Dear Editor, Dear Co-Editor, Dear Referee,

First, we would like to thank you for providing us with a very detailed referee report, rich in helpful and insightful suggestions. I will address your comments one by one below.

Title and abstract

1. The title mentions “political conflict”. However, this is only one component of the INSCR political fragility index exploited in the analysis, so the title is somewhat misleading. Note that also in the text the authors sometimes incorrectly talk about warfare in place of political instability index.

We changed the title and text according to the suggestions above, using “state fragility” or “state fragility index” only in the context of our analysis.

2. The title also points to causal evidence, but I have some doubts about this claim, as I will clarify below.

We deleted the word “causality” from the title in favor of a more general description of the econometric results.

3. The abstract has few typos.

Abstract was revised and typos were corrected. Rest of the text was also revised and typos were corrected and text clarity was improved.

Initial discussion and literature review

1. I think the authors state what they do in the paper at the end of an introduction that can be made sharper. Given that the focus of the paper is knowing the effects of natural disasters (ND)/political instability (PI) on the banking system, I would start by saying that there is a huge literature on ND (less sure about PI) which focuses on their direct effects in terms of deaths/affected and their effects on GDP. Effects on banking stability have not been explored yet (unless partially in some studies cited in paragraph 4), so this paper is very valuable. The more so, because of the reasons you state in paragraphs 1 and 2 of the introduction (i.e ND are a key risk factor, etc.).

Suggestion was incorporated in the introduction section.

2. Regarding PI and its role for banking stability, the authors cite in paragraph 5 Dell, Jones and Olken (2012), but this paper actually looks at the role of temperatures (ND) on PI. I’m not sure if
the aim of the authors is to point out that ND are a component of PI, thus when they look separately in the econometric analysis to the effects of ND and PI, they are actually analyzing the same phenomenon? If not, aren’t ND a possible confounder of the effect they find for PI?

We believe that the advantage of the VAR methodology used in our article is that it is particularly suited to solve the problem mentioned above, meaning that the VAR analysis can separate and identify the components of ND and PI that affect banking stability even when ND also affects PI or vice-versa.

2. Equations (3), (4) and (5) seem to me the theoretical justification of the empirical strategy so I think they should go together with the “econometric methodology”. Thus, I think the material in the sections titled “econometric methodology” and “data and empirical results” should be rearranged in two sections titled “data and empirical strategy” and “results”.

Sections rearranged according to suggestion.

3. The authors proxy ND with the variable Disability Adjusted Lifeyears index of Malal and Noy (2015). All the references I found cite Noy as the only author, please check. Is the analysis robust to the use of more traditional measures of ND (objective measures such as temperatures or less objective measures such as number of deaths/affected)?

References to Malal and Noy were corrected. We are not able to offer an answer concerning the robustness to changes in ND proxies. We assumed before the writing of this article that the most relevant variable is lifeyears lost index due to dependency of the banking sector business model on the longevity and ability of their customers.

4. Most important: the time span exploited by the authors is the period 1999-2011. This is a crucial period of bank system instability due to the financial crisis. It is true that the financial crisis started in developed countries, however we all know how developing countries were also badly affected through the financial system. How can the authors address this concern in the data?

The panel data includes developing countries that, even though affected by the crisis at some point in time, were not affected as strongly as the central economies. When affected, the timeline of events tended to differ among countries. We do not believe therefore that, in the case of the group of developing countries used in this study, and due to the VAR empirical strategy, this should be a major concern, since financial shocks will be identified by the VAR regressions as residuals/innovations.

5. Again, how can we separate the role of ND from PI given that some components of PI are economic and social instability? Can the authors be clearer about the data imputed to construct the PI index?

Please see our answer to question 2 in the Introduction and Literature Review section above. Also, the state fragility index components are described in the third paragraph of the “Data” section of the article.

Results

1. The authors present impulse response functions (IRFs) and discuss the results: according to their reading they find that ND increase the amount of aggregate non-performing loans and the
likelihood to default, while they decrease both financial system deposits and banking deposits. As I pointed out earlier I am not an expert in VAR models, however, from what I see from the graphs I find it hard to agree with the authors since the standard errors intervals in Fig. 3, 4,5 and 6 are so large that the effect cannot be considered different from zero. The same observation applies to the results for a shock in PI (or state fragility index).

The text has been altered to acknowledge this fact and also by making the distinction between economic and statistical significance.

2. The authors also emphasize the “puzzling result” (in their words) that both ND and PI do not significantly affect GDP per capita. This result is actually not puzzling if the authors look at the metaanalysis of Lazzaroni and Van Bergeijk (2014). Incidentally, this reference is in the References section but it is not cited in the main text. By statistically analyzing the results of more than 900 regressions in about 30 studies on the effect of ND on GDP Lazzaroni and Van Bergeijk (2014) find that ND have an insignificant effect in terms of GDP losses. Thus, the result concerning GDP is not puzzling at all

References to mentioned articles have been corrected, and the mention of puzzling results has given place to a citation of Lazzaroni and Van Bergeijk (2014).