

Anonymous Referee #1

Note: We take the revised version of the review to embed our comments.

Based on the response by the authors, this revised evaluation is offered in advance of a revised manuscript for open review. The authors (Dr. Peters in particular) have clarified and/or corrected some of the initial review concerns by this reviewer, and this is appreciated. Below is a revision to the first review, modified to incorporate the response posted. It still points to significant revisions needed, but with consideration for what might be useful for the authors in their effort. Presumably, with this revision the authors can successfully present an ACPD revision, and they are encouraged to do so.

We thank reviewer 1 for the comments and the opportunity to respond. Reviewer 1, raised some very critical comments. However, we believe that these can be adequately addressed in a revised version as many comments relate to descriptions and misunderstandings as opposed to incorrect methods, data, or results.

Key issues raised by reviewer 1 were as follows:

- More descriptions needed
 - We added a small section giving an overview of each model and how they are used in the paper.
 - Particularly in the areas explicitly mentioned by the reviewer, but also more broadly, we expanded the discussion of the model and assumptions, without going over board when the material is available in other references.
 - We reorganized the introduction to make it a little clearer on what we cover
- More model comparisons needed
 - We included a new section in the article focussing explicitly on model comparisons
- Accessibility
 - As discussed with the editor, the results are available by contacting the corresponding author

In the full review and interactive discussion the referees and other interested members of the scientific community are asked to take into account all of the following aspects:

1) Does the paper address relevant scientific questions within the scope of ACP?

a) PRIOR ASSESSMENT: Maybe;

b) Authors' response to comment is valid. A better presentation **would better address these questions this**, but the authors are correct that the topic is relevant to ACP.

2) Does the paper present novel concepts, ideas, tools, or data?

a) PRIOR ASSESSMENT: Only so far as FRISBEE, but it is in context of previous work not well compared or discussed.

b) Updated assessment: **Perhaps**. It is not necessarily "novel" to apply economic an engineering models to produce new estimates (this has been done before), but the authors may claim this paper to be novel in using these to estimate Arctic oil and gas production and shipping given that FRISBEE has been used to do this.

i) "A global model of oil and gas production (FRISBEE, peer reviewed and well described, see Aune et al) is used to estimate the oil and gas production and in this paper we focus on the Arctic results in addition to distributing them over a 1x1 grid." The authors also respond by saying, "It is not uncommon, in our opinion, to use an economic model to generate scenarios or projections."

ii) Having a *unique* tool (FRISBEE) may qualify as “novel”, and the authors appropriately point this out. However, whether the paper presents “novel concepts” for the community is unclear without more transparency (see below).

c) The authors point out that they “are not aware of such a detailed model elsewhere in the literature.” I may agree, and perhaps should allow for this to be considered “novel” in that sense.

d) The question is this: ***Are the novel results presented transparently enough to differentiate previously published descriptions of the FRISBEE model with specific settings/inputs used?***

Here the paper revision could be used to explain

i) whether the derived estimates for shipping (for example) are determined by FRISBEE settings from prior published work (e.g., unchanged for this work, but reused);

ii) whether the FRISBEE model (or other analysis to derive emissions) are able to produce estimates more similar to other emissions estimates already published (see brief summary below);

iii) whether the authors believe the work presented here can be adopted by others (in other words, are the concepts, tools, data able to be adopted by the community, or only those with access to the FRISBEE model, etc.).

3) Are substantial conclusions reached?

a) PRIOR ASSESSMENT: Maybe, but not ones that acknowledge the context of this work.

b) This is a comment on transparency, whether the conclusions are reviewable in a way to be reproduced or approximated by review, whether the conclusions explain themselves in light of conflicting results previously accepted in peer-review publication. Assessing importance of the conclusions depends upon this transparent discussion – possible if the authors revise the ACPD manuscript to be more clear.

4) Are the scientific methods and assumptions valid and clearly outlined?

a) PRIOR ASSESSMENT: Yes, except the model FRISBEE is a black box for this journal.

b) The authors make a very important correction to the original review comment. FRISBEE is described in this manuscript. My concern is that the description of the economic model is at the edge (at best) or outside the general expertise of ACP and ACPD readers. The authors have previously published the model structure and presented estimates using the model in peer reviewed work; therefore, by reference the methods are “validated” to the quality of that prior review, presumably by economists.

c) What is missing is the necessary discussion of how the model was (or maybe should be) exercised to provide results that would “capture” or reproduce ranges of emissions estimated in previous work. The scientific nature of ACPD is to contribute to better understanding of how methods and data and concepts may disagree at the time, and how the assumptions may converge as leading studies are synthesized. This work cannot yet be used in this way, given the FRISBEE inputs called out are not discussed in terms of alternate inputs that could have resulted in closer agreement with existing published emissions work.

5) Are the results sufficient to support the interpretations and conclusions?

a) PRIOR ASSESSMENT: No.

b) WRT to the comments in (4) and below, this can be improved by a focused revision, including sensitivity analyses that improve the transparency of the integrated economic-energy-emissions modeling and analysis.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

a) PRIOR ASSESSMENT: No. Data not made available, and no inter-comparison to allow adjustments to publicly available data.

b) The response says, “We have no issue posting our data as supporting information.” This would be expected in a revision to ACPD, and with the authors’ work to make clearly connections with other inventories, it could be a significant improvement. It also is the minimum data needed to begin to reproduce (or trace) the results described. Without adopting the FRISBEE model (or other partial equilibrium model, similarly calibrated for O&G), traceability may suffice.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

a) PRIOR ASSESSMENT: No. It could seem that the authors intentionally leave out discussion with work published in ACP in 2010, misrepresenting it in supplemental as still in ACPD, and with no clear inter-comparison of results, similar strengths, common weaknesses, or particular differences.

b) The authors respond: "We have performed comparisons, but these could be more detailed. We also perform comparisons of the oil and gas production. We are currently involved in a project comparing Arctic inventories and atmospheric/climate responses. In a revised version, we can include more detailed comparisons between studies."

c) ***This statement captures the intent of the comment very well***, and will satisfy a range of the critiques in the prior review. If the comparisons are dependent upon the chosen settings for FRISBEE, then these settings need to be adjusted to capture (if possible) the existing literature. Alternately, if input-output from FRISBEE is linear for key inputs, then discussion of alternate settings that would capture previous work needs to be considered and discussed.

8) Does the title clearly reflect the contents of the paper?

a) PRIOR ASSESSMENT: Perhaps, but it appears to be a Norwegian-focused subset of arctic shipping. The transit discussion in abstract and paper seems beyond the title.

b) The authors response seems to address this.

9) Does the abstract provide a concise and complete summary?

a) The travel time information is incomplete and seems unclear.

10) Is the overall presentation well structured and clear?

a) Not enough, without good maps and access to the data product for this work.

11) Is the language fluent and precise?

a) Satisfactory.

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

a) Yes.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

a) Yes, see comments

14) Are the number and quality of references appropriate?

a) Maybe.

15) Is the amount and quality of supplementary material appropriate?

a) No, see comments

These comments are either addressed in our summary remarks on major changes, or more explicitly in the following.

Peer-Review Comments (ACPD)

General comment: The authors response address a number of concerns in the initial review that could be revised for presentation in ACPD to address major concerns in analysis, organization, and clarity of presentation. These generally address the initial review request to include a) transparent presentation of inputs; b) sensitivity analysis of alternate results under reasonable alternate assumptions; and c) self-critical inter-comparison with existing work already meeting a) and b), above. The authors are encouraged to provide this standard of presentation in the revised manuscript. Importantly, the willingness of the authors to make the data accessible as part of the manuscript is consistent with the open source nature of this journal and the standard practice within the community; given the attention on Arctic environment, this is especially important.

Detailed comments:

1) One main area of concern in reviewing this manuscript is the FRISBEE model. Readers of ACPD should note that the application of the partial equilibrium model of international energy markets is

a) an economic model, not an atmospheric or chemistry model; and b) that this model was not intended to directly estimate shipping activity; and c) inputs and assumptions for the FRISBEE model are not transparent in this work and variable.

a) The authors correctly describe the various ways in which the FRISBEE model has been presented previously, and the pedigree of the model is indeed established in economic domains. The authors respond that the FRISBEE model “is well described in the paper and supporting information.”

b) This is necessary but not sufficient for ACPD readers to understand how results from the economic model are driving the emissions results. What is important here is whether the scientific understanding of Arctic emissions that currently exists falls within a range defined by alternate assumptions available to FRISBEE, or whether this is “not possible”.

c) **Simply: what explains the difference in results presented by this work, and are these different results reconcilable with alternate assumptions?** The FRISBEE estimates of O&G production, the derivation of shipping activity from FRISBEE results, or other emissions-related assumptions?

d) My prior review followed the shipping activity discussion enough and the emissions estimation analysis enough to suspect that the **divergent results here may be explained in terms of the underlying economic activity estimated in FRISBEE**. If this is true (and I don’t run FRISBEE, so I need the authors to do so), then economic drivers may be dominating the analysis (thus the Schumacher quote previously included).

e) Insight into the effects of alternate FRISBEE inputs would be very useful to understand in terms of comparing or updating independent literature on these topics. Certainly, the Arctic Marine Shipping Assessment attests to the importance of understanding the economic drivers for growth in Arctic oil, gas, and shipping activity. The authors need to evaluate this with clarity for readers to understand.

a) b) e) The FRISBEE model and its references have now been pointed out more clearly. There is also a new internally reviewed working paper, which is more common in economics, describing the Arctic results in detail. We give an overview of the model in the paper that is sufficient, we believe, for the reader to follow. The reader is free to access the already published material on this model.

c) We explain the difference between the FRISBEE model and other work in a new section on comparisons

d) The transit shipping activity is independent of the FRISBEE model. This is now started more clearly

2) This is critical to a paper like this because the resulting estimates of emissions appear to be far and away different than prior work; if the FRISBEE model is the source of this difference then this needs to be transparent for critical (skeptical) review of that and preceding estimates. The authors do not engage the effort to perform comparison with other studies in sufficient detail, and they do not present any sensitivity analyses under alternate reasonable FRIBEE model settings.

a) Questions that arise from the FRISBEE model, which includes exogenous price signals and other features that drive the model predictions, include generally: What inputs would be required to match existing shipping inventory estimates? Are these alternate inputs within a reasonable choice set for FRISBEE? Why or why not use alternate assumptions that conform to other expert opinion? Is the supplemental material estimating transpolar shipping coupled with or independent from the FRISBEE model and oil and gas inventory results?

b) These questions must not be unaddressed in this manuscript, even as a discussion paper that may not be ACP quality. Efforts like these are as useful as they are transparent. These need to be included in an ACPD revision.

Our comparisons do not lead to the conclusion that we are “far and away different than prior work”, as the reviewer acknowledges below. We include more detailed comparisons in a separate section in the revised version.

a) b) It is now made clear that transpolar shipping is independent of FRISBEE. We show the sensitivity of the FRISBEE model to different oil prices and the references discuss other model sensitivities.

3) Another concern is the extrapolation of energy model data to shipping activity data. In this regard, the manuscript appears only passively aware of the prior work on Arctic shipping, in ACP. While reference is made to acknowledge this work, insufficient effort is made by the authors to perform inter-comparisons of baseline emissions estimates for a common year, nor is there effort to evaluate how the scenario offered compares with the bounding range of scenarios in the prior work. Without any information from the authors, one is left to speculate whether this may be due to very different FRISBEE-derived growth assumptions, whether this is due to the authors' careful but vaguely presented inclusion of platform activity (not included in other Arctic shipping estimates), or some other reason(s). These are important gaps in the value of this paper's contribution, especially given that it will follow closely on Arctic work already published in ACP. The paper cannot be considered for more than discussion presentation without substantial work to make this work usable in comparison with other independent efforts.

a) The response to the first review includes some very important new information. For example, "Paxian et al. (2010) give a range of 0.73 – 1.28 Mt for fuel consumption in the North-East Passage in 2050, which is less than our estimate of 1.78 Mt, though system boundaries differ which makes comparison difficult. Corbett et al. (2010) has higher estimates, by a factor 1.3-2, but again care is also needed to ensure consistent system boundaries. The reason for the difference seems to be that Corbett et al. (2010) assume that as much as 2% and 5% of global seaborne trade will be shifted to the Arctic in 2030 and 2050, respectively."

b) This begins to provide the transparent and comparative analyses required by the first review.

ii) It is encouraging, for example, that Paxian et al and Corbett et al – taken together – seem to bound the shipping activity described. More of this comparison in the revision will improve the manuscript significantly. Along with questions posed in Comment 2, above, the authors may want to consider that:

(1) Corbett et al provide estimates for in-Arctic only emissions, along with comparison with Paxian. Excluding as assumption of trans-Arctic shipping (presumably NOT strictly related to O&G development modelled by FRISBEE), the comparisons of Corbett et al and Paxian may both be lower than estimated in this work.

(2) Does this difference make sense to the authors, perhaps attributed to alternate shipping activity or emissions rates compared with independent work, or do the authors identify other reasons for the higher estimates?

(3) Does FRISBEE (or the shipping model) insert assumptions about the global seaborne trade shift, or rely mostly on O&G demand to generate ship trips?

(4) These answers may be in the original article, but the understand depends upon the additional comparisons begun to be offered in the response posted.

b) The authors respond: "We do acknowledge, however, that we can discuss the differences between the different modelling approaches and corresponding results in more detail." Agreed.

The transpolar shipping is independent of the FRISBEE model.

a) The revised version has more detailed comparisons of shipping model with other studies.

3) The oil and gas shipping is dependent on the FRISBEE model, but this component of shipping is not transpolar.

These types of issues are addressed in the revised version in a more detailed section comparing and interpreting the results.

4) Lastly, the gridded inventory is described but not provided. From description, its resolution is coarser than other available inventories, either global or specific to the Arctic; however, the figures (Figure 4a through e) reflect images with legends and icons, rather than geospatial presentations of the data expected from description. Whether it follows the same route information as the AMSA

study or offers a variation or independent version of spatial allocation is unclear – and needed before readers can utilize the paper in the work intended for such inventories. In fact, I don't find that the geospatial domain is clearly described, only saying "we use a broader definition of the Arctic region to fully capture oil and gas activities that potentially requiring shipping in the Arctic Ocean."

Readers of an open source journal, indeed most journals, would expect that the data presented was accessible. That data is not provided and/or sufficiently described bothers me as reader and reviewer.

a) The authors state in response: "We have no issue posting our data as supporting information."

This is much appreciated, and would mean at least two Arctic shipping inventories would be accessible by ACPD and other scientists. It would also expand the impact of this paper.

b) The response points out that "care is also needed to ensure consistent system boundaries". This is true. The Arctic inventory data for Corbett et al is linked with their article, and although higher resolution than this work, the "system boundaries" can be evaluated by this group to allow for consistent comparison. The Paxian work could also be compared this way, unless that data has not been made public.

The transpolar routes used are described in the paper.

The Arctic boundary is based on another study, not something we created. As stated in the article the AMAP boundary is a geo-political boundary. We use an extended AMAP boundary to be consistent with economic boundaries and relevant statistics. We did not develop the revised version of the AMAP boundary for his paper, but it was used for an assessment of the economic activities in the Arctic (as we reference). We reference and briefly mention the various common definitions of the Arctic in the paper.

a) After consultation with the editor, the data is available via the corresponding author.

b) This is discussed in the comparison section.

In summary, the authors response articulates good ideas for redesigning its presentation of information, analysis, and results comparisons. With this, the contribution should complement the existing work presented in ACP last year.