

Response to review comments

We first give a general response, before responding to the reviewers' comments.

General response

We thank the reviewer for helpful comments, which we feel have strengthened the article.

Based on the reviewers' comments, we made a number of small changes to increase clarity, corrected spelling mistakes, and expanded on examples in the results. We have also references other relevant work. We have clearly mentioned the importance of correlations in this analysis, both in the abstract, methods and conclusion. We have written two large paragraphs discussing the issue in the discussion section. We are happy with the changes and feel they have improved the article.

Response to reviewer

Reviewer's comments are in *italic*, while our responses are in standard font.

Referee #1

General comments

The authors use a GTAP-based I/O model to simulate pollutant emissions from a consumption perspective. They estimate uncertainties in economic data and trace these uncertainties through the I/O model to emissions estimates by perturbing the GTAP tables used to calibrate the I/O model. The paper is well written and clear for the most part, and is a nearly-comprehensive look at the topic (with one major caveat described below).

Given the audience of ESD, I would suggest that some less abstract examples be included regarding the data and parameters that are being perturbed. For instance, the (rather complex) procedure for estimating variance in each economic flow value is described in depth in 2.3, but then examples of what this means for particular values are not given until 3.1 (for the examples of trade flows from China and etc.). I would suggest moving these examples to 2.3 and expanding on them a bit so the reader understands why this flow ends up with such a low uncertainty. This will have the added benefit of streamlining the results section to focus on the results in aggregate rather than the estimates of particular flow values, which is really more methods than results.

We thank the reviewer for pointing this out. While we feel it is inappropriate to move one of the key results from the results section and in to the methods section, we see the need for better explanations. In the methods section, we have added text to indicate what other studies have found using the same general relationship: "Implementing this general methodology has given individual regions relatively small uncertainties in other studies (Lenzen et al., 2010; Wiedmann et al., 2008). Structural uncertainties have also recently been found to be relatively small for major economies (Moran and Wood, 2014)."

Furthermore, we have expanded on the examples mentioned by the reviewer in section 3.1 to help explain why the uncertainties end up relatively low: "These fluxes are mainly dominated by the largest sectors, which have been given the smallest uncertainties in the respective regions. The

export from China to USA is mainly coming from the manufacturing sectors, which combined is one of the largest Chinese sectors, hence with lower uncertainties. Annex B countries are given lower uncertainties than non-annex B countries, which explains the low uncertainty from USA to Western Europe.”.

Important to connect this work to emissions uncertainty work using dynamic GTAP-based models. I'm not as familiar with what is available on the pure I/O accounting type models, so I assume the existing discussion of previous work is sufficient, but there is certainly some work using dynamic GE models that is relevant. Richard Plevin's dissertation work for instance is highly relevant and a good complete take on the subject from a CGE/life-cycle perspective (Plevin 2010). My group has also done work on this topic in the CGE context, which might be interesting (see for instance Elliott et al 2011 and Elliott et al 2010).

We have mentioned the work done on dynamic CGE models in the introduction, and referenced Elliott et al 2012: “Other economic models, such as computable general equilibrium models, have also received attention recently with regards to uncertainties (Elliott et al., 2012).”

One step that is ignored in the causal chain defined here (consumption->production->emissions->climate) but also introduces uncertainty is the mapping from emissions to atmospheric concentration. This is especially serious for the carbon cycle which has complex positive and negative feedbacks with both the ocean and land operating a different important time-scales (Glotter et al 2014).

The carbon cycle is included in the metric equations, and reflected by CO₂'s IRF. The uncertainty on the parameters is based on the model comparison (Joos et al., 2013). We perturb the parameters that go into explaining the feedbacks of the relatively quick response of the land surfaces, shallow oceans and atmosphere, and the relative slow response of the deep oceans. Additionally, the positive and negative feedback effects in the climate system are included in the climate sensitivity parameter, which is also given an estimated uncertainty. The uncertainties in the link from emissions to atmospheric concentrations is thus included, and mentioned in section 2.5 Emission metrics. To clarify this in the paper, we added text in the results section 2.5 (inserted text in *italic*): “Metric (temperature) values have an uncertainty range for the different pollutants and different time horizons, due to the perturbed metric parameters (RF, lifetime, and climate sensitivity). *This includes uncertainties from mapping emissions to atmospheric concentrations through the global carbon cycle, which is represented by the relatively uncertain climate sensitivity.*”

Probably the biggest concern I have is correlation of data error. Certainly its understandable (as stated on page 1021 line 23-25) that little information exists about correlations, but in order to make the rather strong comparative assertions made in the paper (for example in the abstract pg1014 line 13-18) it is absolutely crucial to address some of these possible correlations, at least qualitatively and hopefully quantitatively. If you assume fully uncorrelated errors in data or parameters, then of course it's not surprising that uncertainty in high-resolution data/parameters such as sectors within countries would have a smaller effect on global variables like cumulative emissions or temperature than large-scale global parameters like climate sensitivity. It certainly may be that the estimated emissions factor for cement production (for example) has highly correlated error (if, for instance, sector emissions tend to be under-reported everywhere). A scenario using this sort of perturbation on the underlying data would likely find a much stronger impact on emissions and climate than one

assuming that the emissions factor for cement industry in each country had completely uncorrelated errors. At the very least this puts a strong caveat on the key assertion of the paper (that economic uncertainty is of less importance to global metrics) which seems fundamental to the point of the paper and really must be addressed.

Given that the paper is otherwise nicely comprehensive in most aspects, it's disappointing (and I think a real missed opportunity) to not include something on the topic of correlated error. At the least I would hope that some synthetic examples of correlated error could be evaluated in order to test whether the strong assertions still hold. For instance, a scenario assuming correlated errors in all sectors within a country but uncorrelated between countries and another considering correlated errors in a given sector between countries but no correlation between sectors. The actual amount of correlation in these scenarios is probably not even all that important, although one could estimate values of correlation that lead to certain critical thresholds based on the relative uncertainty of the factors described (what level of correlation would be required for the uncertainty from econ data in global temp to rival the uncertainty from climate sensitivity). I suspect you would find that this critical correlation is not actually very large at all.

We agree with the reviewer on the importance of correlations, and it was amiss of us not to emphasize this in the article. We certainly acknowledge the need for addressing correlations in uncertainties of the datasets. In cases of high correlation, this may change the aggregated results substantially. We (as authors) have had many discussions on the issue of different types of correlations, particularly in the economic data, and how to feasibly and realistically include this. Through the review, we have revisited this issue by trying to implement correlations on a smaller scale. After trying to implement this, we have come to realize that this is a larger problem than we initially thought for a variety of reasons, which we discuss further down. However, we have now put more emphasis on the "caveat" by noting it clearly in the abstract, methods, discussion and conclusion section. In the discussion, we have added an extended section which discusses the issue of the correlation in economic data. Overall though, to do a thorough analysis of correlations in economic data is a significant undertaking which we see as beyond the scope of this paper. It is nevertheless an issue we continue to work on and would like to get a better understanding of.

The example mentioned by the reviewer about emission factors for cement production may be correlated across countries, is very plausible, as a similar chemical process is occurring everywhere where production is happening. However, the datasets we use consists of emissions, where the emission factors have been multiplied with energy use (and the emissions that follows from energy use). The emission factor used may be correlated across countries because calculations may have used a global or regional number for this, but the energy use is more likely to be uncorrelated across countries as production technology and production recipes varies across countries. Thus it is difficult to estimate the extent of correlation in the emissions data we use.

The review comment focused on correlation in errors, while we are of the opinion that correlation in data points is more relevant (particularly given balancing issues). As an example, we might have decent constraints on the total oil consumed in the country, and if the oil consumption in one sector is overestimated, then this implies an underestimate in another (anti-correlation). We included this correlation in the emissions data (using the quadratic programming routine), but because of the

extreme computational need we decided not to include in the IO data. Balancing the data will also introduce correlations, but to balance each MC run would be computationally expensive.

On the computation aspects, even if we had a correlation structure to apply, we at best could only do this in small scale problems. Given a dataset has N points, the correlation matrix has N^2 data points. Our economic system has about 7500x7500 data points, which means the correlation matrix is prohibitively large for most computers. Given we have no real data on correlations, populating this matrix is guess work. But, given the size, even if we could populate it, the computational issue prohibits using it.

Generating log-normal numbers with correlation also poses a difficulty. Log-normal numbers are important in our context as they are never negative, which is important in terms of both economic and emissions data where many numbers are small in magnitude but with high uncertainty. If correlations were linear on a log scale, they would not be showing linear relationships between the parameters in a non-log domain, and that would make it difficult to implement what we want and to interpret the results.

As a final note, we stress that we agree that correlations are important and that they may take many forms in our datasets. The inclusion of them may also change our results and conclusions, mainly with respect to the economic errors in comparison to other errors. It is important to note that we have discussed this issue in detail, but it was a mistake not to emphasize this issue in the original submissions. However, with large difficulties in implementing them, and no actual data to base them on, we feel we have no other choice than to just flag the issue in the text and let the reader make up his own opinion about the value of the results. As mentioned, this is an issue we continue to work on.

To clarify this, we added text in the abstract: “Based on our assumptions, which does not account for correlations, the economic data appear to have a relatively small impact on uncertainty at the global and national level...”

We also mentioned this in the methods section: “Implementing correlations in such an analysis is a major difficulty, but may also have significant effects on the results. See the discussion in section 4.”

Furthermore, we discussed this in the discussion section: “A major difficulty not included in the economic and emissions data is the issue of correlations. There is a large need for addressing correlations in datasets and uncertainties, as this may have significant impacts and the results of such an analysis. We have explored correlations for metric uncertainties (temperature and CO₂ IRF), and in one sense introduced correlations in that we make all non-Annex B countries have double uncertainties of Annex B countries. However, correlations may further be an issue in several places in the datasets and methods which we have not included, and we see at least three places where they may be important: (1) in the way the MC analysis is build up where uncertainties given to certain region/sector combinations may be correlated in each run in order to simulate corresponding behavior in the model (if Norway’s emissions from cement production in one run is low, then Sweden’s emissions from the same sector may also be low in order to build a plausible world in each MC iteration), (2) overall (median) uncertainties in certain region/sector combinations could be similar (spatial correlations; if Norway’s emissions from agriculture is low, then Sweden’s emissions from the same sector could also be low, due to similar technology, statistical offices using similar

methods, etc.), and (3) between datasets (a perturbation in e.g. fossil fuel use in the economic dataset should be reflected by a similar correlating perturbation in the emissions dataset).

Implementing these correlations, which can be argued to be important for some sector/region combinations, may clearly change the uncertainty outcome. However, we have not included correlations in the economic and emissions data due to computational and conceptual issues: there is little or no data indicating correlations in uncertainties in sectoral economic data or emissions data although correlations might be plausible (thus populating a correlation matrix is guess work), the generation of log-normal numbers with correlations is not as straight forward as with a normal distribution as they would not be showing linear relationships between the parameters in a non-log domain, and due to the large datasets used in this analysis, the correlation matrix would be prohibitively large, posing serious computational issues. Thus, this is an issue for future work to investigate.”

We concluded in the conclusion section: “We did not account for correlations in the economic or emissions data, which may play an important role and have a significant impact on the results.”

Smaller questions

1018/12-13: Does the I/O model here use the full GTAP region/sector resolution without any aggregation?

Yes. This has been mentioned explicitly in section 2.1: “We use these data to construct an MRIO model without any aggregation, which connects all regions at the sector level...”. We furthermore state in section 3.4: “To facilitate our discussion we aggregate the 58 economic sectors (post analysis) to 9 sectors.”

1023: The explanation for how to construct relative uncertainty in each GTAP value needs clarification. My impression is that the MRIO model is a traditional accounting type I/O model and thus the only “parameters” in the model are relative consumption/production/trade shares. This means there is a very simply mapping from the GTAP values to the parameters, but this isn’t made clear. I understand that the model construction is explained elsewhere, but at least this one piece of information is fundamental enough that it should be described here.

We thank the reviewer for pointing out this possible confusion. All GTAP values are given uncertainties and distributions, not only the trade shares. This has been mentioned in section 2.3: “In other words, we estimate the uncertainty of the MRIO data based on the uncertainty in the data used to construct it, which consists of all data points in the GTAP database used to construct the MRIO model.”

1024/18: This should be restated for clarification: “To retain balance, we therefore choose not to rebalance. . .”

We thank the reviewer for point out this typo. The sentence has been modified to: “We therefore choose not to rebalance, which effectively causes the “unbalanced” component to be shifted to the value added.”

1025/18: typo? “. . .emissions with roughly 12%...” should this be “. . .by roughly. . .”?

This has been changed to: “...by roughly...”.

1027/25: *You might take a look at Pierrehumbert (2014) for a useful critique of GTP. I’m convinced that you are using it appropriately in this case (though I’m no expert) but it may be useful for context in the discussion.*

We thank the reviewer for this reference. The reference has been cited in the methods section (inserted text in *italic*): “Although it has been shown that the GTP may have larger relative uncertainties than the alternative metric global warming potential (GWP) (Aamaas et al., 2013; Reisinger et al., 2010) *and it has been critiqued for some of its characteristics (Pierrehumbert, 2014)*, the GTP directly links to global temperature change and is thus arguably more policy relevant (Shine et al., 2005). In addition, the physical interpretation of the GWP is less clear and the metric has been criticized by many authors (Peters et al., 2011a; Shine, 2009; *Pierrehumbert, 2014*).”

1032/10: *these uncertainties seem very small to me intuitively, and I suspect the casual reader will agree. So this should be justified in much greater detail I think. I suppose the explanation is again due to cancellation of uncorrelated errors, and I again wonder how different the answer would be if you introduced even a small amount of correlation.*

This section has been moved to the Methods section according to a previous comment, and the discussion on the examples has been extended. In the results section, we added: “See the examples and the discussion on why the regional uncertainties are so low in the Methods section.” On the issue of correlations, see previous discussion.

1032/21-22: *a vast number? That’s pretty subjective. Can you say how many operations are required?*

Building a procedure to count all operations in the model is a large undertaking, but we have estimated that just the matrix inversion (which is done 10000 times in the MC model) requires more than 10^{12} operations. This has now been mentioned in the text (inserted text in *italic*): “Since we start from the raw GTAP data to construct the MRIO table, and normalize and invert the MRIO table, a vast number of summations and multiplications are done with the initial perturbed data (*only inversion requires more than 10^{12} operations*).”

1033/9-12: *I’m not convinced by the argument for discounting the GTAP uncertainty accounting in favor of the highly complex and somewhat ad hoc approach described in section 2. Surely if different methods for estimating uncertainty can give vastly different results then this must be accounted for, given that this is precisely the point of “uncertainty”. Its fine to choose one for the paper but you must at least test your conclusions against the alternatives, which it doesn’t seem that you do (indeed, given the very large uncertainty implied by the GTAP estimates, I’m assuming your conclusions would no longer be valid in this context). Explaining away the GTAP estimates because the model structure can’t handle it or because you don’t have the computational power to rebalance the matrices is not a convincing argument for saying that the uncertainty is not that large. . .*

According to GTAP (McDougall, 2006), the Table 19.6 in the GTAP documentation consists of “large sectors in large regions with large relative changes”. This is to show and explain some of the largest changes: “Of the more than eleven thousand (region, category) pairs, we select those that make the

largest contribution to the entropy distance measure". This is thus not a good representation of the overall uncertainties in the dataset, as it only deals with a small number of outliers. Further, these values represent those data points that move most in the GTAP balancing procedure, which may not reflect uncertainty. We use these numbers explicitly as a sensitivity analysis to see what happens if we assume that all data points are having the same data size/uncertainty relationship, but we cannot assume that this is representative for the whole dataset in the main analysis. Our approach may underestimate uncertainties, but we feel this is based on more sound assumptions than making Table 19.6 representative for the whole dataset. It is important to note that we do not exclude the GTAP data. We use the statistical relationship, but apply the parameters from the UK economic data.

1033/21: I'm worried about the many uses of the phrase "we find small uncertainties". It seems to me that you are assuming small uncertainties by the structure of your methodology, rather than "finding" them.

The structure of the methodology for estimating uncertainty in the economic data and emissions data is very similar, and thus they were expected to behave similarly. We found this not to be the case, and thus use the phrase "we find small uncertainties" several times to underline the sometimes counterintuitive results.

We should emphasize that others have found similarly small uncertainties (Moran and Wood, 2014). It is correct that this may be a consequence of "garbage in, garbage out", but we have attempted to make our error estimates follow any available literature. We additionally include a sensitivity analysis with larger uncertainties. To us, the issue of correlations is the main place that could introduce larger errors given the input we use.

1035/15-18: how have you handled the natural gas sector? My experience is that GTAP has 2 sectors for gas, gas extraction and gas delivery, and these must actually be combined to get a consistent treatment of the gas sector. I recall this is quite tricky for tracking natural gas carbon.

It is correct that GTAP has two natural gas sectors. Bear in mind that we use EDGAR emissions data, not emissions data from GTAP. Fugitive emissions from gas extraction is allocated to the gas extraction sector (which appear in the mining sector in our aggregated results), while a part of the emissions from public electricity and heat production are allocated to the gas distribution sector (which is part of the aggregated service sector). The gas distribution sector is the major consumer of the gas extraction sector, although a relatively small amount of emissions are allocated to this trade link as most emissions from gas (in a production view) are allocated to the gas distribution sector, which consuming regions and sectors purchase from.

1035/15-18: how are you handling refined petroleum products? Refineries produce a huge diversity of products with different emissions profiles. Tracking this downstream to the consumers of refined petroleum is not easy. I think Elliott et al. 2010 (in the "carbon accounting" section) describes some of this using a simple example.

The global supply-chain (using MRIO analysis) explains the link between extractions of petroleum (sectors including coal, oil, gas), production of petroleum based products and demand by consumers (sector refined petroleum). Thus, oil/gas extracted is sent to the refined petroleum sector. A consistent treatment is used in the emissions datasets (emissions from extraction in the coal, oil, gas

sectors, and emissions from refining in the refinery sector). Refined petroleum products is a single sector in the GTAP database, and we do not disaggregate this sector, which means that this sector is directly linked from production to consumption.

1040/6: should be "individual MC ensembles" I believe, not "runs".

This has been changed to "individual MC ensembles".

1044/24: typo ". . .but is this. . ."

This has been changed to "...this is...".

1045/7-10: I'm not sure why you would expect consumption uncertainty to be higher if you don't account for factors such as the uncertain distribution of carbon in multiproduct outputs from different sectors, with the most important example being refined petroleum and coal products. Coke sold to steel manufactures has very different emissions than does gasoline sold to consumers or jet fuel sold to airline services. If you acknowledge that there is additional uncertainty introduced at every step of the carbon accounting flow from production to consumption, then I suspect you would find the consumption perspective much more uncertain.

Intuitively, we expected the addition of uncertain data in the analysis (economic data) to significantly contribute to uncertainty in the end results. However, due to cancellations of errors, we found that there were only small errors from the economic dataset on an aggregated level. We do not track individual products, but agree that the uncertainties would be higher if we did (due to uncertainties in disaggregation).

1046/26: typo "emissions uncertainties often dominate over emission uncertainties".

This has been changed to: "emissions uncertainties often dominate over metric uncertainties".

1047/5-6: I have a hard time with this. It seems like GTAP has tried, however imperfectly, to estimate uncertainty in their data. However the authors have chosen to ignore these estimates seemingly because they are "too uncertain". Instead they have specified a much small "uncertainty" which is really not uncertainty because obviously the true distribution of possible values is much larger if even the very group that synthesizes and releases this data is not comfortable putting anything smaller than huge error bars on it. The authors could described the ensembles they create as using "perturbations" specified using xyz methods and assumptions, but describing them as "uncertainty" is not right.

The GTAP community has never published uncertainty data with their datasets, as far as we are aware. The table we refer to is only comparing numbers that is used as input and by how much they are changed after harmonization and balancing procedures are done (see answer to comment 1033/9-12 above). We choose to use this relationship as a function explaining the relationship between sector sizes and uncertainties. The table itself consists only of large sectors in large countries, having large changes. Thus it is only valid for these data points, and cannot be directly used to explain uncertainties on all sectors in all regions. We used this data as a test to see how this would affect the results (simple sensitivity analysis), but did no further analysis on this as we have no information on how this is representative for other data points. The word "uncertainty" referred to

here, points not only to economic uncertainties, but also uncertainties in metric parameters and emissions data, and thus the studies we refer to.

In the results section we refer to the GTAP table as “uncertainties”, which was unfortunate, and we have now changed the wording: “The “unfitted” and “fitted” data from Table 19.6 in the GTAP documentation (Fig. 2), however, act as a simple sensitivity analysis to our applied uncertainties.”

Conclusion

Overall I think this work has the potential to be a comprehensive take on carbon accounting and uncertainty, but it falls short in essential ways that must be addressed. The paper is detailed, comprehensive and well written, but it makes strong (and probably inaccurate or at least incomplete) conclusions that depend fundamentally on the assumptions made in setting up the problem (small uncorrelated errors in individual economic flow values). It does consider some limited alternative scenarios, but then discounts them without considering how they affect the conclusions.

References

- Plevin, Richard Jay. "Life cycle regulation of transportation fuels: Uncertainty and its policy implications." (2010). <http://plevin.berkeley.edu/docs/Plevin-Dissertation-2010.pdf>
- Elliott, J., M. Franklin, I. Foster, M. Loudermilk and T. Munson (2011). Propagation of Data Error and Parametric Sensitivity in Computable General Equilibrium Models, *Computational Economics*: 1-23. DOI: 10.1007/s10614-010-9248-5 <http://link.springer.com/article/10.1007/s10614-010-9248-5>
- Elliott, J., I. Foster, K. Judd, E. Moyer, and T. Munson (2010). CIM-EARTH: Framework and Case Study, *The B.E. Journal of Economic Analysis & Policy*: Vol. 10 : Iss. 2, Article 11. <https://ideas.repec.org/a/bpj/bejeap/v10y2010i2n11.html>
- M. Glotter, R. Pierrehumbert, J. Elliott, and E. Moyer. A simple carbon cycle representation for economic and policy analyses. *Climatic Change* 1-17, 2014.
- R.T. Pierrehumbert. Short-Lived Climate Pollution. *Annual Review of Earth and Planetary Sciences*, Vol. 42: 341-379 (Volume publication date May 2014).

References

- Joos, F., Roth, R., Fuglestedt, J., Peters, G., Enting, I., Bloh, W. v., Brovkin, V., Burke, E., Eby, M., and Edwards, N.: Carbon dioxide and climate impulse response functions for the computation of greenhouse gas metrics: a multi-model analysis, *Atmospheric Chemistry and Physics*, 13, 2793-2825, 2013.
- McDougall, R. A.: Chapter 19: Updating and Adjusting the Regional Input–Output Tables, in: *Global Trade, Assistance, and Production: The GTAP 6 Data Base*, edited by: Dimaranan, B. V., Center for Global Trade Analysis, Purdue University, 2006.
- Moran, D., and Wood, R.: CONVERGENCE BETWEEN THE EORA, WIOD, EXIOBASE, AND OPENEU'S CONSUMPTION-BASED CARBON ACCOUNTS, *Economic Systems Research*, 26, 245-261, 10.1080/09535314.2014.935298, 2014.

