AGU Advances

Authors' Response to Peer Review Comments on

Robust Earthquake Early Warning at a Fraction of the Cost: ASTUTI Costa Rica

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Authors' Response to Peer Review Comments on First Revision of Manuscript (2021AV000407R)

Please see attachment that begins on the next page.

Response to Reviewers
12 May, 2021
Prepared by Ben Brooks

** CORRESPONDENCE FROM AGU ADVANCES***
Dear Dr. Brooks:

Thank you for submitting "Smartphone-based Earthquake Early Warning with a Fixed Network:ASTUTI Costa Rica" [Paper #2021AV000407R] to AGU Advances. I have received three positive reviews of your manuscript, which are included below and/or attached.

Many of the comments would be straightforward to handle, but there were points raised, particularly by reviewer #2, that I would like to see addressed before making a final decision on the paper. I concur with reviewer #2 that the paper is long and perhaps repetitive at points, and I think that more could be done to clearly discuss why your approach represents an advance that will have scaleable and exportable impact. I would suggest that you might revisit your title so that it conveys more of your main result. Also, your abstract and conclusions might better address the broader impacts of your paper (imagine you were writing the brief Editor's highlight for a broader audience). I am therefore returning the paper to you so that you can make the necessary changes.

Please submit a revised manuscript that addresses the reviews and any editorial comments by May 12, 2021.

In your revision, please follow our Checklist and use our Templates for the main file and any supplements. Please provide the following:

1. A response to reviewer file that lists each major comment and describes how the manuscript has/has not been modified in response to those comments.

2. A copy of the manuscript with the changes noted (e.g., highlighted, "track changes," italics or bold changes).

3. The final revised manuscript with changes incorporated and separate final figure files (figure parts should be combined into a single file), which will be used for publication if the manuscript is accepted. If final figures are already uploaded, they can be easily copied over to the next revision version.

4. If any, supporting information text, figures, captions, and small tables in single PDF file using AGU's template. Large data tables and multimedia should be uploaded separately.

AGU requires that data needed to understand and build upon the published research be available in public repositories following best practices. This includes an explicit statement in the Acknowledgments section on where users can access or find the data for this paper. Citations to archived data should be included in your reference list. All references, including those cited in supporting information, should be included in the main reference

list and cited in-text. All listed references must be available to the general reader by the time of acceptance. AGU requires the corresponding author, and encourages all authors, to register for an ORCID.

Please check and verify authorship, and that all authors are included, have approved the revisions, and agreed to be listed in the order given. Authorship is final with publication. Responsibilities of the corresponding author are given here.

When you are ready to submit your revision, please login to your account (https://advances-submit.agu.org/cgi-bin/main.plex) and click "Revise 2021AV000407R."

I look forward to receiving your revised manuscript. If you have any questions, or need additional time to complete your revisions, please contact us at advances@agu.org.

Yours sincerely,

Peter Zeitler
Editor
AGU Advances

***** POINT-BY-POINT RESPONSES*****

Editor Peter Zeitler recommendations:
I would suggest that you might revisit your title so that it conveys more of your main result.
We have changed the title to: Robust Earthquake Early Warning at a Fraction of the Cost: ASTUTI Costa Rica

Also, your abstract and conclusions might better address the broader impacts of your paper (imagine you were writing the brief Editor's highlight for a broader audience).
Thank you for these comments. In response, we have re-written the Abstract and Conclusion following the editorial suggestions. In particular, in the Abstract, Conclusions, and in the first paragraph of the Discussion section we have more clearly stated the ease with which the approach can be scaled.

Reviewer #1 Evaluations:
Recommendation: Return to author for minor revisions
Significant: Yes, the paper is a significant contribution and worthy of prompt publication.
Supported: Mostly yes, but some further information and/or data are needed.
Referencing: Mostly yes, but some additions are necessary.
Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.
Data: Yes
Accurate Key Points: Yes

Reviewer #1 (Formal Review for Authors (shown to authors)):

The author provide a smartphone-based EEW system and show its performance in Costa Rica. What I am most interested in is the data that generates by smartphones (or precisely the MEMS sensor in the phone). As the author didn't mention the phone model that was used in this EEW system, readers may want to know more about the data situation. So I suggest you add some more details about the phone model selection, such as what's the considerations about the phone model? Did you take any tests on different models (MEMS)? and what are the main differences?

We had mentioned the phone model, although we did so only in the supplemental material, section S1. We still prefer to keep the model name brand in the supplemental section but based on this comment, we are now more explicit about performance metrics in the main section by adding at the end of 1st paragraph Section 2.2: “Given that MEMs accelerometer performance in current model smartphones exceed performance metrics from those that had been previously tested in the field and on a shake-table [*Minson et al.*, 2015; *Kong et al.*, 2016], our criteria for phone choice included in-country availability and cost (Supp. Mat).”

Additionally we modified the the Supplemental Material document, so section S1 now reads: “The ASTUTI network uses Samsung SM-J415G smartphones purchased in Costa Rica for ~$US 120 per unit. This phone model utilizes a 16 bit MEMs accelerometer and as such has acceleration resolution equivalent to or better than all of the 12 phone models tested on a shake-table by Kong et al. (2016). We refer the reader to Kong et al. (2016) and other smartphone EEW publications for examples of smartphones’ noise, resolution, and capabilities for sensing specific earthquakes [*Minson et al.*, 2015; *Finazzi*, 2016; *Kong et al.*, 2016; *Finazzi and Fassò*, 2017; *Kong et al.*, 2018; *Kong et al.*, 2019; *Finazzi*, 2020; *Kong et al.*, 2020]”

And I partly-disagree with the authors' view about "Smartphones used in a fixed network can provide earthquake early warning performance similar to scientific-grade instrumentation". The author indeed shows the reliablity of this smartphone-based EEW system, but it will be more convincing if the author could provide the lower limit magnitude of this system.

In response to this comment, we have added at the end of section 4.3 the following text: “These outcomes combined with the smallest detected event having M_w 4.8 (Table 1) suggest that the lower magnitude threshold for ASTUTI is currently somewhere between M_w 4 and 5. This is not, however, an absolute threshold as it is a function of station spacing and site specific ground-motion variability that requires a much longer time period of operation to accurately assess.”

Reviewer #2 Evaluations:
Recommendation: Return to author for major revisions
Significant: Yes, the paper is a significant contribution and worthy of prompt publication.
Supported: Yes
Referencing: Yes
Quality: The organization of the manuscript and presentation of the data and results need some improvement.
Data: Yes
Accurate Key Points: Yes

Review for AGU Advances Smartphone-based Earthquake Early Warning with a Fixed Network: ASTUTI Costa Rica Brooks, B. et al. I consider that the paper submitted by Brooks and his co-authors constitutes a novel and interesting idea that will be of interest to a broad range of readers in the Earth and social sciences communities. As the authors stress, and I fully agree with them, this technology, once perfected and more developed, may represent an interesting alternative to the deployment of Earthquake Early Warning Systems (EEWS) in countries that have neither the funding nor the communications infrastructure to install and operate an EEWS with more sophisticated instrumentation. I believe the paper is very interesting and innovative and I would recommend its publication in AGU Advances. I would encourage the authors to consider some of the comments written below, as well as those that I added as corrections to the Word document attached to this review. I used this Word document because it was easier than to comment on the tightly spaced pdf pre-print available. Also, it has comments included by other reviewers

(?). I hope that this marked document makes it easier for the authors to produce a revised manuscript. Other comments are listed below:

1. The paper is extremely long. At times, it seems like the authors want to publish a review of EEWS. It is always important to provide the background, but the authors go to unnecessary lengths. I would encourage them to shorten some sections substantially, I elaborate below.

See below for responses.

2. The paper seems to be written in haste and not thoroughly checked before submitting. Some sentences are awkward, confusing or simply unnecessary; I point out many of them in the marked document.

We thank Dr. Suarez for his exceptionally detailed reading and excellent suggestions and copy-editing of the manuscript. We have gone over his marked document and addressed all of the suggested changes. We accepted, verbatim, the majority of them. The ones that we did accept were mostly based on stylistic preference. Responses to his numbered comments in the PDF version are summarized here, and responses to his detailed comments are given below the comments.

GS1: A Wikipedia ranking (https://en.wikipedia.org/wiki/List_of_countries_by_total_wealth) lists Mexico as in the upper 10% of wealthiest countries (19 out of 175). To make this statement slightly less strong we modified it to read *"generally wealthier countries"*

GS2: To clarify, we changed "coupled" to "*combined*"

GS3: To clarify, we modified the text to read: *"any earthquake, regardless of magnitude.*"

GS4: To clarify, we modified the text to read: "*phones continuously streamed data sampled at 10 Hz; subsequently we increased the sampling rate for the entire network to its current rate of 100 Hz*"

GS5: Based on earlier editorial suggestions to shorten the text, we preferred to leave out discussion of Japan's implementation of PLUM. To address this comment and to clarify, we modified the text to read: "*In a polygonal mesh of station locations, we compensate for relatively noisy smartphone accelerometers (compared to traditional seismometers) by requiring multiple neighboring stations to experience anomalous accelerations in order to trigger an alert*"

GS6: We have labeled the vertical axis on the figure to read "P" for "probability". We have also changed the caption for figure 6 to read: "***Figure 6****. Probability distribution function of PGA values for one station for one day (gray, N= 395682) and one hour (red, N=10150). The current triggering threshold is 0.6%g.*"

GS7: To clarify, we have added the following text to the Figure 7 caption: "See Supp. Mat, section S2 for how latency is calculated."

In fact, in the supplemental material we had stated how we calculated latency. In Section S2 it reads: “We calculate latency associated with the transmission, $t_{TRANSMISSION}$, from phone to processing center as $t_{TRANSMISSION} = t_{CENTER} - t_{QED}$, where $t_{ARCHIVE}$ is the POSIX time when the message arrives at the center.”

GS8: We believe the reviewer was referring to Figure 7b, not 7a (which was referenced in the preceding paragraph). To clarify, we made the reference only to Figure 7b. See also Reviewer #3’s final comment – to clarify even more we have made Figures 7c and 7d now Figures 8a and 8b and they are referenced later in the paragraph, and in the next paragraph, respectively.

GS9: We would prefer to leave the 6 lines of texts that the reviewer found unnecessary as they are. We believe these lines demonstrate the types of spurious signals that smartphones are susceptible to, and how increasing the number of stations to 4 in a triggering polygon reduces the probability of FAs in our implementation to acceptable levels.

3. An example of this impression that it was rapidly put together is that in many cases the authors cite some references in the text with the full name and last name of the authors and not simply by last name as is the norm. This certainly gives the impression that the paper was not proofed critically by the authors before submitting.

We apologize for the inconsistency in reference formatting. This was caused by an issue in EndNote that has now been corrected. We have gone through each reference and checked that it meets APA v6 format.

4. I agree with the authors that for a country the size of Costa Rica, an EEWS that warns for every earthquake detected may be acceptable. However, what the authors call the ¨boy cry wolf¨ syndrome may wear down the population in time. Having worked in collaboration with the Mexican EWS, I know that the public is very demanding and unforgiving with systems like this. In addition, the authors only touch lightly on the presence of intermediate-depth and crustal earthquakes in Costa Rica. They admit that ASTUTI may not work for them, but this is done only in passing. I would ask the authors to consider a future magnitude threshold and mention it at least in this initial paper.

In response to this comment we have reworked the pertinent section in the discussion to read: “As expected, a low detection threshold criterion combined with a country-wide alerting region also led to significant population percentages where an alert would be received when no shaking was experienced (TP-NS outcomes). TP-NS percentage could be reduced by modifying alerting to include shaking estimation from source-parameter characterization and/or by imposing a more conservative magnitude threshold; these could potentially reduce user dissatisfaction from receiving uneccesary alerts. We suggest, however, that the benefit of attempting a more refined warning may be outweighed by the added variability associated with EEW parameter estimation and ground-motion prediction [*Minson et al.*, 2018a; *Minson et al.*, 2019]. Furthermore, it is not clear what penalty there might be in terms of user engagement if an EEW system provides alerts without felt

shaking. We note that the general population may appreciate receiving an alert whenever an earthquake occured, even if no one felt it [*Nakayachi et al.*, 2019]. Indeed, we are at the very early stages of studying the nuanced relationships between EEW system performance and human sentiment. Given this evolving understanding, a clear high-priority must be studying and refining EEW pre- and post-event education and messaging [*McBride et al.*, 2020]. "

5. There are some lengthy discussions that could be summarized in Tables for the benefit of the authors. I found it difficult and boring to read, all the statistics regarding the delay times for the earthquakes tested. Also, the discussion towards the end of the paper of percentages of the population that felt shaking or not and its relation to the DYFI data is extremely dry. I would encourage the authors to consider putting the data on Tables and refer the readers to them.

In response to this comment, we have created a more complete Master Table 1 to which all statistical discussions are referenced. This includes MMI data as well as all warning outcomes for the 5 detected events. We have eliminated Table S1 as its information is now in Table 1. Additionally, we have gone through and thoroughly reviewed and significantly re-edited both Section 4. "System Performance" and Section 5. "Discussion" based on this comment and comment 6, below. In general, we eliminated superfluous text and condensed discussion of statistics/percentages to only the most pertinent values. We also moved all of the normalization statistics to Supp. Mat (section S5 "Cost Considerations").

6. Following on the last comment, the Discussion section is too long and repetitive of what is said previously in other parts of the paper. Also, it makes the Conclusions section seem unnecessary and gratuitous. The authors know well that many scientists today read carefully the abstract and conclusions of papers. The authors should give a critical review of how this section is written today and shorten it, emphasizing their findings and innovations.

In response to this comment we did a complete critical review of the discussion resulting in significant improvements. We cut entire paragraphs, rearranged some paragraphs, and eliminated much superfluous language (for instance, description of statistics percentages which we condensed into more thorough tabular form; see comment 5 above). The result, we believe, is a much more concise, impactful discussion section.

7. Like all EEWS, to become a useful tool for society, the public has to be educated in its use. I know this is no easy task. However, I believe the authors´ definition of Missed Alerts covers too broad a range of earthquakes. I think the authors have to be realistic as to where earthquakes need to occur for ASTUTI to be an effective and useful tool. Whenever they roll it out, this has to be made clear. I encourage them to think about it and include it in the paper.

We appreciate this comment, but we respectfully prefer to maintain our more conservative definition of Missed Alerts by including all felt earthquakes (even those out-of-network). To clarify, in section 4.2 we have added the text: Our MA definition permits a broad range of

earthquakes to be counted, even those with source locations outside of the network and/or country. For this initial assessment, we prefer this more conservative MA definition."

8. In terms of the False Alerts, the authors point out to power failures of the grid line; a common problem in many countries. Although the idea is to keep it as a low-cost system, an inexpensive power supply would solve this problem. It is not clear frome their discussion how many times this happened.; it would be useful to know. I cannot help but to suggest to the authors also reference the statistics of SASMEX in terms of FA: one false alert during its operational life.

In response to the power-cycling comment, in Section 4.3 we have added the text: "The power-cycling issue is such an infrequent occurrence that we do not believe any additional mitigation measures are required, though if it were to become more frequent, backup power aside from the phones' battery could be added."

In response to the SASMEX FA comment, we have included in section 4.3 the reference to 1 SASMEX FA in 26 years (Suarez et al, 2018).

Suárez, G.,Espinosa-Aranda, J., Cuéllar, A., Ibarrola, G., García, A., Zavala, M., Maldonado, S., & Islas, R. (2018). A dedicated seismic early warning network: The Mexican Seismic Alert System (SASMEX). *Seismological Research Letters, 89*(2A), 382-391.

9. Some figures are confusing and of low quality. For example, Figure 7a shows both the average ASTUTI latency for all events (or at least this is what I understand of it) together with a discussion on the performance of the system during the 27 July 2020 earthquake. Figure 7a is not referred to in the text and the reader, as I did, is forced to speculate what it means and what it represents. If Figures 7b, 7c and 7d refer only to the 27 July quake and 7a reflects the median latency for all quakes they should not be in the same Figure. This needs to be corrected.

We note that Figure 7a was, in fact, referred to in the text (line 7a) but we understand how this could have been missed with the track changes highlighted. We believe that the original language in the Figure 7 caption accurately describes that it is simply the data latency (as defined in the text), not latency related to any earthquakes. To clarify that Figure 7b is not an actual earthquake but a simulation of the 2012 M7.6 Nicoya event, we have eliminated the date from the title of the figure (we understand how the original title, with the date of the simulation could have been confusing) and in Figure 7 caption added the date in front of M7.6 Nicoya to clarify that it is a simulation of an event that occurred in 2012. We believe it is appropriate to include Figure 7a in the rest of the figure as the purpose of Figure 7 was to use assess the entire system's latency and the 200-plus vibration tests permitted assessing alerting and detection latency,

10. In Figure 8 what authors call copper population is confusing. There are too many color bars. The ones for DYFI are clear. The others, however, are confusing. Three color bars

have the title Pop. What do they mean? I would encourage the authors to improve the figure making it more didactic and clarifying the caption.

We would prefer to keep the three different colormaps as the figures demonstrates, with the three different colormaps, the percentages of the population that would have received three difference alerting outcomes (TP-S, TP-NS, NA). In order to further clarify the colormaps' meaning, we have added the following intro sentence to the figure caption: "ASTUTI results from the 5 detected earthquakes demonstrating percentages of the population who would have experienced three different alerting outcomes: True-postive (TP-NS), True-positive no-shaking (TP-NS), and No-alert (NA)." And we have more explicitly described the three different colormaps in the text of the caption: "Hot population colormap, percentage of the population ("Pop.") that experienced True-positive (TP) outcomes. Copper population colormap, percentage of the population that experienced True-positive no-shaking (TP-NS) outcomes. Gray population colormap, percentage of the population that experienced No-alert (NA) outcomes."

11. The main question to me is whether Amazon Web Service Notification will work when the system is implemented fully, and thousands of users need to be notified (maybe a few million). The authors touch on this in passing. I admit to being totally ignorant of this web service, however, having witnessed how pilot programs using cell phone notification worked well for a reduced number of users but failed dramatically when the number of users increased, I have a grave concern whether this solution will work. Needless to say, if you do not notify broadly and on timely, the system is futile, no matter how sophisticated it may be technically. Not understanding how this web notification works, I would encourage the authors to include tests that confirm that it is a usable alternative.

The AWS text message system is not our final, nor preferred messaging platform. In response to this comment, and to clarify, we added the text in the last paragraph of Section 3.2: "Moreover, for widespread roll-out, push notification [*Warren et al.*, 2014] is a more appropriate and lower-latency protocol than text messaging and development of this capability is occurring in the next phase of our work."

12. Finally, the question of why the authors, particularly the local ones, do not mention the option to use the excellent national strong motion network data is puzzling. Costa Rica has perhaps the best network of this nature in Latin America. I understand it is run by an independent organization but building a valuable system like this the question of whether it can be integrated and how to a future EEWS pops up naturally.

In response to this comment, and to clarify that the intent of this study is to focus on assessing only smartphone-based EEW, we included in the last paragraph of the Introduction section the following text that includes reference to the strong-motion network:
"In order to focus on assessing smartphone EEW capability, we do not include any other data from higher-grade seismic sensors [*Protti et al.*, 2014; *Moya-Fernández et al.*, 2020].

Gerardo Suárez Instituto de Geofísica UNAM

Reviewer #3 Evaluations:
Recommendation: Return to author for minor revisions
Significant: Yes, the paper is a significant contribution and worthy of prompt publication.
Supported: Yes
Referencing: Yes
Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.
Data: Yes
Accurate Key Points: Yes

Reviewer #3 (Formal Review for Authors (shown to authors)):

The authors are developing a low-cost observation network system over a wide area for earthquake early warnings using smartphones. In addition, the delay time is considered using a large number of seismic records, and it can be evaluated as a highly practical study.
Although it is not related to the essence, I think that it will be a better paper if the following points are corrected, so please consider the correction.

p3 LINE 56
It says "70% and 15% of the population", but according to the description in the text at LINE 494, it would be "15-75%".
Corrected

p.6 LINE 183
It seems to be an explanation of Figure 2a. Therefore, it is desirable to change the reference from Figure 2 to Figure 2a.
Corrected

p.10 LIME 363
The word "from" continues twice in a sentence.
Corrected

p.10 LINE 364
Figure 8d is duplicated, one should be deleted.
Corrected

p.11 LINE 405
DFYI is misspelled. DYFI is correct.
Corrected

p.16 LINE 633
The quotes contain unknown characters such as "% @".
Corrected

p.17 LINE 698,702
The quote contains unknown characters such as "% @".
Corrected

p.17 LINE 708
The quote contains unknown characters such as "0% @" and "% U".
Corrected
Also, the volume number is missing. (Risk Analysis, Vol. 39, No. 8, 2019.)
Corrected

p.18 LINE 736
The quote contains unknown characters such as "% @".
Corrected

p.18 LINE 751
The unknown character string "1." should be deleted.
Corrected

Figure 3
Labels (a),(b) are not written on the figure.
Corrected

Figure 6
The 1-hour histogram is 0.4 to 0.6 more frequently on the horizontal axis than the 1-day histogram. Is the vertical axis common? The vertical axis label should be shown.
Corrected, see also response to GS6, above.

Figure 7C
It seems that the terminal with a distance of 30.3km is shaking earlier than the terminal with a distance of 9.9km from the epicenter. I think it is necessary to explain this.
Also, it looks like there are three terminals that aren't shaking at all. Isn't it necessary to explain this?
In response to this comment, as well as Dr. Suarez's comments about the previous Figure 7, we decided to separate Figure 7 into 2 figures: new Figure 7 and new Figure 8. This is a natural division as new Figure 7 now is only the Data latency PDF figure and Figure 8 is the Nicoya earthquake vibration simulation figure. We eliminated the move-out plot from old Figure 7 (which was, admittedly confusing and not necessary) and now Figure 8 includes the map of the Nicoya simulation and the two histograms of the detection and alert latencies determined from the Nicoya simulation vibration tests. We have also, accordingly, changed old Figure 8 and 9 to new Figures 9 and 10 and changed all appropriate call-outs.

Table 1
I think that TP-NS is correct when combined with the description in the text.
Corrected