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

Anonymous Referee #2

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This paper aims to evaluate the differences of impact estimation between IO and CGE models. It is an interesting topic; however, what presented in this paper are rather obvious and the derived results are not investigated thoroughly enough.

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.

I do not think that it is not necessary to evaluate or highlight the differences among

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

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.

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".

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 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.

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- 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.

- Section 5: English becomes rough and erroneous in this section.

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Sincerely,

Anonymous Reviewer

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