This paper contains a very interesting analysis and you have obtained some impressive results. Here are a few comments / suggestions that I hope might be useful.

There were a few things I was confused about regarding the mathematical description of the algorithm that perhaps you could clarify:

Thank you for your valuable comments. Our responses are summarized below.

In forming the covariance matrix (9), I guess you are assuming that the columns of X have already had their means subtracted?

As the reviewer pointed out, it is sometimes better to subtract the means from the columns of the data matrix. However, we did not subtract the means this time, because there was no difference in the cases with consideration of subtracting the means and without it. Since the point should have been clarified, we would like to mention it in the revised manuscript.

In line 121, j=1,...,n should be j=1,...,N I think? and also in the summation in the denominator of the equation on line 126 it should be N not n.

Because the number of the rows of the data matrix $\boldsymbol{X}$ is n, the size of the covariance matrix is $n \times n$ as shown in Equation (9). This means n eigenvalues can be obtained. Thus, the expression, $j = 1, ...n$, is correct.

In line 138 where you say C and C' have common eigenvalues, I think in general they have a different number of eigenvalues, but any excess ones are all zero. You never make it clear how many rows the matrix X has in this section, i.e. the number of observations, but I guess this is greater than N in general so that C is a larger matrix than C'?

As the reviewer pointed out, C and C' have a different number of eigenvalues. We should have stated that they have common non-zero eigenvalues. We would like to revise that way.

Regarding the sizes of C and C', as you mentioned, C is generally larger than C' since the length of data vector n is generally larger than the number of scenarios N. Because the meaning of the row of the matrix X was not clear in the manuscript, we would like to add an explanation in the revised manuscript.

In line 156 do you mean only the coefficients α_{ij} for j=1,..,r not for j=1,...,p ? I guess so from (16), but worth clarifying. So $f(\beta)$ in general is a vector of length r.

The reviewer's inference is correct. As stated after Equation (15), we extract only significant modes from n eivenvalues/vectors to construct the surrogate model, which can therefore be a sort of reduced order model. As the reviewer suggested, we should have clarified this truncation when we wrote Equation (16).

Let us add some appropriate sentences to explain the procedure.

In this paragraph why do you use subscript j rather than k as used in (15) and (16)?

As the reviewer points out, it is indeed confusing. To avoid unnecessary confusion, we have decided to replace $f_j(\boldsymbol{\beta})$ by $f_k(\boldsymbol{\beta})$ in Section 2.3 and accordingly revise some parts in this section. In response to this revision, we would like to revise α_{ij} in section 2.3 to α_{ik} because it is more appropriate.

In (17), I think the exp(...) after the second = sign should be $= \sum_{i=1}^{N} w_i \exp(...)$

The reviewer is absolutely right. Equation (17) in the original manuscript is incorrect as it lacks some symbols. We would like to fix it.

At first glance (18) seems to be a square N by N linear system so it seems no regression is needed, so maybe it's worth pointing out that it is really Nr by N since $f(\beta)$ has length r.

Equation (18) is a square N by N linear system. But, as stated below this equation, we used ridge regression (Hoerl and Kennard, 1970) to minimize the error of the surrogate models to prevent the overfitting problem. Nevertheless, the explanation seemed to be insufficient. We would like to add an explanatory equation and some additional explanations in the Subsection 2.3 to avoid confusion.

Lines 188-190 weren't clear to me. Maybe say the slip was varied from 0.7 to 1.4 times the original slip in the model of Figure 2. When I first read this I also thought you were varying the rake between -20 and +25 degrees, which would be wrong for a subduction event, so maybe also make it even clearer that these are the range of perturbations in the rake angle from the ones given by Fujii-Satake?

We determined the ranges of the fault parameters by following the calculation condition reported by Kotani et al. (2020). According to the paper, JSCE (Japan Society of Civil Engineers (2011, in Japanese) conducted calculations with different rake values and reported that ± 10 is a suitable rake range. But Kotani et al. (2020) changed the rake from -20 and +25 degrees to cover a more suitable range and check the effect of the variation of rake. Hence, the range of the rake used in our study may not be based on the real perturbations suggested in the Fujii-Satake model. Nevertheless, because the main objective of our study is to propose the instant prediction of tsunami forces, we think the wider range of the rake is not necessarily an irrelevant condition. We ask for the reviewer's understanding.

In Figure 5 the cyan lines for the Obs. are very hard to see, maybe make these lines black or red?

As the reviewer suggested, we would like to change colors of these lines to discriminate them.

Line 209, by "concrete connection method" I think you mean the method for coupling the 2D and 3D methods together, but this was confusing to me at first. Maybe say something like "To couple the 2D and 3D models together, the method used in the study of Takase et al (2016) was employed." (By "concrete" I think you mean the specific method employed here, and it might be better to use "specific" here and some other places. Since in English concrete is also a building material, and you are talking about forces on buildings, there could be some confusion.)

Certainly, "concrete" also means a building material and so may be not appropriate here as an adjective. We would like to replace "concrete" by "specific" as suggested by reviewer.

Line 231, discussing the 2D mesh used for evaluating the tsunami force: do you average (or sum?) the force over all vertical building faces that happen to lie in the 10m cell? It seems like this would vary a lot from cell to cell just based on the particular geometry of the buildings. In particular some cells might include no walls and hence have 0 force (?) while neighboring cells might have one wall or perhaps at the corner of a building a cell has two walls. So I'm surprised the plots of forces look as smooth as they do and perhaps you can say more about this?

The force is calculated by integrating the pressure acting on all the vertical faces of building within each evaluation cell. Therefore, as the reviewer pointed out, the tsunami force strongly depends on the surface area of buildings, and cells with no walls have zero force. Although we might have been able to employ other risk indices, such as average pressure, the force was considered to be the simplest to check the result. Nevertheless further insight into this aspect is left to future work.

It is great that you can get a surrogate model that reproduces the time evolution as well as it does, in addition to the spatial patterns. But I wonder if this is mainly because you are only considering perturbations to the Fujii-Satake model in which the basic spatial structure of the fault slip is always the same and so the time evolution shows similar sets of waves and arrival times, just somewhat varying magnitudes? The results are still impressive, but I wonder if you can comment on how this might be extended to developing a surrogate model that could be useful in real time for some earthquake that is not a small perturbation of 2011 (which the next big one almost certainly won't be).

As the reviewer pointed out, one of the reason why we could reproduce the time evolution using spatial modes would be that the basic spatial structure of the fault is always the same. Although the range of the rake is wider than the realistic one, as the reviewer suggested, we might be able to check the potential of the surrogate models by considering wider range of the slip. We determined the ranges of the fault parameters by following Kotani et al. (2020), we would like to consider the condition with larger perturbation. Basically, the surrogate models can work even under large perturbation if enough calculation cases are available. But, even in that case, we have to consider the effective parameter sampling. Because the discussion is very important, we would like to add some explanation in the manuscript. In addition, this remains to be explored in future studies.

To develop a surrogate model that would handle a greater variety of quakes, perhaps it would be necessary to give up on trying to model the full time-dependent solution and instead build a surrogate model that only attempts to predict the maximum inundation depth and force, which might be much easier to do and still very useful.

Yes, we can construct the surrogate model for the maximum value in the same manner and it is also very useful. In fact, although it is not shown in the manuscript, we have constructed it in preliminary studies. But if we can consider both the space and time as in the manuscript, the resulting surrogate model can contain various information and of course accommodates the distribution of the maximum value.

I don't understand some of the discussion in the paragraph just below Table 2 (lines 312-320). You say "the ratio of mean values was 434%...". I think a ratio should just be a number, not a percent. Do you mean the ratio was 4.34? And what mean values is this the ratio of? Is it the ratio of the error to the true result, i.e. the relative error? This isn't clear.

Maybe this would be clearer if you included also tables of the raw numbers you are comparing, it's not clear where these numbers come from or how they relate to Table 2.

In line 316-317 you give ratios like 0.78%. Again I'm not sure what you mean by a ratio as a percent, do you mean the ratio of error to true value is 0.0078?

As the reviewer pointed out, the explanation of the error indicated in Subsection 3.2.4 was not appropriate. It might be difficult to understand. We would like to modify the corresponding expressions and Table 2 so that potential reader can easily understand the results.

In spite of my questions and possible confusion, in general I like the paper and believe it should be published after some clarifications.

We appreciate the referee for taking time to review our manuscript with some flaws. Thanks to the reviewer's comments and suggestions, the quality of our paper would be improved significantly after adequate revision.