Review Comments for “Seasonal variations of the high-frequency site ground shaking” by A. Haendel, M. Pitz, L. C. Malatesta, D. Litwin and F. Cotton

This manuscript presents an appropriate study of the strong ground motions from the Japanese borehole arrays for the observational analysis of the spectral decay parameter. The study is well presented along with applicable and informative figures. Overall, I found this to be a well-produced manuscript and I offer only a technical clarification comments and suggestions. I would anticipate that the authors can successfully address these items to allow for the acceptance of the revised manuscript for publication.

Below I present my review comments, which are classified as either general comments (GC) or editorial comments (EC) with reference to specific places in the manuscript when applicable. I feel that the GC should be addressed, and or resolved and the EC comment can be taken as suggestions for the authors.

GC-01: A general comment, which I think would improve the manuscript, would be to discuss the need to have site specific or region specific data for forward application. This study is based on the evaluation of the widely available data from the Japanese borehole arrays, however, in other regions of the world where seismic hazard ground motion studies are performed, there is a significant lack of data. How would the authors proposed that this Japanese based study be applied to other regions for seismic ground motion studies?

GC-02: It would also be useful and related to the previous comment/suggestion on how this observation of the season variation could be applied in seismic hazard ground motion studies. For example, how would one apply this within the framework of a probabilistic seismic hazard analysis? Would one capture this observed seasonal variation as part of the general uncertainty in the PSHA? If the authors have thoughts on the forward application of their observations it would useful.

GC-03 (Line 150): The authors state that the observed change in the spectral decay parameters could be due to sensor changes. Why would a change in the sensor be reflected in a change in the spectral decay parameter? I would assume that the spectral decay parameter would be independent of the sensor unless there are high frequency limitations from one sensor to another. If this is a case, then I would expect that any recorded data in which the sensor is a limiting factor in the evaluation of the spectral decay parameter should not be included in the study.

EC-01 (Line 192): I would suggest replacing the letters “E.g.” with “For example” to start this sentence.

GC-04 (Supplemental Table S1): The grouping of the stations into the Honshu cluster and the Hokkaido cluster shows a significantly higher Vs30 values associated with the Honshu cluster than the Hokkaido cluster. Is there any potential impact on the observations and conclusions based on these differences in the Vs30 values?

For author and editor

This paper addresses a topic of interest to Seismica readers: temporal variation of the site component of the high-frequency spectral decay parameter kappa, k. The topic is timely and interesting because to the reviewer's knowledge, it has not been addressed before, yet it has the potential to impact seismic hazard analysis. The manuscript is well-written and has good quality figures. While elements of this manuscript are appropriate for publication in Seismica, additional work and/or data is needed to support the conclusions. The extent of the required changes may be greater than can be reasonably described as a major revision, which is why I recommend resubmitting for review.

Major concerns:

1. Stated conclusions are not supported by the data presented: The conclusions of this manuscript are not articulated well in part because the data and analyses conducted do not support more definitive statements and/or recommendations for improving current practices. As a result, the language in this and other important sections remains speculative. Lines 326-331 summarize the main findings but do not provide important context on the uncertainty behind some of those statements, for example:

"On the eastern side of Honshu, Δκ0 changes are more gradual and are likely to respond to the water content at depth, as shown by the good correlation with TWS". However, the authors also report the not insignificant uncertainty is present in the reported TWS values.

"In northeastern Hokkaido, Δκ0 values are well correlated with frost times and a permanent snow cover in winter. However, it is unclear whether the frozen surface layer or water changes in the subsurface cause the changes in Δκ0." The mechanism behind this observation is not well understood.

"At site YMTH07, Δκ0 behaves opposite to all other sites, but correlates well with soil moisture and snow depth."

The lack of a systematic observation of seasonal changes in Δκ0 values along with the lack of an explanation of the driving mechanisms behind it is a major concern and may require significant work and/or additional data to fully address.

1.1. Number of sites where "clear" seasonal variations were identified: The manuscript mentions multiple times that 188 sites were studied, yet the *Δκ0* values for these 188 sites are not provided in the electronic supplement or discussed in the manuscript. Lines 140-143 indicate that "The 13 selected sites shown in Fig. 1 are therefore not exhaustive, but are the sites (out of the total of 188 investigated sites) where we detect clear seasonal variations. All other sites either do not show seasonal variations or we are not able to detect them due to limited or clustered earthquakes or due to the selected frequency band." The definition of "clear" should be stated to understand the metric used to deemed these variations as such. Additionally, the distinction between "not showing seasonal variations" and data limitations is very important. Can you please provide the number of sites (with enough periodic, not clustered data) that did not show clear seasonal variations according to your scheme/metric? How many sites did not have the right type of data to

make an assessment? Why is 7% (13/188 study sites) of your complete database deemed as representative and/or enough to claim "clear" seasonal changes in *Δκ0*? Moreover, it is indicated in lines 345-346 that more research is needed to find out "why we observe strong seasonal variations at some stations but not at other nearby stations". Can you list the nearby stations where no clear seasonal variations were identified? What is your hypothesis for those observations? Unless I missed it, I do not think those were discussed in the manuscript.

1.2. Clear versus significant, systematic, and predictable seasonal variations: Beyond demonstrating the existence of clear seasonal variations in *Δκ0* values, relevant questions needed to shape the practice of site response or seismic hazard analysis is how significant, systematic and predictable those variations are. Because these aspects are not discussed in the manuscript, it is not possible to understand its impact on current practices.

1.3. Lines 343-344 state that: "a parameter like Δκ0 may be better suited to detecting seasonal changes in site effects, especially at high frequencies (than Vs)", but I do not think this statement can be supported or derived from the observations and analyses presented in this manuscript.

2. Title: The manuscript focuses on seasonal variations of *Δκ0* and its connection to high-frequency motion, so the title can be misleading. Moreover, the terms "high-frequency site ground shaking" seem confusing. Alternatives to consider include high-frequency motion at specific sites, or the site contributions to high-frequency motion.

3. The abstract claims that "our results indicate that local site conditions are influenced by environmental conditions and should not be assumed to be constant". While the manuscript provides multiple references of previous studies that show temporal variations in shear wave velocity, the focus of the analyses presented in this paper is *Δκ0*, which the authors do not describe as a site condition but rather "an integrative parameter of local site attenuation and amplification at high frequencies" (i.e., not a property or characteristic of the site, such as shear wave velocity, but a model parameter). This is emphasized in lines 195-197 ("We therefore treat Δκ0 in this study not as a pure attenuation parameter, but as a parameter describing the high-frequency behavior of the Fourier spectrum, which can include both, site attenuation and amplification). Hence, there is a disconnect in the narrative that must be reconciled.

4. Methods: What is the estimation error in the values of *Δκ0* used in this study? As correctly stated by the authors in the introduction, multiple factors contribute to within-station variability in *Δκ0.* By choosing a fixed frequency for the computation of k, the variability stemming from alternative frequency windows is neglected, but it exists. It will be important to compare this variability with the seasonal variability observed at some stations to start addressing the question of "is this seasonal variation significant" or does it fall within the larger within-station variability of the parameter? I also wonder if you could cluster all summers at one station and compute the within-station, within-season variability in *Δκ0*... how would that compare to the between-season variability or across seasons at a single station or cluster of stations

5. Quantified differences across seasons: Throughout the manuscript differences of 20 ms, 30 ms and 50 ms are indicated between summer and winter *Δκ0* values. Hese differences can become lower once you account for the estimation error in *Δκ0* Can you explain the impact on hazard estimates that you foresee because of these seasonal differences in *Δκ0*? Are they coupled with or independent from the seasonal variations in Vs that other studies have reported? Are these differences region-specific?

6. Negative Delta k0: multiple negative *Δκ0* values are shown in figures (e.g., Figures 3, 4, and 5) and in Table S1 for station KSRH09, but their meaning is not discussed. It would be helpful for unfamiliar readers if the authors could elaborate on the reasons behind these negative values. How many more negative *Δκ0* values did you obtain in your 188 study sites?

***7. Δκ0* as a measure of slope:** *Δκ0* is the difference between two slopes, so it is technically not a slope itself. Multiple instances in the conclusions section refer to *Δκ0* as a slope, and I do not think this is correct. Its value could be very similar to the slope of the transfer function (between surface and borehole sensors) at high frequencies for very specific conditions (and using the same frequency band for the calculations), but the match is not always 1 to 1.

8. Section 5 on the influence of Vs and Q: I am afraid I did not understand the purpose of section 5.1. Potential variations of Q with environmental forces are not discussed but should be. Also, equations 4 and 5 also required the assumption of a frequency-independent Q.

- Lines 195-197: Why don't the SBSR for the first cluster show seasonal variations (Figure 7a), yet we see those variations in *Δκ0* values (Figure 3a)? Regardless of the model adopted in Equations 4 and 5, the SBSR capture the effects of both attenuation and amplification. Why is that combined effect not showing a seasonal variation for the Honshu cluster (Figure 7a)? This observation is contradictory to the authors definition of Δκ0 (which was not computed with equation 5) in this paper (i.e., lines 195-197).
- Lines 199-201: Why would thermoelastic strain cause variations in Δκ0? What would be the underlying mechanism? Is the premise here that variations in Vs drive variations in Δκ0? If so, why?
- Lines 232-234: I am not convinced you can reach a conclusion here with the data available. It might be best to indicate that the cause for the observed patterns is still uncertain or poorly understood.
- See item 3 regarding making a distinction between physical parameters that can be used to characterize site conditions, and model parameters that can be used to characterized site response. How knowledge of seasonal variations in Vs can explain seasonal variations of *Δκ0* in Hokkaido stations (lines 240-248)? What are the common mechanisms if you state that *Δκ0* is associated with amplification and attenuation?

Minor comments:

Lines 35-37: Did you mean the opposite here (i.e., an increase in the HVSR peak frequency due to a velocity increase)? It is my understanding that all other things being equal (e.g., same thickness), a lower Vs will lead to elongation of the fundamental period, which means a lower peak frequency… Or maybe I am missing something.

Figure 2: Caption and y-axis should read $\Delta\kappa 0_i$

Figure 3: Edit caption and y-axis to reflect that modified means of $\Delta\kappa 0$ are shown instead of just the means. Because you are computing monthly means, you could also provide a measure of their dispersion to assess the significance of the temporal variation across different months.

Lines 170-172: can you please elaborate on the impacts of the Tohoku earthquake on TWS data?

Figure 7: Please explain why different months were considered as summer (e.g., just August, versus June to September).

Lines 310-311: “The choice of the frequency window for Δκ0 estimation then has a large impact on whether or not the value of Δκ0 will be positive in winter.” This touches on my previous comment on the within-station variability in Δκ0. It would be important to quantify before making conclusions.

Lines 217-218: At what depths were the abrupt variations in Vs reported?

Acknowledgments: Please include any sources of funding for this work. They appear to be missing at the moment.

Figure 3. Why do you think IBRH16 breaks the pattern around 2011?