

## ***Interactive comment on “What can we learn about ship emission inventories from measurements of air pollutants over the Mediterranean Sea?” by E. Marmer et al.***

**Anonymous Referee #1**

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### General comments

The overall quality of analysis is very good, and this paper is an important contribution to the field. As the abstract ambitiously outlines, this paper provides important analysis of independent (or semi-independent) marine shipping inventories using an atmospheric chemistry transport model to evaluate the sensitivity of impacts assessment among these inventories. Using comparisons with field observations, the authors find that these inventories are similarly consistent in many respects and report their differences in some detail. This sort of analysis has been recommended in a short article in Atmospheric Environment's "New Directions" and has been pursued to vary-

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ing degrees for shipping, perhaps since the Capaldo et al paper in Nature in 1999. Importantly, this study presents a well-designed analysis, particularly in its comparison of regional and global ship emission inventories, and a first detailed analysis for the Mediterranean region.

The paper in some detail focuses on inventory differences, although the point most clearly (but indirectly) conveyed is this: Despite differences among inventory resolution, methodology, and assumptions, the most remarkable characteristic of current state-of-art ship inventories is the convergence of estimates. This convergence reveals two very important insights: 1) Most importantly, the detailed comparison of the EMEP, EDGAR FT, and other inventories for ship emissions shows the importance of regional inventories that can include what is often labeled domestic shipping (and/or other local shipping activity) not typically included by regional inventories for all domains; and 2) that accurately representing patterns of shipping representation in spatial inventories are beginning to matter as much as capturing local ship operating conditions. This is largely achieved by the decade or more research efforts that improved transparency in activity data/assumptions and converging agreement on emissions factors.

Remaining work is also revealed in this paper, where the dependency of air quality model results on the ship inventory applied is quantified. The good effort to constrain emissions inventories with limited field observations is also an important contribution that reveals where additional observing capacity can contribute new information.

My main critique is that these three insights could be summarized more explicitly, and that the claim that inventories of emissions from ship "diverge widely" be modified. The last paragraph does this a bit, but not as well as it might. Confidence is rather better on a global basis than many other global sectors and better than some national inventories, I believe. The statement in the abstract and elsewhere that the difference can be up to two orders of magnitude is hyperbole anchored to the emissions rates for non-methane volatile organic compounds – a pollutant factor emitted in very low quantities from diesel combustion, but in rather higher rates from petroleum cargoes.

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The NMVOC emission rate is likely to converge rapidly through studies like this one, and could already be known better than reported here based on the selection of studies for emissions rates comparison. Moreover, the study shows that the effect of NMVOC ship emissions change surface concentration within 3% - attesting to the point that marine diesel engines are rather low-emitters of NMVOCs compared to other VOC sources in the study area.

So the most salient points well demonstrated by this paper appear to be:

1. All current regional and global inventories point to the significance of ship emissions in potential impact to the region.
2. Including domestic and international shipping together in the marine emissions inventories matters, at least and especially where shortsea, coastal, or domestic shipping is significant, because these activities are often not captured by global inventories.
3. Differences in shipping patterns matter to the evaluation of impacts, as shown in the subregional comparisons in this study.
4. Differences in total emissions estimates (including both domestic and international shipping) have converged within the confidence of current observations at this time.

Specific comments

1. Page 7156, line 1: Where is the two order of magnitude difference? Not recently observed in large-scale global models, even across regions around ports or heavy traffic regions. Not among port-based inventories, as far as I know. This appears to be generalized from a specific pollutant (NMVOCs), and may be misleading to readers.
2. Page 7156, line 6: This statement is overstated at least. Evaluating the validity and consistency of ship inventories has been a primary focus of many, and this work implements a 2003 New Directions (doi:10.1016/j.atmosenv.2003.08.003) call to "design experiments that pursue closure and enable critical uncertainties in the linkages between emissions and atmospheric impacts to be reduced." The work to develop net-

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work models for shipping published by Wang et al, ES&T, 2007 demonstrated such evaluations as did the later comparison cited in this paper as Wang et al, 2008); the older work evaluating the calculation system for Finnish waterborne traffic emissions (MEERI, <http://lipasto.vtt.fi/meerie/index.htm>) also worked to do this, although without field observations as done in this work; the very recent work published by Lack et al, JGR-Atmospheres, 2009 also demonstrated this with a strong emphasis on field observations.

3. Page 7156, lines 19-21: So densely populated regions, i.e., regions with significant industrialization and/or human activity, contribute relatively more pollution than ships in such regions, even as they attract heavy ship traffic activity.
4. Page 7156, lines 22-27: And additional work is needed to improve the correlation among inventories and observations. This appears to be worthwhile, where EMEP has provided such work and produces the best match. This work is recommended as the next best way to reduce uncertainties further.
5. Page 7157, line 22: Sentence reading, "The revised Annex VI will enter into force in July 2010" mistakenly implies to the general audience that the measures required by Annex VI will be implemented in 2010. I know the authors understand the difference, namely that the measures will be phased in over the decade following, but it could be made clearer.
6. Page 7159, lines 4-6: While clear, this may not be the best speciation. Please discuss with regard to the Lack et al, JGR, 2009 article at least. Where is the sulfur particle component? Or are you only referring to primary PM (so called)?
7. Page 7160, lines 7-10: Nicely stated.
8. Page 7167, lines 4-9: Could this be a function of the uncertainty in BC reported transparently by Bond et al? These differences seem to be very reasonable given the global nature of the Bond et al work cited, demonstrating in fact the opportunity for con-

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vergence in the BC inventories as has been achieved over time for most well-studied pollutant emissions. Moreover, despite a difference in Table 1 and Table 2 emissions of BC from ships, that span an order of magnitude, there is rather strong signals reported in Table 4 for the region studied that point to the importance of other BC sources even with the uncertainties in ship emissions rates or deposition (as discussed in line 15 on this page).

9. Page 7168, lines 26-28: Do the authors include NMVOC from cargoes or only engine emissions? This could contribute to the discussion, and may help explain the difference.

10. Page 7169, lines 11-15: Again, what would the effect be if cargoes emissions of NMVOCs were included?

11. Page 7170, lines 7-13: I don't understand what the interpolation does for the quality of the analysis, except to imply resolution for illustrative purposes only. This "sharpening" of the resolution is an exercise that does not contribute any definitive information, I think. I would reconsider including it, unless its contribution to the well-crafted logic of the paper is made clearer.

12. Page 7167, lines 4-9: Could this be a function of the higher BC attributed to Bond et al for ships? Check the ratios again and compare with Table 4.

13. Page 7171, lines 1-2: The fact that emission factors for NO<sub>x</sub> and SO<sub>2</sub> agree among inventories is not coincidence. This represents significant convergence among independent studies during the past decade. This convergence is even more remarkable for ship inventories at global and regional scales than for other global source categories, and should be highlighted.

14. Page 7171, paragraph beginning at line 21: Should start with a sentence that says: "We can also conclude that variation in regional patterns of ship activity, both spatially and temporally, may drive the variation in agreement among satellite, model,

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and observed concentrations attributed to ships. Then use one of these statements (the first is correct, I think): "This is a PRIMARY cause of remaining disagreement among the inventories, except where emissions factor differences are very large (e.g., BC and NMVOCs)." . . . or: "This is a SECONDARY cause of remaining disagreement among the inventories, behind the discrepancies among emissions rates (especially BC and NMVOCs).

15. Page 7173, lines 7-9: Please explain the sentence stating "Different seasonal patterns are given by global ship activity data sets ICOADS and AMVER and need more investigation (Dalsøren et al., 2008)." Did your work not consider seasonal variation when reviewing the selected inventories, only cited Dalsoren as evaluating this? Why doesn't this paper report what the current ACPD study finds? It is important that this paper directly evaluate seasonal differences as an explanatory item in this study, not simply cite another recommending investigation.

Technical corrections

1. Page 7157, line 8: Editorial comment: Authors could consider updating this with Lack et al, JGR, 2009.

2. Page 7171, line 14: What is "relative" about this certainty? The paper makes this conclusion certain, in absolute metrics. It is not relative at all that ships impact the region, given that you have used several inventories and all have shown this to be true.

3. Page 7159, line 18: Please check reference to Lloyd's 1999. I believe Eyring used a much more current version of Lloyds data - perhaps 2007. This implies many levels of inaccuracies that are not representative of the Eyring inventory (or the Wang et al inventories).

4. Page 7173, lines 3-5: The reference to biomass burning seems disconnected from the paper, not being discussed at all prior to this as an input tested for sensitivity, but only included among natural emissions "held constant".

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