Overall response

To begin with we would like to express our thankfulness to the referees who made the effort to review our manuscript. We have experienced that this is a text many researchers seem unwilling to evaluate, probably because three quite separate scientific fields are involved (”climate effects of aerosols”, ”health effects of aerosols” and ”health effects of climate change”). For this reason almost a year has passed from the original submission of the manuscript until we finally received any comments on it (we had to withdraw the manuscript from a different journal because they were unable to find referees).

A result of the delayed review process has unfortunately been that the manuscript became out-of-date in some parts. New publications that will be considered in a revised version are, among others, works by Eyring et al, Atm Env 2009, Skeie et al, Atm Env 2009, Winebrake et al., 2009, Lauer et al., Env Sci Tech 2009, Langley et al, Atm Chem Phys 2010 and Fuglestvedt et al. Atm Env 2009. However, we are pleased that there still seems to be no objection that our work is the first attempt to discuss the possibility of quantifying the health effects of aerosols where both exposure and climate change are considered.

It should be emphasized that we have no intention to provide a complete survey of all interactions involved in order to estimate “total” health effects of aerosols. The purpose is to examine and emphasize the need for a more holistic view of the relationship between anthropogenic aerosol emissions and human health (it is a policy paper rather than a technical one and we would like it to be judged as such). The complexity of the problem should not prevent the scientific community from attempting to address these issues that we believe are of great importance for our efforts to tackle climate change. To avoid obscuring the message with too many details we therefore shortened the paper and the number of references considerably before submission to ACP. However, after reading the comments by the referees we agree that the text needs to be more exhaustive in some parts and that several issues have to be developed.

As an illustration we used a “back-of-the-envelope” calculation of “total” health effects of ship emissions. Although the estimate is rough and incomplete it provides valuable information about research needs, uncertainties and order of magnitude of the effects. Simplified calculations are sometimes used (e.g. Andreae, Science, 2007 or Ramanathan and Feng, PNAS, 2008) and may be helpful as a first guess when the complexity of the problem is far beyond also very advanced computer models or when the reasoning is more important than the exact numbers. Hopefully the estimate in this work could be refined and elaborated by future research. Eventually, such estimates would have to include also other common aerosol types and sources of climatic relevance.

It seems that the referees have no objections to the general idea of the manuscript or its most important conclusions. We therefore hope to get a chance of providing a revised text. Below follows some comments to, what we consider to be, the major issues addressed by the referees. Detailed comments by the referees will be discussed point-by-point accompanying the revised version.

Positive criticism

First attempt to calculate the combined health effect of aerosols

Interesting topic for future regulations of emissions from anthropogenic sources

Comparing the number of deaths from direct aerosol exposure with the number of lives saved by the cooling is in principle worthwhile

Negative criticism

Too simplified approach (methodology not sound):

1)All input variables in this equation ($N_{deaths,exposure}$, C, and RF) are taken from the existing literature, picking a single study for each of them.

Reply: The main reason for the limited number of references for the variables is that very few were found that contained appropriate data (a year ago). The article by Fuglestvedt et al. (2008) was for instance the only article we could find that listed the RF from specific components in the ship exhausts (these data are found in the supplement, which probably is why the referee did not understand how we could extract them from the article). Only one comprehensive compilation was available for the number of deaths from a global temperature increase (although this number reappears in several studies and reports). There are now a quite a few new articles calculating number of deaths globally from ship emissions and these will be included in a revision. Also a more substantial discussion about the temperature effects of ship emissions is possible based on more recent reports.

2) The equation is assuming direct and offsetting tradeoffs between the health impacts from a temperature change and from aerosol exposure. Given the likely different populations that would suffer differently, and given that these estimates are statistical rather than identified as the same individuals, this seems to be incorrect methodology.

Reply: One of the reasons ship emissions were chosen for the case study was that there are few trade-offs between "the health impacts from a temperature change and from aerosol exposure". As discussed in the manuscript the people suffering from health effects from climate change are demographically very separate from those suffering exposure to ship emissions. To include a correction for co-variances, that some people would be affected by both exposure and climate change of ship emissions, seemed unnecessary. This is not only because of the demographic differences, but also because a very small fraction of the population is affected (which means it is extremely unlikely for a person to be influenced to a large extent by both aerosol exposure and climate change). However, we agree that this is not entirely clear in the manuscript.

3) The different timescales of health impacts due to these two different impacts are ignored.

Reply: Timescales of the health impacts are discussed in the manuscript, although very briefly. This was among the things we shortened in a previous manuscript version, but we think that the referee is correct – this has to be discussed in more detail.

4) Non-linearities in the system are neglected

Reply: We are discussing non-linearities briefly (pp15065-15066), but do not explain why we think linearity is a good first approximation. Most equations may be approximated with linearity when the studied interval is sufficiently small, which we think it is in the case of radiative forcing (and its consequences) due to ship emissions.

5) The geographical pattern of the temperature response due to a heterogeneous RF needs to be discussed (see Fuglestvedt et al., EST, 2009 and references therein).

Reply: We are discussing geographical patterns of temperature response, but will be able to do it in a more sophisticated way based on more recent publications.

Except the back-of-the envelope calculation no new material presented

Reply: We are not aware of any publications where "total" health effects are quantified even on a very basic level. We think this issue is highly relevant for both science and policy making.

Ignore recent studies (about shipping)

Reply: As described above the manuscript definitely needs to be updated, but no new studies contain material that fundamentally changes the conclusions of our work.

Uncertainty analysis sketchy; no uncertainty is given for the number deaths per K.

Reply: The uncertainty analysis should be more extensive, which is greatly facilitated by some very recent publications. The reason we only discuss (and not provide exact numbers for) the uncertainty in the "number of deaths per K" is that no uncertainty estimate has been made in the literature and it would be far beyond the scope of this paper to include such calculations. Instead we give several arguments that the number we used probably is an underestimate (pp 15065-15066) (which means the "total" health effects of ship emissions may very well be on the other side, i.e. saving lives).