Earth Syst. Dynam. Discuss., 5, C427–C432, 2014
www.earth-syst-dynam-discuss.net/5/C427/2014/

Earth System Dynamics Discussions

Interactive comment on "Climate impacts research: beyond patchwork" *by* V. Huber et al.

V. Huber et al.

huber@pik-potsdam.de

Received and published: 14 September 2014

REFEREE 2: General: This paper provides a useful overview of the state of the art in climate change impacts modelling with particular reference to the question of uncertainties within impacts projection. It provides reasonable coverage of the methods that have been hitherto employed to address this important topic. It continues to describe a recent initiative to break new ground in this area and provides a useful summary of the methodology and some of the new findings. It can therefore be said to present a novel concept (that of the new model comparison process) and continues to suggest future useful research ideas, and reaches important conclusions about where research in this area needs to be improved.

The topic which the paper addresses, that of uncertainties in climate change impacts modelling, is of critical importance owing to the status of the UNFCCC climate change

negotiations and the upcoming review of the global temperature rise target. The topic is of relevance to the Earth System Dynamics Discussions.

Proper credit is given to related work, all of the references should be retained. The title and abstact are appropriate. The paper is clearly presented and well written,and the language is fluent and precise. As this paper is largely a review and opinion piece, describing the state of the art reached a result of a number of existing publications, and suggesting further ways forward, questions 4 to 6 asked of peer reviewers are not relevant to this paper. The figures used to illustrate the text are useful and should be retained.

One or two remarks made in the paper are questionable and should be reworded – for detail see below. There is one key issue that should be detailed further, see below.

P734 line 9-10. This sentence is questionable and needs to be reworded. It also prompts me to highlight an issue which is not discussed in the paper and needs to be highlighted and clarified. Delete 'modellers tend to adjust their models'.

I think the concept you should be conveying here is that model intercomparison needs to do the following (i) Allow identification of errors in input data, so that this can be excluded (ii) Isolate the influence of model structure, which requires harmonisation of model input data. The output of such a comparison does produce more convergence of output. (iii) However, once errors and structural issues have been explored/addressed, one then still has to go back and explore the total uncertainty, which results from the sum of uncertainties in input data and uncertainties in structure. Obviously, where there is genuine uncertainty in input data, a model intercomparison that then ignores this uncertainty is biased. In ISIMIP I think that of necessity input data has been harmonised? Rather than saying that modelers have been adjusting their data, implying malpractise, say that of necessity input data has to be harmonised in order to tease out those output differences that are dependent only on model structure (i.e. (ii) above). Then, when one returns to assessing the uncertainty in model output, one has to put

BACK the diversity in knowledge of the input data in order to encompass the full range of uncertainty (i.e. (iii) above). It is important to highlight whether this has or has not been done in the ISIMIP papers published so far, in other words, whether the range of uncertainties presented is actually lower than it should be due to artificial harmonisation of input data in order to isolate the effect of model structure only.

Of course, if separately, you think that modelers have been deliberately adjusting their models to produce a mean outcome, then that is quite a serious inditement of (presumably particular) scientists integrity and I would recommend that you steer clear of making such statements which would be extremely difficult to substantiate and likely to open up an unhelpful debate about scientific consensus about climate change in general that could undermine the work of large numbers of extremely meticulous and upright scientists.

AUTHORS: We did not want to imply any malpractice of modelers, intentionally adjusting their models so as to follow the ensemble mean. Yet, we agree with Knutti (2010) that a tendency towards consensus can be a potential shortcoming of model intercomparison projects. Based on his experience as a leading scientist of CMIP, he has termed this phenomenon an 'element of social anchoring'. In the revised version of the manuscript we have deleted "modelers tend to adjust their models" as suggested and cite Knutti (2010) more directly, using his wording (supplement: ll. 393-396). (In fact, in the previous version of the manuscript we accidently cited Sanderson and Knutti, 2012 on this point.)

Concerning the referee's more general statements about dealing with uncertainties in model intercomparison projects, we mention on several occasions that ISI-MIP relied on harmonized input data (e.g., supplement: ll. 193, ll. 197-202). The identification of errors was part of the process of providing data to participating modeling groups. The simulation output was then investigated in two ways as suggested by the referee: Some studies focused on structural uncertainties of impact models basing their analysis on a selected input dataset (i.e., one of the five GCMs and one RCP scenario considered

in ISI-MIP) (supplement: ll. 376-384). Other studies investigated "total uncertainty" combining uncertainties in input data (stemming from different RCPs, different GCMs, and bias-correction) with structural uncertainties from impact models (supplement: ll. 333-340). However, as we point out in the manuscript, this assessment of total uncertainty is strongly dependent on the considered model ensembles, both with regard to input (climate model) and output (impact model) data (supplement: ll. 358-360).

REFEREE 2: P733 lines 19-25 Remove the citation to Tavoni & Tol, this argument does not make any sense – just because only a few people have estimated something doesn't mean it is necessarily underestimated. It just means the numbers are less certain. In fact, economic costs of mitigation are probably overestimated because of lack of proper incorporation of economic gains resulting from investment in new technologies, and the incorporation of assumptions that the economy is in perfect equilibrium in many models. Secondly, cobenefits (such as energy security and improved health effects) were not included in the AR5 and these influences would seem to be far more important- effectively reducing the costs of mitigation by a large amount. There is plenty of evidence for these processes in IPCC AR5 and citations therein.

AUTHORS: The citation to Tavoni and Tol (2010) was meant to provide an example of a potential shortcoming of model intercomparison, rather than be a comment on the question whether economic costs of stringent mitigation were underestimated in the IPCC AR 4. In the revised version of the manuscript, we have reformulated the entire paragraph, so as to make a much more general statement on the importance of communicating model assumptions to policy makers (supplement: ll. 402-413). We now cite Tavoni and Tol (2010) together with another paper (Knopf et al. 2012), presenting their argument as part of a controversial debate about whether AR4 results were biased due to the selection of specific models in the underlying EMF ensemble.

REFEREE 2: Minor comments: P728 line 20-25 The millions at risk approach was implemented, not proposed. Please change 'proposed' to 'implemented'. I am not convinced that the population scenarios used therein were inconsistent – this study used SRES scenarios. Whilst these have since been updated, it does not follow that they were inconsistent. Such a statement needs backing up with a citation where this has been conclusively demonstrated.

AUTHORS: We agree that the millions at risk approach implemented by Parry et al. 2001, based on the SRES scenarios, is not a good example of a cross-sectoral synthesis that relied on partly inconsistent scenario input. In the revised manuscript, we cite two other review-type studies that summarized impacts as a function of global mean temperature rise (supplement: ll. 215-219). These studies relied on a huge literature basis with the trade-off that some inconsistencies in the underlying data could not be avoided.

REFEREE 2: P729 line 1. The discussion of the hotspots work should emphasize that this paper does not definitively identify the areas which are the most affected by climate change in the world, because it does not include all impacts sectors, and also it is very difficult to decide how to 'weight' different levels of impacts in different sectors. Rather, these hotspots perhaps show where interactions between climate change impacts upon different sectors will be most likely to manifest themselves.

AUTHORS: To our mind, it is impossible to "definitively identify the areas which are the most affected by climate change in the world". Any hotspot map will be dependent on specific assumptions, e.g., about what is considered severe change in the different sectors considered. However, we agree that the Piontek et al. study is only a first step towards a more general hotspot map, which would need to rely on data from more impact sectors and optimally account for adaptation potentials. We mention this point in the revised version of the manuscript (supplement: ll. 225-229).

REFEREE 2: P729 line 28 to P730 line 12. Consider moving this paragraph which seems out of place here.

AUTHORS: This paragraph discusses the integration of global, regional, and local models and therefore belongs to the section entitled "Integrating impacts projections

across sectors and scales". In the revised version we have added "spatial" in the section title, and we have changed the beginning of the paragraph (supplement: l. 264). It should now be obvious that we turn from discussing sectoral integration to discussing the integration across different spatial scales.

REFEREE 2: P731 line 11-21 Mention the debate over whether it is appropriate to weight GCMs, including whether their ability to represent current climate is related to their ability to represent future change.

AUTHORS: We have added a sentence in the revised manuscript (supplement: ll. 305-307) to express this specific caveat with regard to the weighting of GCM output.

Please also note the supplement to this comment:
http://www.earth-syst-dynam-discuss.net/5/C427/2014/esdd-5-C427-2014-supplement.pdf

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Interactive comment on Earth Syst. Dynam. Discuss., 5, 721, 2014.