

***Interactive comment on “Research article:  
Household resilience to major slow kinetics  
floods: a prospective survey of the capacity to  
evacuate in high rise buildings in Paris” by  
Nathalie Rabemalanto et al.***

**Anonymous Referee #1**

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Summary: This manuscript addresses an important and unfortunately neglected issue—flood evacuation from high-rise buildings. The literature review is good but, as noted below, can be improved. The sample size is excellent, although the response rate is low and so the sample’s representativeness is uncertain. However, as the authors note, the response rate is comparable to other mail questionnaires so it is not a major flaw in the study. The cluster analysis of the respondent profiles is a noteworthy innovation. The paper’s conclusions

Line Comment 115 Other factors likely to affect flood evacuation decisions are envi-

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ronmental cues; social cues; warning sources, channels, and messages; protective action perceptions, stakeholder perceptions, and personal characteristics (e.g., sensory and physical mobility limitations, hazard experience)—see Lindell (2018). Some of these are mentioned later in the manuscript—experience on line 135, warnings on line 138, social cues on line 147, and environmental cues on line 148. These other factors should be summarized here.

128 It is indeed possible that the effects of demographic variables on evacuation is complex. Alternatively, the variation across studies in the significance of demographic variables in predicting evacuation can be explained as little more than random fluctuations that Baker (1991) characterized as small and inconsistent among studies and Huang et al. (2016) Figure I showed have consistently small effect sizes when aggregated in a statistical meta-analysis (SMA). At the very least, the authors should acknowledge that the effect of demographic variables is controversial.

134 I re-read Dash and Gladwin (2007) and I can’t find any statements that support the proposition, implicit in this statement, that experience causes risk perception which, in turn, causes evacuation. In fact, Dash and Gladwin repeatedly propose experience and risk perception as competing predictors of evacuation. Also, I can’t find the term “awareness” anywhere in the Whitehead et al. (2000) article.

232 Huang et al. (2016) cited 11 studies of evacuation expectations (what the present authors call prospective surveys).

243 Lindell and Perry (2012) is not listed in the Reference section.

286 Social cues are observations of other people’s behavior that influence them to evacuate. The most common social cues are observations of businesses closing and other people evacuating. Social assistance is most commonly seen in people staying with peers (friends and relatives) rather than in commercial facilities (hotels or motels), government shelters, or other locations (e.g., second homes). Lindell et al. (2019) Section 6.2 summarized the US research as indicating that an average of 62% of evacuees

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stay with peers, 27% stay in commercial facilities, and 3% stay in government shelters (auditoriums or gymnasiums of schools or churches).

298 The acknowledgement in this statement that some studies—especially Baker’s (1991) review and the Huang et al. (2016) meta analysis, concluded that demographic variables do not seem to be significant predictors of evacuation—needs to be reconciled with the discussion of demographic variables at line 128.

337 Huang et al. (2012) also cited a response rate of 24.6% from the Texas coastal evacuation expectations survey by Lindell et al. (2001). In addition, Huang et al. (2012) p. 294 point out that the concern about low response rates is that some groups are under-represented. However, any bias in demographic characteristics is only relevant to the degree that these characteristics are correlated with evacuation expectation and the variables that are highly correlated with it. In fact, as noted earlier, the correlations of demographic characteristics with psychological variables and evacuation expectation are generally very small even when they are statistically significant. Consequently, demographic representativeness might not be as big a problem as many authors seem to think.

349 Chang (2009) is mis-cited as Chang (2019) in the Reference section.

428 I don’t understand what is meant by the occupational category “inactive”. The authors should explain this term.

438 Pets in the home are indeed a likely evacuation impediment, but this obstacle can be avoided if people know that there are places where they can evacuate with their pets.

504 It seems odd that the authors’ conclusion about the importance of authorities did not cite the most rigorous and comprehensive review, Huang et al. (2016), because their review shows that this is the most important predictor of evacuation.

544 The discussion in subsequent lines suggests that “responsive” (which implies a

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passive lack of cooperation) would be a better word choice than “reactive” (which implies active opposition).

546 Although the relationship between how people respond to a behavioral expectations questionnaire and how they actually respond in a disaster is not perfect, it is statistically significant and positive (Kang et al., 2007). Moreover, the variables that predict behavioral expectations also predict actual response in a disaster (Huang et al., 2016).

577 Baker (1991) reported a narrative review of hurricane evacuation studies conducted prior to his article and Huang et al. (2016) reported a more powerful SMA of hurricane evacuation studies conducted between Baker’s review and 2014. The results of the Whitehead et al. (2000) and Dow and Cutter (1998) studies were included in the Huang et al. (2016) SMA, so it is a logical error to cite those two studies as if they provided independent evidence. A rough analogy at the level of a single study would be to find a correlation of  $r = 0$  between gender and evacuation but argue that there is some evidence that women tend to evacuate because a subset of the women did evacuate. Given the similar findings between findings from Baker (1991) and Huang et al. (2016), any studies reporting contrary findings are most likely to be due to random sampling fluctuations.

625 The conclusion about the relationship between floor level and evacuation expectation would be stronger if supported by a  $\chi^2$  test.

662 As a minor point, the quote from Dash and Gladwin is true for hurricanes but not necessarily for inland floods—see the Lindell et al. (2019) report on the Uttarakhand flood.

670 The information provided in hazard awareness brochures and hazard awareness meetings is not necessarily limited to those who read the brochures or attend the meetings. Lindell et al. (2015) found that more people knew they should evacuate immediately after a severe earthquake because it could cause a tsunami than had read haz-

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ard brochures or attend hazard meetings. The likely reason is that this information was passed through informal social networks either before the earthquake or immediately after it struck.

679 The reference to the Colbeau-Justin and de Vanssay (2001) and Becerra et al. (2013) papers raises the question how prevalent were rumors about and denial of the flood risk? Rumors and denial are always present, so the question is whether these were characteristic of 1% of the population or 99% of the population.

681 It is unclear what is meant by “alarm systems”. Are these warning systems?

694 I think it is fine to credit Thouret and D’Ercole (1996) for presenting the concept of risk personalization but the authors should also mention Mileti and O’Brien (1992), who presented this term earlier based on Withey’s (1962, p. 106) theorizing.

702 It is unclear if the authors intend “should not create panic” to mean an injunction (“We don’t want this person to create panic because it is possible for that to happen.”) or an expectation (“We don’t expect that this person will create panic because people won’t panic in response to this type of information.”). If the authors intended this statement as an injunction, they are mistaken because panic is extremely rare even during life-threatening disasters—see Lindell et al. (2006) Chapter 8. If the authors intended this statement as an expectation, they should restate it that way.

716 As noted in my comments on line 546, there is relevant research on the relationship between expected and actual evacuation behavior.

726 Asking people to endorse specific reasons why they didn’t evacuate seems like a good idea, but it is actually not. Such questionnaires typically ask people if they evacuated and then branch to two different groups of followup items—one group of items for those who did evacuate lists reasons why they did evacuate and different group of items for those who did not evacuate lists reasons why they did not evacuate. As an example, suppose that one reason for not evacuating is “I was concerned about

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leaving my pets”. The problem with providing this item only for those who did not evacuate is that there are probably people who did evacuate that were also concerned about their pets. Indeed, it is possible that people who did evacuate were just as concerned about their pets as those who did not. Unfortunately, the structure of the questionnaire makes it impossible for the researcher to find that out. A better way to address the issue is to have one item that asks “When you were deciding whether or not to evacuate, to what degree were you concerned about the safety of your pets?” and another item that asks “Did you evacuate?” Calculating the correlation between the responses to these two questions makes it possible to assess the degree to which concern about pets distinguishes between evacuees and non-evacuees rather than assuming that concern for pets is only relevant to non-evacuees.

732 As noted in my comments on line 577, the Baker (1991) narrative review and the Huang et al. (2016) SMA summarize the literature more effectively than any list of individual studies. Additional individual studies are appropriate to include only if they were not included in the Baker (1991) or Huang et al. (2016) reviews. This would be the case for hurricane studies conducted since 2014, or for any studies of inland floods or tsunamis—neither of which were addressed in those reviews.

765 The claim that “most studies focus on past experiences” seems to conflict with the Kellens et al. (2013) statement that only a small amount of research on flood risk perception and communication has studied households’ immediate behavioral response to imminent flooding. The apparent discrepancy should be explained.

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