Assessment of past, present and future health-cost externalities of air pollution in Europe and the contribution from international ship traffic using the EVA model system

Response to Referee #2

Full review, 01 July 2013: Fintan Hurley, IOM, Edinburgh fintan.hurley@iom-world.org

Introductory remarks

Reviewer: First, I apologise to the authors for the long delay between my receiving this paper and preparing this full review. As requested, I prepared a quick review of the paper in I think January 2013. I don't know if the authors have seen this. I include it as an Annex. I am unfamiliar with the Journal's interesting and public peer review process and so I didn't understand that occasional requests from the editors, that I contribute to web-based discussions, were in fact requests to complete and post a full review. I finally understood a few weeks ago that I was to write a full review; here are my detailed comments. They reflect my experience, i.e. within the overall impact pathway approach of this paper, I am not really familiar with the modelling of emissions and concentrations; my expertise is principally in methods for estimating health impacts and how they link with other components, including associated monetary values. In particular, I led the Methodology development of CAFE cost-benefit analysis though I haven't re-visited it in preparing these comments. Nor have I looked at the comments already on the website, including those of the other referee.

SUMMARY OF COMMENTS

Reviewer: This paper has some interesting and quite important results about the extent to which emissions from shipping contribute to air pollution and consequent disease and mortality in Europe. Methods and results are reported on how emissions from shipping (in the Northern Hemisphere, in SECA region) contribute to air pollution concentrations across Europe. Results are given on consequences for health, in terms both of health effects and monetary values. However, corresponding methods (for estimation of health impacts, for monetary valuation) are not given, certainly not systematically. This is a serious limitation which needs to be addressed. Comparison of results with CAFE, without comparison of methods, is no substitute for describing and discussing Health Impact Assessment (HIA) methods properly. The issue is particularly relevant because the EVA Model is applied to Europe as a whole; consequently the paper needs to address whether a common methodology will work between EU and elsewhere Europe in terms of e.g. "exposure-response" functions; background rates of mortality and morbidity; monetary valuation; and if so, what that common methodology might be. There are various other things that need to be addressed. I think that to have a coherent paper the authors have either of two principal options.
a. Maintain the original vision of the paper but strengthen it very substantially in terms of describing and evaluating methods for HIA and monetary valuation;
b. Focus on emissions from shipping and their effect on air pollution concentrations across Europe, without expressing those implications in terms of health effects and monetary values.

Answer: First of all, we would like to thank the reviewer for valuable comments and suggestions. However many of the comments are clarifications concerning the methodology used in the integrated EVA model system, which is understandable, when reviewing this paper only. Overall, this paper has been submitted to ACP as a companion paper to the paper "Contribution from the ten major emission sectors in Europe and Denmark to the Health-Cost Externalities of Air Pollution using the EVA Model System – an integrated modelling approach". The overall idea of the structure and objectives of the two papers is as follows:

1) the first paper (the other paper) contains a detailed description of the integrated EVA modelling system, together with a demonstration of how the system works estimating the contribution from the ten major emission sectors (defined by the SNAP categories) on health impacts and related external costs from changes in the atmospheric composition applying a regional chemistry transport model with full non-linear chemistry.

2) the second paper (this paper) contains a study of the full health impacts and related external costs with respect to the total air pollution levels (including natural emissions) together with an assessment of the contribution from international ship traffic, and development over time, which is not included in the first paper.

The main idea in the two papers is, on an overall level, is to estimate the total health impacts and related external costs from the total air pollution levels and the main contributors to these impacts. The main difference compared to the RAINS/GAINS system is not the use of the impact pathway approach on the total air pollution levels (even though we include the natural emissions), but the calculation of scenarios where the RAINS/GAINS system is assuming linear and predefined source-receptor relationships for every species and country, while we calculate the scenarios using the model system every time in order to take into account the non-linear effects from atmospheric chemistry, which is new in this concept.

The inclusion of a comparison of the total health impacts and external cost from the total air pollution levels is not because we consider the CAFE results as "a gold standard", but because the results from CAFE are the single results used presently to support decision making at the European level. Therefore, the results from the EVA system with respect to impacts from the total air pollution levels needs to be intercompared with the results obtained in CAFE, since the total impacts from air pollution is a benchmark in itself for addressing the challenges with respect to air pollution and to find the magnitude of the overall air pollution problem so it can be compared with other societal challenges. As we have developed a system, where the only standards refer to CAFE we needed to make this intercomparison at the overall levels. Not to test whether we obtained the same results, but to test whether the results we obtained for the total air pollution levels are in line with previous results. Only in this way, the possible differences between the results obtained by the EVA model system, applying realistic (non-linear) source-receptor relationships can be compared with previous results when applying the system for emission scenarios. The comparison with the results obtained in the CAFE project for the total air pollution levels is the only way to intercompare the different model system under similar conditions, since the basic assumption assuming linearity in the atmospheric system or not, which are important for the emission scenarios are very different.

Since many of the comments from the reviewer concerns discussions and results already conducted in the first paper, we chose the option a: to maintain the original vision of the paper but to refer to the first paper in terms of describing and evaluating the methods for HIA and monetary valuation.

DETAILED COMMENTS

Overview comments – what is new and useful here?

Reviewer: This paper has some interesting and quite important results about the extent to which emissions from shipping contribute to air pollution and consequent disease and mortality in Europe. In particular, it identifies and highlights the benefits in pollution reduction from policies to reduce sulphur emissions from shipping in the Baltic and in the North Sea (SECA Area).

This leads to a proposal, which sounds good and reasonable (though I don't know how difficult it would be to implement) to extend these low-sulphur fuel measures to shipping elsewhere in the Northern Hemisphere, on the grounds that health gains would be significant. The likely benefits to population health in Europe of such a measure could be estimated afresh using the EVA Model. The paper provides a basis for

estimating these approximately assuming that the measures internationally would give reductions in resulting population-weighted air pollution *pro rata* those that have come from the SECA measures.

The authors use non-linear atmospheric chemistry modelling of the fate of emissions from particular sources, which they say is an improvement on the linear modelling used in GAINS / RAINS / CAFE – I'm not competent to judge but I'm happy to take the authors' word for this. (Non-linearity might put in question any assumptions of *pro rata* reductions – is that why the authors didn't do this explicitly?)

Answer: We agree with the comments above from the reviewer. We are not completely sure what the reviewer means with the last sentence. Due to the non-linearity of atmospheric chemistry, we calculate every scenario explicitly.

Reviewer: The authors use the 'impact pathway' approach – I agree. As best I can tell, the paper does not develop any new methodology for this but applies the methodology, as implemented via the EVA Model, to emissions from a particular pollution source, in this case shipping. The main novelty methodologically seems to be "tagging" (p5), though other recent papers from the same team have explained this.

Answer: Yes – it is true that we have not made any further development on the impact pathway methodology in itself besides improving input data and reviewing concentration-response functions and updating the monetary valuation for European conditions, since we believe, that the methodology is appropriate. The main difference lies in the calculations of the source-receptor relationships which are calculated explicitly for every emission reduction scenarios and in the tagging method, which increases the signal to noise ratio. In the companion paper, it is the first time, tagging methods has been published in connection with the the EVA system in a peer-reviewed journal. Therefore, we think the novelty still lies in the application of the tagging method for estimating the contribution from international ship traffic.

Reviewer: The value of the paper is therefore in the correctness and importance of the application. Because of lack of detail on methods, those unfamiliar with the EVA Model need either to trust that it is 'fit for purpose' for the present applications, or do a wide amount of background reading which I haven't done.

Assuming that the results given here are usable to inform development of policy, the discussion of policy implications seems to me generally sound (though the role of shipping is, I think, somewhat overstated).

On that basis, I think this paper is potentially a useful contribution to discussions on how to reduce outdoor air pollution in Europe and in particular to the role of better controls on international shipping.

Answer: As stated above, the full documentation of the EVA system is conducted in the companion paper, and therefore we have only referenced to that paper.

Structure of the paper

Reviewer: In principle the structure is good; in practice far too many of the detailed methods are either unreported or appear as asides in the commentary on results, and that is much too late.

I appreciate that it is not easy to judge well how much detail on general methods to include in a specific paper from a longer series but, while I have focused on the practical implications of the results, the authors also have a methodological purpose (c/f extensive comparisons with CAFE; and the final sentence p16, lines 18-20). The comparisons with CAFE really need a lot more information about methods. In fact, there is too little detail on methods even for a substantive paper. Methods on HIA and on monetary valuation need to be reported much more clearly and more fully, early in the paper, where Methods are usually to be found; and their strengths and limitations discussed later. Then the reader can understand what's behind the

results that otherwise we are asked to take on trust; and the methodological comparisons with CAFE can be much more incisive.

NB – I do not take CAFE as a gold standard. I think its methodology was good, but it can be improved. However, without description of methods, we don't know the similarities and differences.

Answer: We refer to the answer above, that the system is documented in the companion paper. Here the discussions about the differences between the methodology in RANS/GAINS and the EVA system are given. We agree that it can be a little awkward that it is need to read the companion paper for deeper discussions and documentation, however, we believe that this structure is optimal for the two companion papers. It is first in the second paper that we are able, at the overall level, to compare the overall EVA results for the total air pollution levels with previous and comparable results. We also need to calculate the total impacts and related external cost from the total air pollution levels, in order to estimate the contribution from the international ship traffic. Since this sector is one of the yet unregulated emission sectors (except for the SECA areas), it is important to make a full assessment of the international ship traffic in comparison to the total air pollution levels from all sources. We have also emphasized the development over time, since the land based emissions are projected to decrease while the emissions from the international ship traffic is projected to increase, which increases the importance of this sector over time and emphasizes the need for regulating the sector not only in the present SECA areas.

Limitations in description of methods – and perhaps in the methods themselves
Reviewer: My main concern is that description and discussion of methods are largely missing. I support the authors' ambition of using the "best available and most accurate" methodology (p4, line 30) throughout the impact pathway chain, even if that is computationally demanding. I do not know how well that has been achieved but if the authors have sufficient grasp of the issues to develop a leading edge methodology, it is strange that several important aspects of this have been neither described or discussed.

Answer: Again we refer to the companion paper for the full description and discussions of the methods.

Reviewer: As I see it, estimation of health impacts involves integrating four kinds of information:

a. The population exposed / at risk – in this paper "Europe" (undefined, but we get an idea from most of the Figures, and some text in finally 14, line 13 – this needs to be defined clearly, early) and "Denmark".

Answer: The definition of Europe in the calculations is now defined in the introduction.

Reviewer: b. The relevant pollution levels experienced by that population. These vary by scenario (c/f Table 1); the non-linear (with "tagging") methodology is described in at least some detail; and results are given (c/ Figures) for the effects of several scenarios – enough for the reader to get a feel for what is or isn't going on.

c. Concentration-response functions (CRFs – more accurate than the authors' name of exposure-response functions) – these are not given, not even the pollutant-outcome combinations. Tables 2-4 show us what health outcomes are used – but which pollutant-outcome pairs are used? And which CRFs?

d. Finally, implementation needs to use background rates of mortality and morbidity, often age-specific (depending on the CRF). These are not even mentioned. (In CAFE we integrated CRFs and background rates into 'impact functions', for ease of implementation; but the component parts need to be described separately.)

Answer: All the descriptions and documentations needed by the reviewer are given in the companion paper. There is a general discussion in the community of whether to use the term “exposure-response” functions or “concentration-response” functions. We would like to keep the exposure-response expression, since the functions we use are multiplied by the population exposure (population times concentrations) and not the concentration alone.

Reviewer: Given the comparisons with CAFE, and the similarity of health outcomes, I assumed the CRFs might be the same as CAFE. But I recognise at least two differences: cardiovascular hospital admissions are sub-divided here, though not in CAFE; and there is reference to “exposure-response” functions for SO2, not included in CAFE core. So what was done, and why? If CAFE is a kind of benchmark, what’s similar, what’s different here from CAFE? What functions in SO2 are used?

Answer: We have made a thorough review of the “exposure-response” functions, which are documented in the companion paper and referenced to a larger scientific report on the public domain, see.

Bønløkke, J. H., T. Sigsgaard, J. Brandt, L. M. Frohn, E. M. Flachs, H. Brønnum-Hansen, M.-L. Siggaard-Andersen, 2011: CEEH Scientific Report No. 7a - Description of the CEEH health effect model. Centre for Energy, Environment and Health Report Series, pp. 76, 2011. ISSN 1904-7495. (www.ceeh.dk)

And the exposure response functions used in the EVA system is based on this review. Therefore there are differences between the CAFE and EVA CRF’s (or ERFs).

Reviewer: What are the possibilities of double-counting, when aggregated across pollutants or “compounds” (Table 5)? Chances are not much with just PM and O2, as in CAFE core. But with SO2 also?

Answer: There is no double counting the results in Table 5. All the impacts and external cost are assigned to only one emitted species. In the EVA system, the contributions from different chemical species from different sources are tracked through the system and careful book-keeping of the species is made.

Reviewer: And especially: CAFE was designed for applications EU-wide, not Europe-wide, with a population about twice that of Europe, the difference primarily from including countries of Eastern Europe (Ukraine, Belarus, Turkey, parts of Russia are mentioned – were the same CRFs used Europe-wide? How applicable are they?

Answer: That is a good question. We have in this first version of the system, chosen to assign the same CRFs in the whole model domain as a starting point. Of cause, the response-functions vary between the different countries since they depend on the non-air pollution risk factors in the individual populations and since the CFRs are calculated from relative risks from a mean population in the European Western countries. However, there are also large differences between the northern and southern parts of Europe as there are between west and east. To apply country based CFRs will be a part of the further development of the EVA system. However, since there already are relatively large uncertainties connected with the CRFs, we do not consider the risk of applying the CRFs in Eastern Europe as significant.

Reviewer: How were YOLL estimated from cohort studies – how well does that work for Europe as a whole? What account, if any, of cessation lag?

And YOLL from time series studies – are these from effects of ozone only? And calculated how?

Answer: The calculation of health impacts, including the YOLL are described in detail in the companion paper. The key CRFs have been confirmed to apply for Europe as well as for the US in recent review studies, as explained in the companion paper.

Reviewer: Did ozone quantification use the recommendation from WHO TFH, and implemented in CAFE, of quantifying only at daily 8-hr max concentrations higher than 35 ppb?

Answer: Yes.

Reviewer: And other questions…

Answer: That is the basic of science – never stop asking questions.

Reviewer: And with monetary valuation, there are further issues
e. What monetary values were used? Same as CAFE, or different? Same major question – how applicable to countries in Eastern Europe? The actual valuations used Europe-wide need to be considered; and the issue of whether or not to apply the same values Europe-wide surely needs discussion. (From this paper I have no idea of what was done.)

Answer: We have developed our own monetary valuation, partly based on CAFE, partly based on other sources in a review study (see the companion paper, for more details). In this study, we have used the same monetary valuation for the whole of Europe as a first step. To extend the valuation of health impacts per country, will be included in the next version of the EVA system. So the economic valuation in the present paper is based on Danish conditions, which can be extended to the European countries where the mean valuation is app. 30% smaller as a mean over the EU countries. See the following report:

Brandt, J., Silver, J. D., Gross, A. and Christensen, J. H., 2010: "Marginal damage cost per unit of air pollution emissions", Roskilde: National Environmental Research Institute. 23 p. Specific agreement 3555/B2010/EEA.54131 implementing framework contract ref. no. EEA/IEA/09/002. (The results in the report was used in "Road user charges for heavy goods vehicles (HGV) Tables with external costs of air pollution", EEA Technical report No 1/2013, ISSN 1725-2237, pp. 88.)

Reviewer: f. The variation in CAFE results (Watkiss et al.) comes largely from 4 different methods of monetary valuation – based on Value of a Statistical Life (VSL) or Value of a Life Year (VOLY), and using mean or median values. In CAFE, VSL results were higher than those using VOLYs; and results based on mean values were higher than those using means. Yet in this paper, comparisons with CAFE were made using VOLY results from EVA and VSL results from CAFE. Why? In my opinion this methodological difference makes a substantial contribution to the CAFE results quoted being higher than the EVA results (per unit population).

Answer: The reviewer is completely right – this has now been made more clear in the paper.

Reviewer: g. And finally – what, if anything, has been done about Discounting? And why?

Answer: We have not included discounting, as everything is calculated in 2006 prices. It can be discussed whether to use discounting or not, depending on when emission reduction measures are coming into action and when the benefits from the measures are realized. When using fixed prices, it is easier to discount the results to any post applications.

Reviewer: So, many questions. All are important for understanding what the results mean, especially in applying the methodology / EVA Model to Europe as a whole. And all are essential for understanding similarities / differences with CAFE. I find it remarkable to have a model "validation" vis-à-vis CAFE without explicit consideration of issues such as these – which will determine the similarities and differences in results, especially because there do not seem to be major differences between CAFE and the EVA Model in modelling current or future pollution under the All/all scenarios.

Answer: We would again like to refer to the companion paper, where many of the answers to many of the questions are given, including the above.

Focus of the paper

Reviewer: As I've said, I think the focus on shipping, and on reducing emissions from shipping, is good. Beyond that, I have difficulty with two aspects of the focus of the paper. The first of these is the focus on modelling the effects of all air pollution from all sources. I read p8 Lines 17-18 as saying this is the main objective of the paper though on re-reading it's not quite as clear as that. (It should not be ambiguous.)

Answer: The focus of the paper is three-folded: To estimate the contribution of the international ship traffic, to estimate the total health impacts and related external costs from the total air pollution (which is also needed to calculate the contribution from the international ship traffic, and thirdly, to calculate the changes over time, to investigate whether the present measures are sufficient. This has now been made more clear in the introduction.

Reviewer: I find the focus on all air pollution from all sources strange – it's been done already, e.g. CAFE, and I can't see what this paper does that is new in that regard, except extend the domain of application beyond the EU to Europe as a whole – while ignoring completely the methodological issues involved in such an extension.

Answer: When developing a new integrated model system, it is important to test and document the system, also compared to the only previous calculations that exists e.g. for the total air pollution levels – in our study also including the natural sources, since these should be taken into account in the atmospheric chemistry and are important to include to test measures of anthropogenic emissions reductions. The calculations for the total air pollution levels are not made to benchmark the results to the results in CAFE, but to make a benchmark for the present and future EVA calculations, since the total impacts and total external costs are needed in order to be able to compare the contribution from single emission sectors or individual sources to the total external costs. Only in this way, it is possible to quantify whether the contribution from a specific emission sector (in this case the international shipping) is significant or not. The impacts are important to clarify both in absolute and relative numbers. Furthermore, we have included the health impacts and related external costs for the whole of Europe, since air pollution is transported over boundaries – and also over the boundaries of Eastern and Western Europe. If health impacts were only calculated for the EU27 (now EU28) countries alone, a large part of the impacts are disregarded and both the absolute and relative contributions can change considerably. Measures for air pollution reductions and benefits cannot be carried out for single countries alone and neither for the European Union alone. A considerable fraction of the impacts from EU27 emissions are taking place in Eastern Europe. The impacts here have to be included in an overall assessment of the air pollution problem and for developing strategies for policy making. That is the main reason for including the whole of Europe and not only EU. Furthermore, it can be discussed whether a smaller unit prices for health impacts should be applied in the eastern countries for emissions taking place e.g. in Western Europe. For political reasons, it can be argued that the value of a life should be considered equal in both west and east when regulating emissions and not necessarily depend of the PPP of the single countries. It can be argued that the unit prices for the country where the emission reductions are taking place should be used and not the unit prices for the receiver

country where the impacts are taking place. For this reason, we think that an overall estimate of eternal costs at the European level can be carried out using economic valuation for applicable for richer western European countries.

Reviewer: Within the paper there does seem to be a rationale for Question 3: It gives a basis for relative effects, to put the effects of shipping in context – this is done, though it could be developed more incisively.

Results are used to give a basis for comparison with CAFE – but I think a comparison of results is superficial without at least statement of similarities and differences of methods. [These include that the paper includes emissions from natural sources, whereas CAFE doesn't (because these are not amenable to policy measures) – a difference not noted by the authors here.]

Answer: As stated before, the discussion of similarities and differences are given in the companion paper. The fact that we include natural sources is now explained in the paper.

Reviewer: Secondly, I do not understand the focus on results from Denmark specifically – why do this, unless some general conclusions are drawn, in addition to the obvious ones that if emissions are reduced in one part of Europe, i.e. the SECA Region, then there are greater benefits in nearby countries. Half of the text on Conclusions is about Denmark. Why?

Answer: The focus on a single country, in this case Denmark, is to demonstrate the EVA systems capability to calculate for single countries alone and to show that impacts from introducing the SECA area in a country where the contribution from ship traffic is considerable. The example for Denmark illustrates the possible benefits for countries of extending the SECA areas including e.g. the Mediterranean Sea or Black Sea or even parts of the Atlantic for countries near the oceans where the impacts from ship traffic is largest.

Tables

Reviewer: Table 1 is helpful in listing the scenarios examined. Tables 2-4 have a lot of information which has not been used fully. Several aspects deserve comments.
i. In each Table, usually the trend with time is similar for different health outcomes. But there are differences in the sharpness of the slope. I think that lack of proportionality reflects two things – that difference pollutants affect different outcomes, and that the quantification of various outcomes is specific to particular age-groups which differ by outcome. The authors should explain.

Answer: The difference in proportionality is mainly due to differences in the proportionality for the different pollutants causing the different impacts (see Table 1 in the companions paper), since the CRFs are linear. This has now been explained in the manuscript.

Reviewer: ii. As a specific example: Why in Table 3 are acute YOLL in 2020 higher than in 2011, when the converse is true for all other outcomes? (My guess is that it is because acute YOLL reflect an effect of daily ozone only – surely deserves some comment?)

Answer: Yes – the increase in acute YOLL in 2020 is due to an increase in ozone concentrations in the Baltic countries (Latvia, Estonia, and Lithuania) due to an increase in the NOx emissions from the international ship traffic in 2020 in the Baltic Sea according to the emission inventories, where a general increase in ship traffic is expected. The increase in NOx emission induces a decrease of ozone over the Baltic Sea but an increase over land where people are living. This has now been explained in the manuscript.

Reviewer: iii. I'm puzzled with results from Table 4. Assuming that we can add across compounds (and nothing is said that we can't), it suggests that <20% of the burden of air pollution across Europe is from

primary PM2.5. Is this really so? It may well be true but I didn't expect it to be so small. What is the ratio of primary to secondary PM2.5 in terms of population-weighted average concentrations Europe-wide? Once again, what CRFs were used for SO2?

Answer: Yes – it is correct that the primary part of $PM_{2.5}$ contributes much less to the burden of air pollution compared to the secondary part of $PM_{2.5}$. As thoroughly explained and discussed in the companion paper, we assign equal toxicity to all types of particles in this study, and with this assumption the total impacts from secondary inorganic particles is much bigger than the impacts from primary particles. However, in the companion paper, we have also included a sensitivity study showing the impact of applying different toxicity to the primary or secondary components of $PM_{2.5}$.

Figures
Reviewer: These need to be checked and corrected in at least several respects, e.g. (a) Figure 3 is not what the caption says it is; (b) The captions for Figures 1-3 refer to three scenarios when the text refers to two. And (c) Figures 4 and 6 refer to 2020 but the detailed text within the Figures refers to 2000. I assume these errors reflect carelessness – it's surprising that none of the 13 authors noticed them.

Answer: We have carefully triple checked the captions of the figures and the text in the manuscript referring to the figures, and everything should be ok as far as we can see. In the text we are just e.g. informing that e.g. the results in figure 4 (year 2020) can be compared to the results in figure2 (year 2000) for examining the development over time.

Reviewer: To summarise once more: There is something good and worthwhile here but substantial further work is needed. Scaling back to a paper showing effects of measures on concentrations should be considered.

Answer: We would again like to thank the reviewer for the comments and suggestions. Overall, we understand his concerns, not having read the companion paper. We have clarified in the introduction, that the documentation of the EVA system is included in the first paper.

Annex – quick review in Jan-Feb 2013 – now superseded by longer review
Reviewer: This paper highlights that, compared with CAFE in particular, the EVA system has an important advantage – it can and does model the effect of emissions from particular sectors. The present paper uses that attribute of EVA to model the contribution of the shipping sector in the Northern Hemisphere and in the Baltic Sea.

The idea of modelling the effect of emissions from a sector is not new but as far as I know its application to shipping in and around Europe is and the results are important from a policy point of view.

The EVA model has four components – the chemical transport model; population data; 'exposure-response relationships'; and monetary valuation. Of these four, the paper gives detail only on the 1st component. Otherwise, it gives results from the model (in terms of health effects and their monetary value) but without any transparency about the methodology – for this, we are referred to other papers.

The focus on the chemical transport model is I expect of interest to (readers of) this journal; and the 'translation' of effects on PM into effects on health, with corresponding monetary values, is I think of interest also.

I see little that is new (=previously unpublished) methodologically in the paper. The EVA model itself has been described elsewhere. There is a limited 'validation' against CAFE in terms of results for air pollution generally. However, the similarities and differences are not discussed incisively.

I see the paper as being of reasonable interest policy-wise and of limited interest methodologically.
The paper is well written – clearly structured, avoiding jargon, clearly written – though I think to some extent repetitive.

Answer: We have already addressed the items raised in the quick review above.