

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1  
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## Comment on nhess-2022-38

Anonymous Referee #1

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Referee comment on "Are the Rich less Prone to Flooding? A Case Study on Flooding in the Southern Taiwan during Typhoon Morakot and Typhoon Fanapi" by Yen-Lien Kuo et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-38-RC1>, 2022

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This study aims at providing evidence that income affects flood risk mitigation. The authors claim that their analysis shows that this is the case. I have great problems with this conclusion.

The authors state that "high-income individuals may have used their political influence to influence the budget allocation to improve the flood risk reduction facilities in their communities" (Abstract, and Page 15, Lines 286-287). That is quite a statement, that requires strong evidence. The statement would require that 1) flood risk has actually decreased in those high-income areas, and 2) that the flood risk also has been reduced \*more\* in areas with higher incomes, compared to areas with lower incomes. However, neither of these is shown in the analysis.

The only thing the authors show is that there is a difference between income and flood risk. But this is well-known from past research in developed countries as well as developing countries. Lower income households settle in locations that are more flood prone, for several reasons, often a higher flood risk also leads to lower property prices, leading to poorer populations to move here.

I do not doubt that mechanisms of political influence, and nontransparent processes are at play in Taiwan. However, the current study simply cannot deny or confirm any of that to have an effect on actual reduction of flood risk.

Answering the central claim from the paper would require an analysis of the flood hazard before and after the programme, to analyse whether there is any \*difference\* in flood risk reduction for the different income groups. So how was the flood probability of the communities before the programme that started in 2008? The authors cannot show that.

In Tables 5 and 6, in fact some of the effects of the location choices that I refer to can be seen. In particular, elevation plays a role here (and is related to flood probability, as seen in Table 4), with the low-income group having a lower elevation, and thus potentially a higher flood hazard.

Also, I wonder about the uncertainty of the flood probability estimates. The authors report that this is collected from self-reports (Line 198), but how could this affect the analysis?

Additionally, the authors cannot exclude the possibility that floods from typhoons had effects on income, as they suggest also themselves on page 3 (Lines 84-87). Although the income data is from 2006, the authors also report that several typhoons hit Taiwan every year, and such impacts could affect incomes, so this could in fact be an additional factor, as shown also in other studies (e.g. <https://doi.org/10.1016/j.ecolecon.2020.106879> and <https://doi.org/10.1016/j.jenvman.2022.114852>).

Finally, I have reservations about whether the programme has led to such investments that there would be a noticeable effect on flood risk for these two specific events. \$3.86 billion seems a lot, but it also seems this was spent on quite a large area, and both events were quite extreme.

Moreover, the limited description seems to imply that most of the implemented measures would actually benefit several riparian communities, such as “construction works” that suggests structural flood protection, such as levees and reservoirs. Or are there any engineering reasons why the measures would have benefitted certain geographic locations, and not others? The current description is highly suggestive (Lines 54-70), but lacks factual descriptions of what investments and construction works were made.

In sum, I think the main conclusion from the paper is not supported by the research design and the results. The authors only show that the lower income communities have a higher flood risk.