



Dear Prof. Ben-Zion

Thank you for considering our minor revisions of "What Controls Variations in Aftershock Productivity?" [Paper #2019JB018111R]. We are grateful for the expert reviews of our submission and are confident that their insights have improved the quality, clarity and transparency of our manuscript.

The Associate Editor suggested restructuring the manuscript to more seamlessly incorporate the previous edits in the paper. The reviewer thought that the solution was to swap the current supplemental figures with the alternative declustering method for the current main text figures. As discussed below, we have found a better solution which involves explicitly explaining the three alternative aftershock counting results to show robustness to magnitude completeness and declustering method, with a new paragraph directly before the results section. We then systematically refer to each triplet of figures throughout the text in a consistent fashion and have carefully combed through the text to remove any discrepancies in ordering from earlier versions.

A secondary issue raised by one reviewer was the value of the SVM results. The reviewer had no technical qualms with this section, but was underwhelmed with its contribution to the overall study. However, since this is the only section that deals with the nonlinearity of the correlations, we retain it as an important part of the work and now explain this logic in the text.

Thank you for your consideration,

Kelian Dascher-Cousineau  
Emily Brodsky  
Thorne Lay  
Thomas Goebel

Associate Editor (Remarks to Author):

The revised paper satisfactorily addresses most of the comments of the initial reviews. The work should be suitable for publication after the authors try to restructure the paper according to the current comments by Reviewer #2. (See also a minor comment by Reviewer #1)

Reviewer #1 Evaluations:

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

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

The authors have satisfactorily addressed almost all my points. In reply to point 7) I raised in my previous report, there have been improvements to the supplemental material. However, the authors should explicitly cite the relevant sup mat sections in the main manuscript (e.g. L. 513ff).

*Done. We now structure the manuscript to systematically address supplemental material every time a result is presented and have added the following explanatory paragraph directly before the results section:*

*“For each major result figure we provide three versions: the preferred solution in the main text based on aftershock counts using the space-time windowing described above and a completeness threshold of  $M_w 4.5$ , solutions in Section S1.2 of the supplement based on aftershock counts using the same space-time windowing but with a more conservative completeness threshold of  $M_w 5.0$  and solutions in Section S1.3 of the supplement based on the Zaliapin declustering method with a completeness threshold of  $M_w 4.5$ . All three figures are referenced as each result is introduced for easy comparison. For the most part, the alternative methods are confirmatory and further commentary is only provided when conspicuous differences emerge. Given that the three treatments have varying intrinsic suppression of bias from background activity, consistency in the results strongly indicates that bias in  $N$  is negligible for our parameterizations.”*

Reviewer #2 Evaluations:

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

Reviewer #2 (Formal Review for Authors (shown to authors)):

Overall the authors have addressed almost all concerns in my first report properly. However, the way some of the changes have been implemented into the revised manuscript, is not very advantageous. In particular, important points have been included in the Supporting Information (SI), while in the main manuscript only text changes and references to the SI have been added. I suggest to change this at least in two cases:

1. The question whether or not to work with declustered data: The authors still present results based on data without declustering in the main manuscript. As a reason they mention that the background rate is probably low and most earthquakes are aftershocks. In the SI, calculations are repeated with declustered data leading to similar results. In my first report, I showed that the relative productivity becomes biased and returns misleading results in the presence of background seismicity. If I understand correctly from the Response-to-Reviewer letter, the methods used here, especially the definition of the relative productivity, are designed for aftershock data only, i.e. data without background activity. For this reason, I suggest to present the results for data with background activity removed in the main manuscript, because the assumptions for applying the relative productivity should be better fulfilled as for the study with the original data, even if the results are similar. The latter can be moved to the SI.

*The reviewer raises an important point which we considered carefully at various stages of this study, notably in this last round of revisions.*

*In the introduction, we add the following statement:*

*“In practice, the aftershocks are mingled with background events and a major challenge of any implementation is to accurately separate these signals in the face of regionally variable activity levels and detection thresholds. Previous efforts to isolate aftershocks have capitalized on the intrinsic clustering of earthquakes to suppress contamination and inferred that} the productivity law fits a wide range of data with  $\alpha \sim 1...$ ”*

*Applying a more sophisticated declustering routine has some merits but also some limitations. In some cases it will be able to distinguish background seismicity from clustered activity, but the*

*regional variability of seismic activity rates limits the confidence of background event identification (as is the case for any statistical procedure). We have found that for this dataset the background rate of seismicity of Mw4.5 earthquakes is extremely low and therefore any benefits of the more sophisticated declustering method that we have used are not significant, as shown in Supp. Mat. Sect. 1.3. Therefore, we have retained the more straightforward time-space windowing results in the main text, but have more systematically and seamlessly connected the supplemental material into the presentation of the main text (see response to Reviewer #1).*

*We have added Supplementary Section 1.4 that explores this issue in more depth. We show that the background activity is within one standard deviation of a Poisson counting error (Figure S27). The supplement text also compares the results to a shuffled catalogs as a further demonstration of the small effect of the background rate on the current aftershock counts.*

*The reviewer also expressed a concern that systematic trend would arise as a function of mainshock magnitude which may confound our results. We have shown that the relative productivity has no systematic trend with mainshock magnitude in Figure S1. We also have shown that the results are similar for smaller and larger mainshocks in figures S3-4.*

2. I still find that Sec. 3.5 is a little bit tacked on: The main manuscript provides only few information on SVMs. The new explanation is rather weird, e.g. what is an "embedded nonlinearity enabled by the transformation of the coordinate system via prescribed kernel functions"? Since Sims are quite complicated data processing techniques without physical assumptions originally designed for big data sets, the meaning of the results is not clear, i.e. do the results stem from the data or from the method? In my opinion, this section is not really valuable for the manuscript. It reads like: We put the data in a black box, get some results and find them interesting.

*We have edited this section to remove machine learning jargon and avoid any appearance of a black box. However, we have retained the SVM results as an important contribution of the study because as stated in the text:*

*"Key differences between SVM regression and a linear regression are 1) a tolerance for a margin of error, 2) a simultaneous minimization of model complexity and 3) non-linearity which arises from the kernel-transformations. These features make SVMs particularly well suited to highly heterogeneous, relatively small, multivariate data sets such as ours [Witten et al., 2011]."*

*The final reason is particularly important. The SVMs allow us to probe non-linear correlations which are not accessed by any other tool in the study.*

In sum, I suggest the authors to put more effort in merging the old manuscript together with the reviewers' comments into a homogeneous and logically organized paper, instead of moving all issues raised by the reviewers to the Supporting Information. Finally, there are several typos in the manuscript as well as in the Supporting Information.

*We have again edited the paper to improve the flow into a more homogeneous paper as suggested and fixed typos.*