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Discussions

Interactive comment on "Modeling economic costs of disasters and recovery involving positive effects of reconstruction: analysis using a dynamic CGE model" *by* W. Xie et al.

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Review of "Modeling Economic Costs of Disasters and Recovery Involving Positive Effects of Reconstruction: Analysis using a Dynamic CGE Model" by Wie Xie et al.

This paper presents a methodology for analyzing the macroeconomic impacts of disasters, with an emphasis on the impacts of post-disaster reconstruction investment. The method is applied to a case study of a major earthquake in China. The model is well crafted and reasonably well presented, and the application to the Wenchuan earthquake carried out adeptly. The paper makes a significant contribution to the literature,

and I recommend publication subject to revisions. The revisions pertain primarily to terminology, need for more explanation at some junctures, and to organization of the paper, rather than to lack of substance or major flaws.

The major revisions include:

1. The authors need to improve some important matters of terminology. They imply that disaster losses are the net effect of the impacts of the disaster and the impact of reconstruction spending. Most analysts would disagree. The standard terminology in the literature is that losses refer only to the first of these impacts, and that the effects of recovery investment are an "offsetting" factor – they do not actually reduce losses but rather represent an activity that stimulates the economy. The distinction pertains a great deal to the important economic concept of "opportunity costs," which refers to the next best use of funds. Also, the funds for investment do not just grow on trees, but t have alternative uses and hence should not be viewed simply as a gain, at least without some consideration of the reduction in positive impacts from their alternative uses. In some cases this is a geographic consideration, as an inflow of funds for investment in a disaster-stricken area come from outside, so, while there is a net gain to the affected region, the shift in funds is a drain on the national economy. Without this consideration, it would seem that disasters might in fact have positive impacts, when one considers the especially strong investment stimulus in some regions (e.g., in the aftermath of the Northridge Earthquake in the mid-1990s). (The authors mention some aspects of this consideration on p. 26, l. 26.) The bottom line is that it is not the losses that are over-estimated, but it is the trajectory of the economy that is under-estimated. I agree with the authors that the difference is the positive effect of investment, but this is not really a reduction in losses, but simply an offsetting factor coming from another source.

2. Another omission relates more specifically to the opportunity cost of the investment in reconstruction. The authors need to explain whether this investment improves the productivity of the economy. For example, if the new plant and equipment is on average the same as that destroyed, then there will be no increase in the productivity.

However, it is likely that reconstruction will embody the latest technology, and there will be productivity advance. This would cause an even greater stimulus. On the other hand, if the investment funds diverted from other sectors would have come from highly productive investments, this would have to factored in as well, and would reduce the positive offset. The authors need not estimate this, but should at least mention it.an offsetting factor.

3. The reference to the U.S. Geological Survey Multi-hazards Demonstration Project (2010) is too vague. It states that the "Recovery of capital stock was simplified." In fact, the recovery of capital is mainly investment driven, and investment will be affect the demand-side of the economy." In the USGS formulation, demand-side stimulus from investments is in fact balanced by an increase in the productive capacity of the economy in the model. In fact, that study accomplishes what the authors of this paper intend, an analysis of both the losses and recovery stimulus of a major disaster. The authors need to more clearly explain how their approach differs from that in the USGS formulation and a subsequent version of the study (Sue Wing et al, (2012).

3. The authors need to explain why there will be an increase in economic activity over time without reconstruction investment. What are the elements or dynamics of this process? The authors also need to explain why they assume that the investment flow is one-half of the total investment funds needed.

Some more specific recommendations include:

p. 1, Title–I recommend the title be changed to "Modeling Economic Cause of Disasters and Recovery: Analysis Using a Dynamic Computable General Equilibrium Model." The phrase "Involving Positive Effects of Reconstruction" is implicit in the term "Recovery." Also CGE model should be spelled out.

p.6359, l.23–The authors should be careful about using the term "induced effects", since this has a very specific meaning in multi-sector modeling, specifically input-output analysis, which is the underpinning for CGE models.

p.6361, l.6–The authors do not explain Hallegatte's "overproduction capacity" parameter very well.

p.6361, l.12– The authors need to explain the "contradictions between the requirement of highly precise data input regarding debt loss and the imperfect methodology used when assessing direct economic losses due to natural disasters."

p.6361, l.17–The authors should insert a short paragraph on the role of resilience in estimating disaster impacts.

p.6361, l.18– The statement of purpose should be moved closer the front of this section.

p. 6363, l.23–The authors have not sufficiently explained why it is necessary to extend the static CGE model into a dynamic one. The fact that there are more than one time period involved is not a sufficient reason. One can simply run a static model over several periods if this was the only distinction.

p.6364, l.20–It is not clear why the two models (i.e., actually two closure formulations) "cannot factor the impacts of disasters on the economy adequately because disasters also have significant effects on employment and incomes." Are not these effects readily forthcoming from the operation of these models? This might be possible due to interest rate adjustments, but the authors need to explain this explicitly.

p.6365, l.11– It is possible for investment to drive savings, if savings are considered more broadly. One can have a fixed savings rate for the afflicted region and allow savings inflows from other regions to balance the equation. Is this what the authors have in mind? Also, the last sentence in the paragraph is not clear.

p.6369, l.6–This sentence is too vague. Which parameters need to be adjusted?

p.6369, l.16– It is not clear why "Pre-disaster conditions cannot serve as a benchmark."

p.6373, l.3–Why is there such a huge disparity between increased production of the

Construction and Building Material industries?

p.6375, l.25–The Conclusion is far too long. It should just include a few of the high points of the paper rather than summarize the paper in detail. p.6378, l.25–The "Discussion" can be shortened significantly as well. It should not be a separate section but simply provide some suggestions for future research.

p.6380, l.21– The authors should cite a book paper that explains the "a traditional" CGE model. One suggestion is to refer to some of Peter Dixon's work.

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