We would like to thank reviewer 2 for taking the time to read our paper and giving us valuable comments. We have revised the paper by taking into account the reviewers suggestions and provide responses to the individual comments below.

Specific comments:

Page 21944, Line 19: *stricter control on sulphur emissions from 2010. Hard to un-derstand for the reader as the first paragraph of the abstract discussing regulations forgets to explain what happened to sulphur content (and emissions) in 2010. The abstract therefore discusses several air quality results without informing the reader about the premises. Please discuss the sulphur regimes before and after 2010 in the firstparagraph.*

This part of the abstract now reads:
In 2010 stricter regulations for sulphur emissions were implemented in the two sea areas, reducing the maximum sulphur content allowed in marine fuels from 1.5 to 1%. In addition ships were required to use fuels with 0.1% sulphur in EU harbours.

Page 21944, Line 23-25: *At the same time, however, an increase in ship activity has resulted in higher emissions and subsequently air concentrations ..... Suggest to state that this is for other components (not sulphur) as sulphur is discussed in the previousline.*

We have followed the recommendation from reviewer 2 here.

Page 21944, Line 27: *Maybe state that the decrease in emission is for the total emissions from all sectors to assure that this is not misinterpreted as for shipping only.*

We have added: from all sectors

Page 21946, Line 3: *The major fraction of sulphur is emitted as SO2 and not PM (table 1). But it is only mentioned that fuel sulphur reduction affect emitted PM.*

This part now reads:
Fuel sulphur reduction has a significant impact on emitted particulate matter (PM) and SO_2 a precursor for PM. Particulate matter is commonly associated to detrimental effects on human health.

Page 21948, Section 2: *How is chemistry in ship plumes treated in the*

model? Several recent studies have shown that NOx lifetime is significantly reduced in ship plumes and that this affects the calculated impacts of shipping on ozone and oxidation capacity. The model is run in quite high resolution which is good but information whether parameterization of plume processes is used is missing. If it is not accounted for the consequences for the results should be briefly discussed. This could for instance be done in a separate Discussion section at the end of the paper.

We have not included any separate treatment of the plumes from the ships. We have also run the model with hourly emissions rather than monthly averaged emissions. The difference in model results with monthly and hourly emissions were small. We have also added some text discussion the effects of plumes in the conclusions:

In our model calculations the ship emissions are instantly diluted. Model calculations with a parametrisation of the ship plumes suggest that with this approach we overestimate NO_x concentrations and ozone formation (Huszar et al. 2010; Vinken et al. 2011) Our results may not be directly comparable as we use a much finer horizontal model resolution. In addition Vinken et al. (2011) finds that the differences between the plume calculations and instant dilution are smallest over strongly polluted seas as the North Sea. This suggests that implementing a parametrisation of the ship plumes would not change our results significantly.

Page 21950, Line 19: *Emissions of organic and elemental carbon and ash also increase as they are assumed to be unaffected by the fuel type. Is this really the case? Lack et al. (2009) for instance finds that organic carbon is dependent on fuel sulphur content.*

This point was also raised by reviewer 1. This is now commented in the text: In reality, different amounts of ash in distillate (0.01 w-%) and residual fuels (0.07 w-%) are allowed as indicated by marine fuel standard (ISO 8127:2010; Chevron2012). In STEAM, ash emission factor is 0.06 g/kWh which corresponds to 0.03-0.04% (by mass) depending on engine specific fuel oil consumption. The values used in STEAM for ash emission factors are similar to the results recently reported by Moldanova et al. (2013) and more details of emission factors of PM components can be found in Jalkanen et al. (2012)."

Page 21951, Line 3-10: *I miss a short discussion on the assumptions about how a NECA affect the emissions. To meet the stringent NOx regulations in Tier III the ships either have to use Selective Catalytic Reduction (SCR) with high costs or alternative fuels. The latter could induce changes in emissions of other components than NOx. How is this accounted for?*

The methodology used in health effect evaluation does not make a difference between primary or secondary PM. With this in mind, there will inevitably be a large contribution from secondary aerosols which are formed both from gaseous sulphur and nitrogen compounds. We state in Section 3.2 that "...as most of the PM from shipping is Secondary Inorganic Aerosol or has been emitted with a particle size below 2.5 microns." Keeping this in mind, the uncertainty regarding the primary BC emissions from ships is unlikely to change the conclusions made in the manuscript. The formation of SIA is (Secondary Inorganic Aerosols) included in the EMEP model. We have also added further description of PM components to Section 2.1.1.

However, the reviewer is correct that there are several studies (Lack et al JGR 114 (2014) 2156; Cappa et al. ACP 14 (2014) 1337; Buffaloe et al. ACP 14 (2014) 1881) which report various black carbon measurements and their dependency on marine fuel sulphur content. They seem to indicate that BC emissions are linked to fuel sulphur content. However, the studies done in a systematic approach (same engine, same operational profiles, same lubricants) with fuel sulphur content as the only changing variable are very difficult to find. Comparing BC emissions from distillate and residual fuels, especially with results from various studies using different types of engines and measurement techniques, are like comparing apples to oranges. The study of Ushakov et al (J. Marine. Eng. and Technology 12 (2012) 30-39) is one of the first of its kind, which artificially adds sulphur (DMDS) to distillate fuel and reports its impact on emitted PM. In this setup, Ushakov et al. report that engine operational parameters (rpm, load) have a larger impact on BC emissions than fuel sulphur content. The issue of BC/fuel sulphur content dependency needs more careful investigation as Cappa et al state in their report. We hope that the reviewer also recognises the need for further experiments and agrees that once conclusive evidence between fuel sulphur content and emitted BC is available, emission model adjustments can be made. These are, however, beyond the work described in this manuscript.

Page 21955, Line 17: *not affect, rather strong statement, suggest changed to have limited effect*

Changed to limited effect

Page 21956, Line 22: *Suggest clarifying that it is the effect of changes in all emission sectors that is discussed (not only shipping).*

We now state that there are expected changes in land and sea based emissions.

Page 21956-21957, Section 3.4: *I miss figures and discussions on how ozone and SOMO35 is affected by the changes in ship emissions from 2009 to 2030. In 2009 you find that the NOx increase due to shipping results in titration of ozone in and around the major shipping lanes. What are are the results for ozone in 2030 with decreased background NOx levels and rather stable (baseline) or decreasing (NECA case) NOx emissions from shipping? Ozone response is highly non-linear dependent on back-ground NOx levels. Would different background NOx in 2030 lead to a different sign of the ozone response? As mentioned above the NOx-ozone chemistry is highly non-linear. Likewise, formation of nitrate aerosols is very dependent on background sulphur levels. This study uses only one scenario for non-shipping emissions and two for shipping (baseline and NECA). The results are discussed without mentioning alternative scenarios. Would chosing other scenarios for non-shipping sources have a large effect on the results, why/why not? I suggest some discussion on this in a separate Discussion section.*

An additional table has been included in the manuscript showing the effects of the emission changes on ozone from 2010 to 2030, and the effects of shipping for those two years. The discussion of these results are included in several places in the manuscript.

The relationship between NOx, SO2 and NH3 emissions and the formation and deposition of Nitrate and sulphate aerosols are discussed in the conclusions.

Page 21958, Line 3 : Acidification from sulphur deposition is mentioned here (in the Conclusions) for the first time. Earlier the paper only addresses the effect of sulphur on PM levels. I miss a discussion of the effect on sulphate deposition in the results section. I would have preferred to have it in. Alternatively, it should at least be mentioned in the beginning of the manuscript that this still is an important effect in certain regions but that you have chosen to focus on the effect of sulphur on PM and YOLL.

It is true that acidification first mentioned in the conclusions sections, and not in previous parts of the paper. We do however show the effects on sulphur and nitrogen depositions. We have added more text in the conclusion relating the depositions of sulphur to acidification, and the deposition of nitrogen to acidification and eutrophication.

This is the new text: Emissions from shipping also affects acidification. Emissions of acidifying components from all sources have been reduced significantly over the past decades. Even so, critical loads for acidification are exceeded in areas at risk around the Baltic Sea and the North Sea Gausset al. (2013). A large part of these areas are located relatively close to the

sea. As shown in Table 4 emission reductions of sulphur from shipping has contributed to an lowering the depositions and thus less acidification. The expected future reductions of sulphur and, provided NECAs are implemented nitrogen emissions, will contribute to further reductions in the exceedances of critical loads for acidification.

We have also included additional text related to eutrophication: Critical loads for eutrophication are exceeded throughout most of Europe, including most of the land areas around the Baltic and the North Sea. Gauss et al. (2013. As shown in Table 3 and in Figure fig:refConsd a significant fraction of the calculated nitrogen depositions are from shipping. if NECAs are implemented this fraction will remain almost unchanged as land based emissions are also expected to decrease. Without NECAs the fraction from shipping will increase.

Page 21958, Section 4: *In my view the Conclusions section should summarize the main findings of the study. This is done rather briefly and should be extended with more information. The findings could also be discussed in relation to the studied regulations with some policy relevant perspectives like did /will the regulations have theintended/optimal effect. The last and longest part of the Conclusions section seems a bit misplaced. I suggest to move it to a Discussion section. It is interesting but seems to be repetition of discussions in the studies referred to. It would therefore be better to shorten it and point the interested reader to the references. Very much attention is given to fuel prices which is only one driver for change in emissions while other drivers (trade markets, policies, technology, alternative fuels, etc.) are not discussed.*

The Conclusion section has been strengthened. As suggested by the reviewer the last part of the conclusion section has been put into a new subsection titled: **Cost and effects: Some final remarks.** The normal order would be to include such a section before the conclusions. However, as requested by reviewer 2, this section focus on technology, alternative fuels, fuel prices etc. that are not discussed elsewhere in the paper, but rather put the actual conclusions into a wider perspective.