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Comment

Interactive comment on “A framework for modeling clustering in natural hazard catastrophe risk management and the implications for re/insurance loss perspectives” by S. Khare et al.

S. Khare et al.

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Dear Editor:

In what follows, we provide responses to reviewers #2 and #1 (in that order). Based on the comments from the reviewers, a substantial update to the paper has been made. The updated manuscript has been uploaded with this response.

Note that as part of the response, we have included a Fig to illustrate the calibration process (only intended for the purpose of the review, and not included in the updated manuscript).

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Reply to Reviewer #2

Major comment #1:

Overall, we have made a considerable effort to make sure our updated paper is properly referenced. Specific to comment #1, we have included the Eastou and Tawn (2010) reference in the introduction as well as the conclusions. We hope that it is now clear that what we call the Poisson-Mixtures has indeed been studied/applied in other contexts, and we agree that full credit should be given. While we do discuss why the Poisson-Mixtures methodology is particularly suited to our intended application, in the conclusions, we now also reference the Villarini 2013 paper, to indicate to the reader that other alternatives to modelling clustered behavior exist in the literature. We also now refer to the Poisson process as a homogenous Poisson process (which is more standard terminology). Also note that based on the comments of reviewer #1, we have added a number of relevant references related to clustering. Overall, we now feel the manuscript is for the most part appropriately referenced.

Major comment #2:

We have made a considerable update to the Section 2.2. It was not our intention to emphasize the framework in Section 2.2 as the major novelty of the paper. Section 2.2 overall in the paper is now emphasized to a much lesser degree. Our idea in providing what we now call a conceptual framework for modelling clustering, is to present/discuss the notion of identifying groups of historical events which can be shown to be driven by a unique set of physical drivers. By identifying groups on historical data, we can use those groups as a basis for building stochastic simulation models. The idea of identifying groups of storms driven by unique physical drivers is now well established in the scientific literature, and we have updated Section 2.2 with a number of relevant references (Kossin, Camargo, Gaffney).

As suggested by the reviewer, we have dropped our 'super' terminology, and simply call the different groups Clusters (this seems more appropriate). As well, the reviewer

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makes a good point that in applying the conceptual framework to real problems, many uncertainties exist (length of the historical record, the details of the clustering framework, etc.). We now discuss this directly in the manuscript in Section 2.2.

We do feel that our discussion of this conceptual framework is valuable to readers of this paper because: 1. The idea of identifying groups is now a well-established scientific practice (as indicated by the provided references) and it ties in nicely to building clustered models of natural hazard phenomena 2. Despite the uncertainties (which we now point out in the manuscript), we feel this provides a nice conceptual starting point for building clustered models 3. This sets up nicely (we feel) our discussion of the Poisson-Mixtures methodology.

Major comment #3:

The reviewer makes good points with regards to the fact that we do not discuss in detail the Clustered model calibration process. We have made a significant update to Section 3 as a result of these comments. We would like to emphasize that the intended scope of this Section of the paper is to provide a simple and clear demonstration of the application of the Poisson-Mixtures methodology to develop a Clustered model of European windstorm historical data (which appears to exhibit strong clustering). The updated Section 3 now makes very clear the criterion we have used in developing the Clustered model: 1. The model should apply clustering to the more severe/intense events, consistent with our best understanding in the scientific literature (references due to Pinto, Mailier, Vitolo are now provided in the updated manuscript). 2. The model should generate over-dispersion statistics that are in line with the historical data (to a reasonable degree). 3. The model's implied occurrence statistics should fall within the range of uncertainty implied by the historical data we have used.

Note that Section 3 makes extensive use of occurrence exceedance probability statistics (OEP, OEP2, OEP3 and OEP4). We have updated the manuscript so that we feel it now states very clearly what these mean, and how they are derived. We provide the

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appropriate reference on order statistics (David and Nagaraja 2003), and also more formally define the OEP and OEP2 in an updated Appendix B (the OEP3 and OEP4 are logical extensions of this formal definition). We also provide appropriate references for quantifying the uncertainty bands around empirical/data-derived exceedance probability curves. We have tried to make sure our definitions are clear and precise in the updated manuscript, as these are not commonly looked at in the geosciences literature, but hope the definitions we provide is a useful starting point for readers not already familiar with these statistics.

Section 3 demonstrates clearly that a Poisson model does not provide an adequate model of the historical data (Poisson model has an over-dispersion of 1.0, the occurrence EP statistics implied by the Poisson model oftentimes fall outside the range of uncertainty implied by the historical data, and the return period of a significant historical year 1990 is greater than 5000, and we now state very clearly that it our opinion that this is too long for a year that has occurred in the historical record). We then build a Clustered model that matches our calibration criterion, and yields what we feel is a far superior model of the data (compared to the Poisson).

As suggested by the reviewer, other calibration criterion could have been used. However, we felt our calibration criterion was fit for purpose given our intention to provide a clear and illustrative example. So, we would like to keep this model calibration as is, and we feel this is a fair given that we now clearly state the criterion we use, and our intended purpose. Certainly, more statistically rigorous approaches could have been explored, but that goes well beyond our intended scope, but the reviewer does make some good suggestions.

Note that in trying to match the calibration criterion we set, we tried a number of model configurations (both in the SSI250 threshold, as well as the gamma variance). Along with this reply, we have provided a pdf file [New_Calibration.pdf](#), for the purposes of helping understand the procedure we went through in matching the calibration criterion. Essentially, we built a large ensemble of models and checked the over-dispersion, and

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OEP, OEP2, OEP3 and OEP4. We did so for a large search space in SSI250 and gamma variance. Some results from this process are illustrated in New Calibration.pdf for the benefit of the reviewers. In upper panel, we fix the gamma variance, and vary the threshold (the grey bands represent the uncertainty in the OEP implied by the historical data). In the lower panel, we fix the SSI250 threshold and vary the gamma variance. An SSI250 threshold of 2.5 and gamma variance of 1.5 was chosen. As stated in the updated manuscript, this matches our intended calibration criterion. Note that we checked our results once again, and found that the over-dispersion was 1.38 for the model, and not 1.39 as stated incorrectly in the original manuscript.

Major comment #4:

As discussed in our reply to major comment #3, we have updated the manuscript in a way that we feel the definitions of OEP, OEP2, OEP3, OEP4 are clear. Appendix B (which interested readers can look at), provides more formal definitions of the OEP and OEP2. We have reviewed the updated manuscript several times, and now feel the description is clear. Note that our simulations for the Poisson and Clustered models are based on 10^5 years (we felt that this was a sufficient number of years of simulation so that we could ignore having to quantify convergence errors). The historical data has 39 years.

Major comment #5:

We agree with the comments regarding our analysis of the NAO. As a result of this, we have dropped Figures 1-3 from the previous manuscript. The intended scope of our paper does not include trying to provide a detailed description of the physical drivers for European windstorm activity. As we show in the manuscript, the historical data we have chosen to use is indeed over-dispersive. Our view is that this over-dispersive behavior arises due to large-scale atmospheric patterns, and this is notion is well supported by the scientific literature. We now mention this point in our updated Section 5 (summary and conclusions) with appropriate references from (Mailier, Pinto).

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Major comment #6:

We have made a significant update to Section 4 to make the various definitions more clear to the reader. We recognize these contracts/definitions are not standard in the geosciences literature. As for our definitions of the (OEP), we have reviewed this Section 4 several times, and feel that the definitions of the contracts should now be clear. We have made every effort to use standard insurance industry terminology, and we hope our manuscript provides a useful introduction to these types of contracts for those not working directly within the insurance field. A number of the concepts we use in the paper draw on the Klugman reference which is a standard text book in the field of insurance mathematics.

Major comment #7:

We agree with the reviewer that the original manuscript that we submitted could be far more concise. Once again, we have made an effort to update the manuscript in a way that it is more concise. In particular with regards to our discussion of the Poisson-Mixtures methodology in Section 2.3, we have taken out the repetition that existed in the first version of the manuscript. Beyond simply stating the definition of the Poisson-Mixtures methodology, we do feel that it is important to state the associated properties of the methodology discussed in points 1-6 in the updated Section 2.3 (but repetition has been taken out). This is important because we want to make clear various aspects of this methodology, as applied very specifically to the natural hazard risk modelling problem. These properties of the Poisson-Mixtures methodology are not, in our opinion, well-known and are worth stating clearly, and casting into our context. The points we make are summarized as follows: 1. The method is analytically tractable in that we can write down the probability generating function (shown in Appendix A) 2. The model expression for the over-dispersion is important because it is a key statistic we use in model evaluation 3. The Poisson-Mixtures method gives cross-event correlation, unlike the Poisson model. 4. We can relax the assumption that different Cluster groups need to be independent, which is interesting for the reader 5. Makes clear that

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the over-dispersion can be ‘tuned’ through the gamma variance and 6. We can break down our cluster groups into a Poisson and Clustered part, and this can be helpful in model calibration. We feel that these are important points to make in this paper (to serve its intended scope). We have made every effort to make these points brief.

Note that we have dropped our point about the Poisson-Mixtures being different from a negative binomial (which we agree it is not). Our intended point was that this methodology, is fundamentally different from methodologies which treat events as behaving independently.

As suggested by the reviewer, we have taken out our point 5 (in the original manuscript) regarding the idea that this approach to modelling is ‘top-down’. This point is now brought up briefly in the conclusions, providing a reference to work done using general circulation models (which could be viewed as a more ‘bottom up’ approach, ie. not only using statistical modelling approaches).

Minor comments:

a. We have updated the title of the manuscript to be more precise, and something along the lines of what the reviewer suggests b. Section 3 now includes a definition (and web link) to the word CRESTA (this is an insurance industry jargon, and makes sense to define clearly) c. We agree with the reviewers point regarding the fact that a 10000 year event may have occurred in our short historical record. In reality we have no way of knowing. We have updated the manuscript to make clear that it is only our view/opinion that in the context of the example in Section 3, a model (the Poisson) which assigns very long return periods to years that we have observed in the historical record (50 years) seems suspicious. The Clustered model example we have provided in Section 3 provides what we feel a more reasonable return period to the year 1990. Again, we feel the updated manuscript makes it clear that this is simply the opinions of the authors. d. We have corrected our mixing of the red/green curves (in the new Figure 2, the empirical data is red, the clustered model is green). We apologize for this

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confusion (the first author is red/green color blind and made a mistake in interpreting the graphs). e. We thank the reviewer for the references (most of which are now referenced)

Reply to Reviewer #1

SC1: We agree that pointing out the issue of a limited data record is important. Part of the intended scope of this paper is to provide a simple methodology and demonstration of modelling clustered natural hazard phenomena. This is limited, in some sense, by the relatively short data record we have to work with. The updated manuscript recognizes this in several places including Section 2 which discusses the uncertainty in defining Cluster groups that arises due to a limited historical record. As well, the concluding Section 5, we discuss how the methodology we have presented is a ‘top-down’ approach which makes use of statistical models built upon limited data set, and that there is certainly a role for physically based numerical modelling to play in developing clustered models, and the updated manuscript now references the paper suggested by the reviewer (Pinto et al. 2013)

SC2: After considering both reviewers’ comments, we decided to remove our regression analysis of activity versus the NAO index. We never intended the scope of the paper to carefully analyze the physical drivers of clustering in European windstorms. The data we have used certainly does exhibit clustering, and we attribute this to the large-scale atmospheric patterns that have been extensively looked at in the literature. We emphasize this point in the concluding Section 5, referencing Mailer et al. 2006 and Pinto et al. 2009. By eliminating our analysis of the frequency versus NAO, we feel the paper is much cleaner, and aligned with our intended scope.

SC3: The updated manuscript covers much more clearly (in Section 3) how we went about calibrating the Clustered model of the data. In the response to reviewer #2, we provided a detailed answer regarding this calibration process (along with an additional plot in New_Calibration.pdf) which can be found in major comment #3. We refer re-

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viewer #1 to the major comment #3 we have prepared for reviewer #2 to answer the questions related to our calibration. Below, I copy major comment #3 (from reviewer #2) for the sake of clarity: “The reviewer makes good points with regards to the fact that we do not discuss in detail the Clustered model calibration process. We have made a significant update to Section 3 as a result of these comments. We would like to emphasize that the intended scope of this Section of the paper is to provide a simple and clear demonstration of the application of the Poisson-Mixtures methodology to develop a Clustered model of European windstorm historical data (which appears to exhibit strong clustering). The updated Section 3 now makes very clear the criterion we have used in developing the Clustered model: 1. The model should apply clustering to the more severe/intense events, consistent with our best understanding in the scientific literature (references due to Pinto, Mailier, Vitolo are now provided in the updated manuscript). 2. The model should generate over-dispersion statistics that are in line with the historical data (to a reasonable degree). 3. The model’s implied occurrence statistics should fall within the range of uncertainty implied by the historical data we have used.

Note that Section 3 makes extensive use of occurrence exceedance probability statistics (OEP, OEP2, OEP3 and OEP4). We have updated the manuscript so that we feel it now states very clearly what these mean, and how they are derived. We provide the appropriate reference on order statistics (David and Nagaraja 2003), and also more formally define the OEP and OEP2 in an updated Appendix B. We also an appropriate references for quantifying the uncertainty bands around empirical/data-derived exceedance probability curves. We have tried to make sure our definitions are clear and precise in the updated manuscript, as these are not commonly looked at in the geosciences literature, but hope the definitions we provide is a useful starting point for readers not already familiar with these statistics.

Section 3 demonstrates clearly that a Poisson model does not provide an adequate model of the historical data (Poisson model has an over-dispersion of 1.0, the occur-

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rence EP statistics implied by the Poisson model oftentimes fall outside the range of uncertainty implied by the historical data, and the return period of a significant historical year 1990 is greater than 5000, and we now state very clearly that it our opinion that this is too long for a year that has occurred in the historical record). We then build a Clustered model that matches our calibration criterion, and yields what we feel is a far superior model of the data (compared to the Poisson).

As suggested by the reviewer, other calibration criterion could have been used. However, we felt our calibration criterion was fit for purpose given our intention to provide a clear and illustrative example. So, we would like to keep this model calibration as is, and we feel this is a fair given that we now clearly state the criterion we use, and our intended purpose. Certainly, more statistically rigorous approaches could have been explored, but that goes well beyond our intended scope, but the reviewer does make some good suggestions.

Note that in trying to match the calibration criterion we set, we tried a number of model configuration (both in the SSI250 threshold, as well as the gamma variance). Along with this reply, we have provided a pdf file [New_Calibration.pdf](#), for the purposes of helping understand the procedure we went through in matching the calibration criterion. Essentially, we built a large ensemble of models and checked the over-dispersion, and OEP, OEP2, OEP3 and OEP4. We did so for a large search space in SSI250 and gamma variance. Some results from this process are illustrated in [New Calibration.pdf](#) for the benefit of the reviewer. In upper panel, we fix the gamma variance, and vary the threshold (the grey bands represent the uncertainty in the OEP implied by the historical data). In the lower panel, we fix the SSI250 threshold and vary the gamma variance. An SSI250 threshold of 2.5 and gamma variance of 1.5 was chosen. As stated in the updated manuscript, this matches our intended calibration criterion. Note that we checked our results once again, and found that the over-dispersion was 1.38 for the model, and not 1.39 as stated incorrectly in the original manuscript.”

SC4: Our rationale for analyzing annual contracts from Jan-Dec is that this is consistent

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with re-insurance contracts specifically for Europe. The reviewer correctly points out that if we had made a different definition of year (Sept-April for example) the statistics and results may have been different. Our experience with these data suggests that you indeed get a higher over-dispersion if you define the year from Sept-April (we had done this some time ago, but I cannot recall the exact over-dispersion number). By splitting up the year (arbitrarily) on Dec 31st, you lose some correlation (that is inherent to the winter season). However, we do not expect that using a different year definition would help illustrate the concept of calibration a clustered model using Poisson-Mixtures any better, and we do not expect our conclusions with regards to catXL contracts to change. As such, we would like to keep the results as they are, especially as the Jan-Dec year definition is most common type of setup. Also, we do not expect any fundamentally different conclusions related to the 'big years' concept that we have explored in the manuscript.

SC5: As we have pointed out to reviewer #2, the updated manuscript includes many new and necessary references regarding the physics of clustering. We thank reviewer #1 for pointing us to a number of relevant references, which we have referenced appropriately in the updated manuscript.

SC6: We agree that working on seasonal contract definitions would be of interest, but we feel the scope of the paper is already large enough, and we leave this to future work.

SC7: Section 2.1 has been made much briefer, and we have eliminated extensive discussion of the independence of Poisson processes (which we agree is common knowledge in the intended audience).

SC8: This sentence has been eliminated from the Section 2.2 which provides a revised discussion of our conceptual Cluster group framework for building Clustered natural hazard models.

SC9: The reference to 'top-down' in point 5 Section 2.3 has been eliminated. We

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believe that our intended scope is now made more clear in the introduction, and we have also emphasized this point in the conclusions and summary section 5.

SC10: The manuscript has been updated with a definition of CRESTA (which is insurance industry jargon)

SC11: Figures 1-3 from the original manuscript have been eliminated, which after reflecting on the reviewer feedback, was found to be outside the intended scope of this paper.

SC12: See comment for SC11

SC13: Having reviewed several times the text in Section 3, we would like to keep it as is, as it is nice to keep the formula general for all the order statistics from 1 . . . 39. The reviewer's suggestion is a good one, but we also feel that this point is ok as is.

SC14: This is a mistake in the original manuscript. In the revised manuscript's Section 3, we simply state that the Clustered provides a better fit to the empirical data at the short return periods (which is seen by looking at Figure 1 in the revised manuscript).

SC15: The reviewer is correct in pointing that out (the red curve indeed represents the historical data, the green curve represents the Clustered model results, the first author made a mistake).

SC16: Section 3 has been updated significantly, and we no longer make reference to Super-Clusters. Note that the calibration procedure has been addressed in SC4, so we refer the reviewer #1 back to our answer there for the details with regards to how the Clustered model was calibrated. We note that in Section 2.2, our intention is only to discuss a conceptual framework for grouping historical events into (what we now call) Cluster groups, and we feel this is a valuable contribution as the idea of using clustering algorithms is now a well accepted practice (references provided in Section 2.2 of the updated manuscript). The example in Section 3 is intended to provide a simple demonstration, and as such makes use of a set of 135 historical

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events spread over 1972-2010. For a simple demonstration, we did not feel that it was necessary to apply a clustering algorithm, and identify groups as such. Nonetheless, we would like to keep the conceptual framework in Section 2.2, as it provides a way of thinking that may be useful to people building clustered models. The review has a question with regards to our assumption on the rates of the historical events: Note that our Poisson model assumes a fixed mean annual rate of 135/39 (total number of storms divided by the number of years). As explained in Section 3, the severity (SSI250) distribution is obtained using a generalized pareto fit to the historical data. With regards to the threshold of 2.5, the reason for choosing this threshold is that this represents a relatively severe SSI250 threshold, satisfying one of our calibration criterion for clustering only the more severe events.

SC17: We agree with the reviewer's point, we cannot exclude the possibility that the actual observed year is very rare. We follow the reviewer's suggestion and now clearly state that in our opinion, models which assign very long return periods to years which have been observed in the historical record, are not as good a models which assign more reasonable return periods — it is now clear that this is our opinion. We know of no literature which addresses the return period of 1990 and 1999 specifically, and are therefore unable to provide a reference.

SC18: We have now reference the Pinto et al. 2013 paper in the conclusions, where we raise the possibility of using physically based numerical models (GCMs) to tackle the clustering problems.

SC19: We have revised Section 4 to make more clear the notion of contract prices. The updated manuscript makes clear (we believe) that: we study the impact of clustering on the mean loss and standard deviation of the loss. Contract prices are oftentimes a strong function of these two statistics. Therefore, by studying and understanding the changes in the mean and standard deviation, we are indirectly studying the impact on prices. Note that while we could have formulated our own formula for a price, we choose not to do so, as there is such a variety of methods used in the market, that it

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would be difficult to find a generally applicable price formula, whereas the mean and standard deviation are generally applicable.

SC20: No longer applicable due to the removal of figures 1-3. Note that we have checked the figure references carefully to avoid these types of mistakes again.

SC21: We have shorted the explanation of why clustering impacts the standard deviation considerably, as suggested by the reviewer. The updated manuscript avoids the repetition that was evident in the original manuscript version.

SC22: While the reviewer makes an interesting comment, it is not clear that this lack of generality arises due to the limited historical record. As alluded to in the revised manuscript, for lower layers (2-20) years RP, the maximum loss does not seem to be the highly dominant contributor to the loss to layer for small numbers of re-instatements, and therefore we do not see a reduction in annual aggregate loss standard deviation in this case. In the case of the 'higher layer' 20-50 years, the maximum loss dominates the annual aggregate loss, and hence a reduction in the standard deviation would be more likely, due to the reduction of the OEP that the clustering model imposes.

SC23: We agree, the revised Section 5 no longer discusses this point in the context of summarizing our work on the conceptual framework provided in this paper.

SC24: That is true. As discussed above, the intended scope of this paper as far as the numerical results are concerned is to provide a simple demonstration. This entailed using a relatively simple data set, which we felt was not appropriate to apply a Clustering algorithm. The discussion in Section 2.2 is provided as a conceptual framework only.

SC25: we have taken out this repetition in our revised version of Section 5.

SC26: EEF stands for event exceedance frequency and is now clearly defined in the Appendix D of the updated manuscript.

We thank the reviewer for the provided references, all of which are now referenced in

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the updated manuscript.

We have gone through the list of technical corrections suggested by reviewer #1, and corrected all of them that were relevant to the updated manuscript.

We thank both reviewer's #1 and #2 for their detailed reviews of our work.

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/2/C2816/2014/nhessd-2-C2816-2014-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 5247, 2014.

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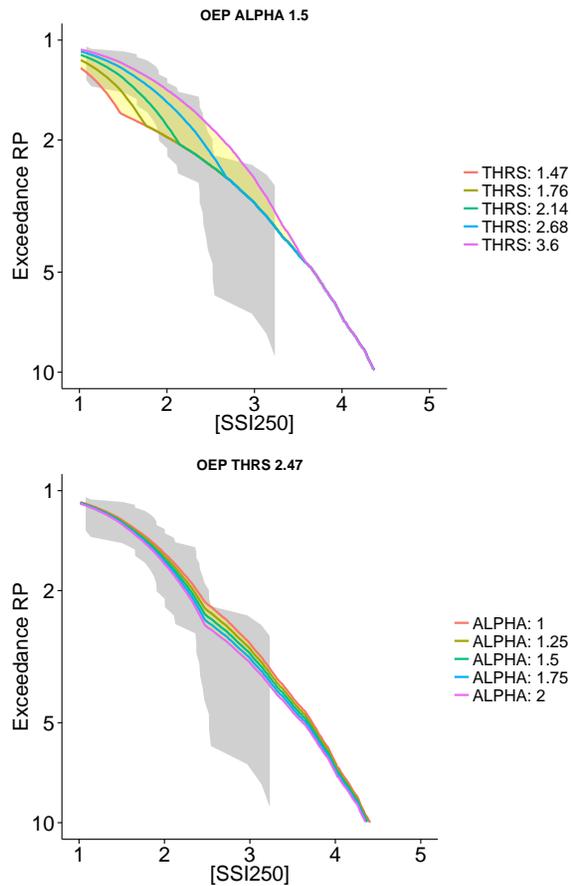


Fig. 1.

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