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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Polycyclic aromatic hydrocarbons in atmospheric aerosols and air–sea exchange in the Mediterranean" *by* M. D. Mulder et al.

Anonymous Referee #2

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This paper on the air-sea exchange of PAHs in the Mediteranean is based on a unique dataset of air and water samples and gives new insights into the cycling of PAHs between different environmental compartments.

While the idea of the paper is compelling and in general suitable for publication in ACP the paper suffers from a variety of shortcomings. Thus, I can only support publication of this work after major revisions.

First of all, the paper would strongly benefit from a revision by a native speaker. Some sentences are obscure and it is not clear what exactly the authors want to say. A few

examples: p5967 l20,21 'Each one' [...] 'as far as possible identical manipulations on site' p5972 l16 'corresponds with no or beging hit of such areas' p5972 l16 'This perception' should probably be 'This finding' p5973 l17,18 '...PAH was almost exclusively quantified in particle size fraction <0.25μm' should probalby mean 'the majority of the particulate PAHs were found in the size fraction <0.25 μm'? p5974 l3 'FLT and PYR are indicated to be close to phase equilibrium' should probably mean 'were found to be' p5974 l24 'observable' should be 'observed'

Secondly, besides the title '... aerosols and air-sea exchange ...' there is only little information on the actual findings about PAHs in/on aerosols. Also the air-sea exchange was only investigated for 3 PAHs.

detailed comments:

ABSTRACT: In the Abstract (p5964 l1-6) Why are only the findings of particles <0.25μm discussed? What is the total percentage of particulate PAHs, how is the size distribution? It is later stated (p5966 l3) that particles were collected with 2.5, 1.0, 0.5, and 0.25μm filters. But the size distribution is never discussed in the paper. Further, with the strong focus on air-sea exchange at least a rough estimate about the total annual flux should be made. p5964 l10-12: 'It is concluded that future negative emission trends or interannual variability of regional sources may trigger the sea to become a secondary PAH source' How large could this source be compared to the primary sources in the Mediterranean?

METHODOLOGY: p5968 l3: Abbreviation LOQ should be explained the first time it is used.

Section 2.4 seems a bit too short. It should be made clear how kw and ka were calculated. Also it should be discussed whether the air-sea exchange is dominated by ka or kw. For most species one of the two mass transfer coefficients clearly dominates the exchange process. Also the difference between 2 and 4 ring PAHs should be discussed here (e.g. what values were used to describe the diffusivity of the different

species, was a temperature dependency taken into account). In general, readers of the paper should be able to reproduce the calculations done by the authors. Giving the formula and the reference to Schwarzenbach et al. is not enough here.

Section 2.5: Is river runoff considered as a source for PAHs? Is it an important source?

Section 2.6 is much too short (2 sentences) to justify an additional section.

RESULTS: Section 3.1: It should be discussed whether earlier crusies are comparable. Different seasons, different wind conditions, different cruise routes can potentially have a large influence on the observed concentrations. Table 2 should state precisely when the different cruises were taking place. The reference (1) in Table 2b is not used.

Section 3.2: As stated before there should be more information about the size distribution of the PAHs from the 2.5, 1.0, 0.5, 0.25μm filters.

CONCLUSIONS: The Conclusions seem to be very short. It would be beneficial to give an estimate about the relation of total annual RET deposition to net revolatilisation. Also there should be more information about the differences between the observed PAHs and on the observed particulate PAHs.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 5963, 2014.