

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 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 overestimated, 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.

Thank you for your comments and explanations. We will substitute the expression ‘reconstruction investment will reduce economic losses’ with ‘the effect of reconstruction investment is an offsetting factor’ in the new version of our manuscript.

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 be factored in as well, and would reduce the positive offset. The authors need not estimate this, but should at least mention it as an offsetting factor.

It is a fact that reconstruction investment will improve the productivity of the economy. As you mentioned, this manuscript need not estimate this impact due to unavailable data of improved productivity resulted from new plant or equipment. But for comprehensiveness, we should list other impacts of reconstruction investment. A description of other offsetting factors will be included.

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 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).

We apologize for our vague reference. In the study of Sue Wing et al, the model only connects non-reconstruction investment with the aggregate save in California. This model assumes demand-side stimulus from investments is balanced by an increase in the productive capacity of the economy. However, in reality, reconstruction investment may be funded by federal government and disaster-affecting area themselves. The reconstruction capital goods can be balanced by an increase in the productive capacity of local economy or be transported from the rest of the country. In order to simulate the reality, our model connects the aggregate investment (the sum of normal investment and reconstruction investment) with aggregate savings (the sum of local savings and savings from outside of disaster-affecting area, e.g., donations from other area or financial support from central government). In our CGE model, the saving from other areas is an endogenous variable. Thus, our simulation allows collection of reconstruction funds from the rest of the country. Meanwhile, the saving from other areas equates trade surplus. Thus, our simulation also allows transportation of reconstruction goods from the rest of the country.

4. 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.

Even under the scenario without reconstruction, normal improved productivity (i.e.,TFP in the CGE model) exists. On the other hand, there is ordinary investment under this scenario. These two elements increase the economic activity over time.

The speed of investment restoring capital stock under post-disaster reconstruction circumstance is quicker than that under normal circumstance. In order to estimate this offsetting factor of reconstruction, in CGE mode one-half of the total investment is assumed to form capital stock in the first year, and the remaining will restore stock in the next year.

We apologize for our vague description. We will add these two explanations in our article.

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.

Thanks. Done.

p.6359, 1.23–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.

The effect in CGE model is the reallocation of national income, i.e., the behavior of transfer payment from local government and central government. This effect just simulates the reconstruction funds partially collected from government or donations. After we understand the definition of ‘induced effect’ and the effect we would like to express again, we found they have different meaning. Thus, this term will be revised in the new manuscript.

p.6361, 1.6–The authors do not explain Hallegatte’s "overproduction capacity" parameter very well.

We will explain this more clearly: overproduction capacity (sectors can instantaneously produce α times more than the pre-disaster production level if demand increases. This overproduction can also increase because additional equipment and workers can move to the affected region).

p.6361, 1.12– 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."

We will explain this more clearly. It means that those previous studies made many contributions to how to incorporate direct economic loss into CGE or IO model subject to the current imperfect direct economic loss assessment methodology.

p.6361, 1.17–The authors should insert a short paragraph on the role of resilience in estimating disaster impacts.

The topic of disaster-proof reconstruction pertains to the hot topic of ‘resilience’. We will add a paragraph on the role of resilience when assessing disaster impacts.

p.6361, 1.18– The statement of purpose should be moved closer the front of this section.

This minor suggestion will be incorporated into the revised manuscript.

p. 6363, 1.23–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.

During post-disaster period, the reconstruction funds in the current year come from savings in the previous year. It means that there is a financial link among every year after disaster.

So a dynamic CGE model is needed. This new reason will be included.

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.

Disasters have significant effects on both labor supply and wage rates. However, the standard closure rules hold one of these constant.

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.

Yes, this is the meaning we would like to express. We will explain this more clearly.

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

This sentence will be expressed clearly as ‘the impacts of a disaster and ensuing reconstruction on an economy can be reflected by adjusting the exogenous variables $Damage_{i,t}$, $QINVd_{ij}$ and $QINVh_i$ in the CGE model’.

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

The explanation sentences before this statement will be revised as ‘In a rapidly growing economy such as that of China, because the amount of new investment (even excluding reconstruction investment) may be larger than the amount of destroyed capital stock in the disaster, the social and economic aggregate levels in the year when disaster occurred even may surpass the pre-disaster level. In other words, if no disaster would take place, the social and economic aggregate levels are much higher than that during the previous year. Apparently, a higher economic level compared with pre-disaster status does not mean that social and economic conditions are recovered. This means that when assessing economic loss induced by disaster, it is wise to choose no-disaster scenario as a benchmark’.

p.6373, l.3–Why is there such a huge disparity between increased production of the Construction and Building Material industries?

A explanation that ‘Most reconstruction activities need the participation of the Construction industry, but not every reconstruction activity need building materials, such as cleaning debris or site performance for new buildings’ will be added.

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.

We agree this suggestion. We will revise this part.

p.6378, 1.25–The "Discussion" can be shortened significantly as well. It should not be a separate section but simply provide some suggestions for future research.

We agree this suggestion. We will revise this part.

p.6380, 1.21– 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.

This reference will be included.