Reviewer #1

The manuscript titled as "Earthquake shaking intensity estimates (Vs30) of coastal cities in Eastern Indonesia" is an important paper for understanding the site conditions in the eastern part of Indonesia. The observed Vs30 maps would be useful for evaluating and predicting seismic ground motions for future earthquakes. Then some comments and suggestions for modifying the manuscript are shown below;

Title:

I feel the title is strange because Vs30s present the one of the proxies for site effects but do not directly indicate shaking intensity of earthquake ground motion. I suggest to modify the title as "Earthquake shaking intensity estimates based on Vs30 of coastal cities in Eastern Indonesia".

L32 in P1:

Please do not use the URL for the reference, and it should be referred as the authors (organization) and published year.

L102-121 in P5:

Please show the number of the MASW observations (although it was described only in the abstract).

Please show the distribution of the MASW observation sites.

L133 in P5:

The authors described that "PuSGen produced a table of MMI observations from the northern and central Lombok". I am wondering how the MMIs were observed in the earthquakes. Seismic instrumental observations or questionnaire survey? Although the work was done by the other researchers, the method of the MMI observations need to be briefly introduced in the manuscript.

L139 in P6:

The sentence of "using criteria published by the USGS (personal communication)" is strange. If the criteria were really published, the reference must be clearly referred by the published material not "personal communication".

L148-156 in P6:

Shear wave velocity profiles are required for calculating Vs30 at the observation sites. Please show the Vs profiles estimated by the MASW surveys in order to understand the characteristics of the site conditions, and the depths of the soft soils.

L148-156 in P6:

Please clearly describe the methodology of the interpolation technique for Vs30 mapping. I imagined that virtual Vs30s were given to elevated areas located outside of the coastal plain. For example, Vs30=741m/s values were given to the hill area shown in Fig. 2 and 3. But these approaches were not explained in the manuscript.

L356-383 in P17-18:

In this section, countermeasures for earthquake risk reduction is discussed. The authors suggested effectiveness of more ductile building materials such as bamboo for safer housing. However, the discussion is too simplistic, and leap of logic. The previous analysis and discussions were focused on the seismic intensities and site effects, but the effects of the ground motions and site conditions to the buildings were never discussed in the manuscript. The strategy for risk reduction and modification of building materials are very important issues for reducing building damage in future earthquakes. But the discussions described here are beyond the main scope of this study.

Overall:

I think the estimated Vs30 maps in this study would be more accurate than previous Vs30 maps because the authors directly observed the site conditions. However, readers would concern with the difference between the proposed and previous Vs30 maps. Could you show the comparison of the proposed Vs30 maps with the previous Vs30 maps introduced by UGGS global Vs30 maps (https://earthquake.usgs.gov/data/vs30/)?

Overall:

Generally, the seismic intensity such as MMI in earthquakes are determined not only site conditions but also magnitude of the source and distance from the source region. However, the MMI in the previous earthquakes were discussed mainly with the Vs30s. The discussions were not enough to understand the distribution of MMIs in the earthquakes. Besides, the MMI distributions would not be validation data for Vs30 mapping. The MMIs must be discussed not only with Vs30 maps but also with the earthquake magnitude and distance from the source to sites.

Reviewer #2

The study conducted field surveys (MASW) to measure Vs30 values, based on which to produce Vs30 and site class maps. These maps are then used to explain/interpret the ground-motion intensity from historical earthquakes. I read the manuscript with interest. It is well structured and easy to follow. I provide more comments below.

Major comments:

On novelty/rigor: The main problem with the manuscript in my view is that it lacks novelty for scientist, and the rigor for engineers. It seems to be a bit repetitive. I suggest the authors to focus on one point for more in-depth analysis.

On interpolation: it is very briefly mentioned that in the section 2.3. However, it lacks sufficient details for readers to understand or potentially reproduce this work. Is it just straight interpolation without the use of any background information, e.g., geology or topographic slope, as constraints? There are tons of papers on Vs30 mapping that are not referenced at all, e.g., Foster et al., (2019) and the references therein.

Minor comments:

Line 160: incomplete sentence.

Line 172: Figure 2 was first mentioned earlier in line 170 in which the brief description is more appropriate.

Line 403: did you mean the single-station mHVSR method? This method needs independent information as constraints since the solution to the inversion is non-unique. You will need to provide either thickness or shear-wave velocity of each layer as priors.

Paper Review: Earthquake Shaking Intensity Estimates (Vs30) of Coastal Cities in Eastern Indonesia

September 2024

1 Introduction

In this document, I present my review for the article "Earthquake Shaking Intensity Estimates (Vs30) of Coastal Cities in Eastern Indonesia". The study shows the summary of helpful and useful shear-wave velocity measurements using MASW. This dataset can aid in constraining and developing seismic hazard analysis for several small islands in Indonesia. I think the manuscript is a contribution to the field and should be published. Nevertheless, some features of this study should be addressed before publication.

My main concern is the over-interpretation of a poorly constrained interpolation scheme. The study adopts an interpolation scheme that, in most cases, is based on statistical extrapolation without a track of the spatial correlation or the physics of the data. Without a strong context to develop these interpolations, the interpolated values can totally misrepresent the analyzed process. The manuscript needs a revision of the spatial scope of the data, the interpolated domain that can be adopted, and its limits.

2 Major comments

2.1 Geologic framework

The authors focused on the characterization of near-surface materials, but in the geologic description, they refer to geologic units with dimensions of several orders of magnitude larger than the shallowest materials that matter to this study. I recommend aligning the description of the geologic framework with the tens of meters scale adopted in this study, linking the implications of the described geologic units with the performed geophysical tests. A well-constrained geologic framework can provide physical context to the interpolation/extrapolations performed. The authors can expand the current geologic framework by including also the type of quaternary sediments of the islands and how they interact with the geophysical tests performed.

2.2 MASW measurements' methodology

SECTION 2.1. The authors neglected to include passive analysis from the microtremor wave field as it was "too weak" to extract dispersion curves. It is a surprise for me, as the measurements were close to the sea, a strong ambient noise generator, I do not understand how the authors reached this strong statement.

According to my experience, using active seismic sources in this kind of test with a sledgehammer of 10 kg does not allow the generation of enough low-frequency energy in the wave field, thus hindering the identification of Rayleigh waves with large enough wavelengths to explore depths beyond 20 to 25 m. This effect may be even stronger in soft sites. Usually, for exploring depths around 30 m, a more potent seismic source is adopted, or the dispersion curve from the active source analysis is combined with dispersion curves from ambient noise. Also, if passive wave field analysis is performed, using two-dimensional arrays when analyzing the microtremor wave field is a good practice, as this avoids azimuthal biases introduced by predominant directions with stronger energy in the wave propagation.

Thus, I ask the authors to explain why they stated that the microtremor wave field is too weak, so they neglected the passive analysis and how their setup of the active seismic source can provide reliable V_S results in the first 30 m depth.

2.3 Distance scaling of MMI

The authors refer to seismic risk, adopting MMI as the only parametrization for assessing ground motions. Understanding the lack of data, this approach is reasonable. Nevertheless, the authors analyze MMI values without considering the source-site distance. I recommend comparing the scaling of V_{S30} with the residuals of an intensity prediction equation, thus removing the distance scaling in the analysis.

2.4 Interpolation of the V_{S30} values

The authors interpolate measurements performed in a domain of a few kilometers to tens of kilometers, which, without a geologic and/or statistical framework, is inappropriate. I recommend linking the geologic and topographic framework to the measurements and then evaluating the most appropriate scale and boundaries of the extrapolation. For instance, by adopting the current interpolation scheme, the authors defines that most of the inner region of the island in Figure 6 has a V_{S30} around 600 m/s, but there are strong slopes in the center of the island, probably due to a volcano, leading to a likely underestimation of V_{S30}.

2.5 The two paragraphs from line 362 to 383

Please consider rewriting these two paragraphs, as it is unclear how they fit into this geophysical and seismological study. Additionally, they are poorly written, with some sentences not being technical writing or not appropriate for a scientific journal. For example, "In Indonesia, the affordability of bamboo and its widespread use in pre-colonial buildings has given it the cultural connotation of being a construction material used only by the poor."

2.6 Paragraph from line 401 to 406

While I agree that including HVSR in the analysis is useful, the authors never mentioned HVSR in the manuscript and just added that at the end of the conclusions. How does a manuscript focused on V_{S30} measurements and never referred to HVSR include that in the conclusions? Given this context, this is not the place to have this paragraph.

3 Minors comments

Lines 47 to 49: "Analysis of borehole and strong-motion data associated with the 1989 Loma Prieta earthquake found that mean peak horizontal acceleration, velocity, and displacement are inversely correlated with mean shear wave velocity (Borcherdt and Glassmoyer, 1992; Borcherdt, 1994)."

I recommend revising this sentence, as softer sites sheared at high amplitudes exhibit deamplification of the high-frequency part of the wave field compared to stiffer sites. There is a classical figure in the Kramer's book (Kramer 1996) conceptually comparing PGA on rock versus PGA on sediments for different types of materials that highlights this fact.

Please use subscripts in V_{S30}.