

264 Does this have a notable influence on the EF?

450: How did the authors find out the engine load?

Figure 7: What is the point in showing the curve for all vessels? The many curves and (useful) error bars decrease the readability of the figure.

515: marine fuel oil: Is this heavy (or residual) fuel oil? Marine diesel oil has typically a lower S-content

---

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 22269, 2012.