Earth Syst. Dynam. Discuss., 5, C510–C516, 2014
www.earth-syst-dynam-discuss.net/5/C510/2014/

Earth System Dynamics Discussions
Open Access

Interactive comment on "Uncertainty in temperature response of current consumption-based emissions estimates" *by* J. Karstensen et al.

J. Elliott (Referee)

jelliott@ci.uchicago.edu

Received and published: 15 October 2014

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.

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).

One step that is ignored in the causal chain defined here (consumptionâĂŤproductionâĂŤemissionsâĂŤclimate) 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).

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

ESDD
5, C510–C516, 2014

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.

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

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 consump-

ESDD

5, C510–C516, 2014

tion/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.

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

1025/18: typo? ". . .emissions with roughly 12%..." should this be ". . .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.

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.

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

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

ESDD
5, C510–C516, 2014

is not that large. . .

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.

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.

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.

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

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

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 multi-product 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.

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

ESDD

5, C510–C516, 2014

ified 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.

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 Plane-

ESDD

5, C510–C516, 2014

tary Sciences, Vol. 42: 341-379 (Volume publication date May 2014).

Interactive comment on Earth Syst. Dynam. Discuss., 5, 1013, 2014.