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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Regional disaster impact analysis: comparing Input-Output and Computable General Equilibrium models" *by* E. E. Koks et al.

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Authors: first of all, we would like to thank referee #2 for her/his critical review on the manuscript. We believe that the critical comments and suggestions of the referee have resulted in a substantially improved manuscript.

1. What obvious is the difference of impact estimations among three recovery paths. Since the impact is defined as the difference between the production level before the event (dotted line in figure 2) and a recovery path (solid line), it is quite trivial that the convex path has the largest (negative) impact (largest area between dotted line and solid line in figure 2), followed by the linear path and the concave one.

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Authors: we agree that the results of the differences between the three recovery curves might be obvious. However, the literature on recovery of disasters is limited and much is still unknown on how businesses recovery after a disaster (especially after floods). Hence, the main reason why we have chosen to consider three different recovery curves is to point out the differences in impact between them. In the disaster risk management field, the modelling of macroeconomic losses is getting more and more acknowledgement, but much is still unknown. As such, we have tried to be as transparent as possible, also considering the fact that the reading audience of NHESS is mostly non-economist. A second reason why we implemented the curves is to show how models can behave differently with different types of shocks. A convex recovery will result in different model behavior compared to a concave recovery due to much larger disruption at the beginning. But, we agree that the difference (and similarities) between the modelling frameworks is the most interesting. As such, the results and discussion section now have much less emphasize on the differences between the recovery paths. Also, the following sentence has been rewritten in the description of the recovery paths:

"Due to the large uncertainty in the potential recovery path and duration, it is worthwhile to test the results between these three recovery paths. As such, the three recovery paths can be interpreted as a 'sensitivity analysis' of the results."

2. I do not think that it is not necessary to evaluate or highlight the differences among these three paths, since the main objective of this paper is the comparison of the impact estimation between IO and CGE models. Additionally, because the models used in this paper do not have any dynamic structure (temporal production and/or trade processes), I do not find any meaningful value of using the three different recovery paths. Therefore, it is sufficient to use just one typical recovery path and to compare what are the differences between IO and CGE models. If, on the other hand, the authors are more interested in the optimal recovery path given the initial damage, they may want to employ the dynamic optimization technique with a set of constraints, such

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as financial, labor, and resource constraints. Otherwise, using these three curves is just not useful or interesting.

Authors: dynamic optimization techniques to find the optimal recovery is beyond the scope of this manuscript (something interesting for a next study though!). In relation to our previous answer, we think it is interesting to show the differences, because we don't know the starting point given an initial damage. But, we agree that we should spend more attention to the results and discussion section on the differences between the models and the modelling outcomes. Please see the next comment how further improvements are implemented in the text.

3. While the paper's main objective is to compare the impact estimations between IO and CGE models, the analysis of the derived results looks shallow. The analysis is summarized mostly in tables 4 and 5 and figure 4. But these are just a direct comparison of results. What would be more interesting is to analyze which model features are contributed to those differences and to what extent. For improving these models, such analysis is needed.

Authors: We have rewritten the results and discussion section to focus more on the model features and why some results are different because of the different models. This has automatically resulted in a smaller emphasis on the recover curves (only one paragraph now explicitly talks about the recovery paths). All other paragraphs talk about how the differences in models come up with different results. An example is the third paragraph of the results section:

"This implies that the largest differences in outcome between the models are occurring in the multiregional effects of the disaster. More specifically, this means that the assumptions regarding multiregional spillover effects (whether or not substitution, additional imports or factor mobility) is an important determinant of the final outcomes."

Nonetheless, we indeed agree that the comparison could be improved. Below, we outlined how the manuscript is rewritten. The first paragraph of the results has been

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extended with the following sentences:

“The IEES model has, as expected, the lowest output losses in almost every model setup. The lowest losses for the IEES model can be explained mostly by the perfect substitution across sectors of labor and capital. This means that labor and capital can move from one sector to another one by no transition cost and may influence the reduction of losses even more than the potential increase in trade.”

The fourth paragraph is now rewritten and extended as follows:

“A closer look at the differences for the affected region in Table 5 shows additional divergences between the models compared to Table 4. Firstly, in Table 4 the ARIO model always predicts the highest losses for Italy as a whole. In Table 5, where only the losses are shown for the affected region, this is only the case for the concave recover path for the Veneto flood event. In all other scenarios, the induced losses calculated by the MRIA and the IEES – Flex model are higher. This may imply that allowing for more flexibility in the model results in higher losses in the affected region. In the MRIA model, this may be explained by the maximum regional capacity. In contrast to the ARIO and IEES model, the MRIA model sets a maximum capacity on the regional production (See Section 3.3). This regional maximum capacity prevents that products which are normally considered as a byproduct, will become the main product to its full extent. When a byproduct will be produced as a main product due to an increase in regional demand, taking into account the Leontief structure of an IO model, the production of the main product will go up as well. This induces the inefficiencies in regional production that are limited by the regional production limit. As such, the model will turn more quickly to alternative suppliers from different regions, which do produce the demanded product as a main product, reducing the inefficient production overall. For the whole of Italy this result in lower losses compared to the ARIO model. In the IEES – Flex model, a similar substitution process occurs with the movement of production factors to other non-affected regions (not possible in the IEES – Rigid). ”

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And the following lines to the fifth paragraph of the results:

“For the IEES models, it is important to note that the productivity shocks decrease the demand for labor and capital because of lower productive capacity and income. This means that remunerations of capital and labor go down and in the IEES- Flexible model capital, and labor can move towards not affected regions. This determines the exacerbation of the profit and losses dynamics and is the main cause for the difference in regional economic losses between the IEES-Flexible and -Rigid model.”

A second paragraph of the discussion section has been added:

“Although the empirical literature finds that the sign and size of population responses varies substantially between different flood events, there is some evidence of post-disaster labour mobility. Husby et al. (2014) finds that the large-scale flood in the Netherlands as well as the reconstruction activities following the 1953 flood had a positive long-term effect on population growth in affected municipalities. This study thus finds some evidence that the reconstruction of affected areas was not restricted by fixed labour force. Nonetheless, since much is still unknown on the potential post-disaster movement on capital and labor, more empirical studies should analyze the post-recovery process in high-income countries.”

And a third paragraph is added as well to the discussion section:

“Besides the differences in multiregional spill-over effects, two additional results within this paper can contribute to the current literature. Firstly, the losses in the affected region (Table 5) itself is relative similar throughout all model setups compared to the losses for the whole of Italy (Table 4). This indicates that the different multiregional models considered in this study all capture the economic effects for the region directly affected by the flood in a similar order of magnitude. In West (1995) and Hu et al. (2014), the differences in outcomes between the IO and CGE models are much larger (in relative terms) in a single-economy framework. This implies that the difference in use between the multiregional models is less of an issue when one wants to know

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what the impacts are for the flood-affected region(s). Secondly, the mobility of capital and labor across sectors and regions in the IEES model and the inefficiency costs of the MRIA model can indicate how resilient an economy is, both on a regional and national level. Large inter-regional mobility effects (IEES) or high inefficiency costs (MRIA) within the affected region may indicate that the region struggles with the impact of a disruption. On the other hand, as also shown by the positive results in the other (non-affected) regions, the national economy may be rather resilient, with low inefficiency and high factors mobility. The ARIO model, using a more traditional IO framework, may be less straightforward in interpreting the economic resilience, lacking the characteristics of either inefficiency costs or mobility effects."

4. One more problem: in page 7071, under section 6, there is the indication "the "truth" might be somewhere in the middle of the results." Without knowing the so-called "truth", it is inconsequential to say this. It is more important to find empirically the actual impact of a particular disaster and then to compare the derived estimations using models than to tweak models to derive some results for a hypothetical case that may not be evaluated with "truth".

Authors: we agree that using the worth "truth" is a bit too ambitious. As such, we have removed this term from the manuscript. One of our objectives is to derive the indirect economic effects of a disaster. It's very hard to derive these indirect effects "empirically" because an economic system is a complex object and isolating the disaster economic effects "empirically" is not easy even for a regional economy. For this reason, macroeconomic models can be useful to test the economic performance by changing specific conditions, ceteris paribus. In line with this "truth" statement, we have also removed the "best of both worlds" statement from the manuscript (Section 2.3.), regarding the use of hybrid models. Refraining from this word use, should give the paper a more objective view on the models used, and the outcomes they give in this manuscript.

There are some minor issues as follows: - In page 7058, there is the indication, "Partial economic analysis such as IO analysis does not link income to expenditure." This is not

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true. Please refer to Miller and Blaire (2009) for the closed IO model and the Miyazawa extended IO model, which includes households as a sector and links income and expenditure endogenously, respectively. Authors: This has been adjusted (removed) in the manuscript.

- Section 2.2 discusses in detail about the differences in modeling frameworks. Since the discussion in the body text appears extensive, I do not feel that the additional Table 1, which summarizes the same discussion, is needed. Authors: we understand the referee's point, but we still think it is nice to have a table that summarizes the differences, next to the explanation in the text.

- Section 5: English becomes rough and erroneous in this section. Authors: We have gone carefully through the text again, and rewrote the manuscript where necessary.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 7053, 2015.