Review Report Williams 3008

Round 1

Reviewer A (Matt Tarling)

Completed: 2026-05-28 02:25 PM

Recommendation: Accept Submission

The manuscript "Fractal dimensions can lead to spurious interpretations of fault-rock character and slip physics" by Williams and Phillips assesses the reliability of fractal dimensions calculated from fault-rock particle-size distributions. They show that data-processing choices, namely binning, play an important role in the estimated fractal dimension on similar data, and that even for actual power-law distributions, that confidence bounds are wide enough to span pretty much the entire range used to describe fault rocks. On this basis they suggest that fractal dimensions might an unreliable metric to describe fault rocks (and the processes that form them).

This paper serves as a much-needed public service announcement to the fault and earthquake mechanics community. The value of this paper lies less in methodological novelty than in the clear message that we need to stop blindly fitting power laws and calculating D.

The paper is very well written and a true pleasure to read. I fully expect this work to become a quasi-mandatory citation in any future study of fault-rock particle-size distributions, whether or not the authors of that study choose to fit a power-law to their data. As such, the paper is a great fit for Seismica.

I have a couple minor comments, concerning the language used and, in places, how the novelty of the approach is framed:

The approach presented in the paper is definitely novel when it comes to grain-size distributions of fault-generated granular material, but other disciplines have (variably) long since determined the same conclusions as Williams and Phillips in their own field with regards to power laws and processing choices. I didn't know this for certain before writing the previous statement, but a 2 min google scholar search revealed several papers in other fields, for example this very well cited 2008 paper in Ecology :

White, E. P., Enquist, B. J., & Green, J. L. (2008). On estimating the exponent of power-law frequency distributions. Ecology, 89(4), 905-912.

The above is a cool but not unique example, because it plays a similar role as Williams & Phillips, but for tree size distribution: theory predicts an exponent, revision of the binning of a dataset that supposedly matches theory shows that the exponent is off and that the power law isn't a good fit anyways. Interesting that other disciplines have faced the same overenthusiasm for forcing power-laws and problematic calculations of the exponent, maybe worthwhile very briefly mentioning in the paper?

Based on this, I have to ask if coining the phrase "Clauset-style binning" and "Clauset-style approach" is necessary. It is a nice nod to Clauset et al.'s work, but the concept of just fitting a line to the cumulative distribution with one point per observation looks to be pretty old and widespread. This is now way outside my knowledge base, but the authors should consider looking at what this approach is called in the broader literature and go with that.

Ultimately, is coining language to describe a statistical process that is elsewhere and otherwise described a good idea? or will it isolate the literature from equivalent problems in other fields? For examples, if someone new to the problem starts looking for Clauset-style binning based on this paper, they won't get very far.

Minor comments and corrections:

The paper is well-written and grammatically sound. There is one slightly hilarious typo in Williams' affiliation: "*Seismolology*"

Best,

Matt Tarling

Reviewer B (Tom Blenkinsop)

Completed: 2026-05-27 11:26 AM

Recommendation: Revisions Required

This paper can be partly seen as a follow up to the important 2021 paper by the authors ("To D or not to D"). That paper demonstrated that power laws do not describe existing particle size distributions well, which are instead better described by log-normal distributions. This paper continues the theme by showing that much previous work that applied power laws is inherently flawed, by as simple a matter as the way in which the results are "binned", as well as uncertainties in the D values obtained through regression.

One justification for the paper is that counter arguments have been presented following the Williams and Phillips 2021 paper to justify the use of D values. This paper can be seen as a riposte to these developments, and points out flaws in data processing and linear regression in many previous studies.

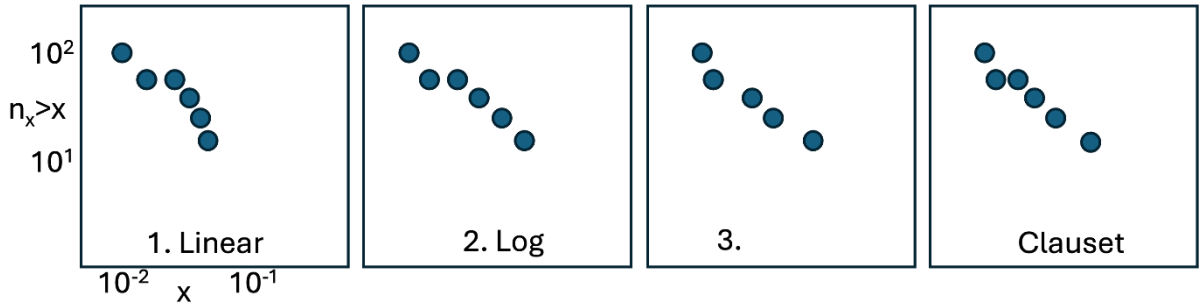
The arguments in the paper are compelling and economically presented. If they are taken seriously, D is likely to have died, and a lot of previous work has to be viewed with scepticism. This conclusion leads to a bit of a dead end. Here are three possible suggestions for making this paper more useful and positive:

1. List possible particle size distributions that could replace power laws in a table, and the mechanisms that generate them, if known.
2. Suggest whether the existing data can be used to test other mechanisms of cataclasis than constrained comminution, despite their limitations.
3. Surely there is also an important case to be made that the raw particle size data needs to be stored to avoid the processing difficulties noted in the paper. That would be a useful addition to the paper.

Detailed comments

Line 46. Misplaced ;)?

Lines 97 -105. Discussion of the binning problem misses out on a third approach. This is to plot $n_x > x_i$ at every value of x_i where $(n_x > x_i) < (n_{x-1} > x_{i-1})$. This allows for particles of equal size. Hypothetical distributions with some equal sizes are shown below:



This third approach is slightly different from the way in which the Clauset approach is described in the text, because it allows for equal sized particles. It is the approach taken in Blenkinsop (1991), which did not use the fixed increment method. In any case this section needs to be changed to introduce the Clauset method at this point and not later in the text. The Clauset method is not binning, and since it overcomes binning problems, it should be included in this section.

I'm not sure if the other papers cited may also have used this approach.

Work on the lower tail should reference Ord et al.

Line 151. Why "For completeness,…." This approach should be taken anyway.

Line 156. " using variably processed representations of data rather than the data themselves" As mentioned above, the Clauset approach could be said to use the data themselves.

Line 163. "Clauset-style binning". The Clauset approach does not involve binning.

Line 208. At least Blenkinsop 1991 did not use linear binning.

Line 318. After all

Conclusions

“It does seem likely, however, that we are neglecting important information (or perhaps even forming spurious conclusions) about fault-rock character and the physical processes that shape it by forcing data to conform to our preconceived notions of cataclastic deformation and a desire for expedient analysis.”

This sentence suggests further aspects of the paper that could be followed up. Surely it is valid to conceive of particle size distributions from theory or models or even experiments, and then to test natural samples for fits to these predictions? It would be a really useful addition to the paper to introduce some mechanisms and associated expected distributions. The sentence could also be used to point out the difference between the inductive approach that seems to be favoured by the authors, and deductive approaches that start with a model distribution from a theoretical model. Testing such models is not necessarily “forcing data to conform to our preconceived notions…”

Tom Blenkinsop

Authors' Response to Reviews of

Fractal dimensions can lead to spurious interpretations of fault-rock character and slip physics

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RC: ***Reviewers' Comment***, AR: Authors' Response

1. Reviewer A

RC: ***The approach presented in the paper is definitely novel when it comes to grain-size distributions of fault-generated granular material, but other disciplines have (variably) long since determined the same conclusions as Williams and Phillips in their own field with regards to power laws and processing choices. I didn't know this for certain before writing the previous statement, but a 2 min google scholar search revealed several papers in other fields, for example this very well cited 2008 paper in Ecology :***

White, E. P., Enquist, B. J., and Green, J. L. (2008). On estimating the exponent of power-law frequency distributions. Ecology, 89(4), 905-912.

The above is a cool but not unique example, because it plays a similar role as Williams and Phillips, but for tree size distribution: theory predicts an exponent, revision of the binning of a dataset that supposedly matches theory shows that the exponent is off and that the power law isn't a good fit anyways. Interesting that other disciplines have faced the same overenthusiasm for forcing power-laws and problematic calculations of the exponent, maybe worthwhile very briefly mentioning in the paper?

AR: We have noted this consilience in the main text and added the citation provided by the reviewer (see lines 153-155 in updated manuscript). It is indeed an interesting intersection between discplines. We note as well that the Clauset paper includes a wide array of datasets from various disciplines with similar outcomes.

RC: ***Based on this, I have to ask if coining the phrase "Clauset-style binning" and "Clauset-style approach" is necessary. It is a nice nod to Clauset et al.'s work, but the concept of just fitting a line to the cumulative distribution with one point per observation looks to be pretty old and widespread. This is now way outside my knowledge base, but the authors should consider looking at what this approach is called in the broader literature and go with that. Ultimately, is coining language to describe a statistical process that is elsewhere and otherwise described a good idea? or will it isolate the literature from equivalent problems in other fields? For examples, if someone new to the problem starts looking for Clauset-style binning based on this paper, they won't get very far.***

AR: We agree and have revised throughout to address this point, which is similar to additional comments made by Reviewer B. It was never actually our intention to coin a new phrase, as we agree that is just more jargon. The previous version was more a result of not knowing quite how to phrase the end-member approaches. We now refer to this approach as "unbinned" as suggested by Reviewer B.

RC: ***The paper is well-written and grammatically sound. There is one slightly hilarious typo in Williams' affiliation: "Seismolology"***

AR: Fixed. In the lead author's defense - that is what it feels like here sometimes...

2. Reviewer B

RC: ***The arguments in the paper are compelling and economically presented. If they are taken seriously, D is likely to have died, and a lot of previous work has to be viewed with scepticism. This conclusion leads to a bit of a dead end. Here are three possible suggestions for making this paper more useful and positive:***

- ***List possible particle size distributions that could replace power laws in a table, and the mechanisms that generate them, if known.***
- ***Suggest whether the existing data can be used to test other mechanisms of cataclasis than constrained comminution, despite their limitations.***
- ***Surely there is also an important case to be made that the raw particle size data needs to be stored to avoid the processing difficulties noted in the paper. That would be a useful addition to the paper.***

AR: We agree that this manuscript does not provide a new framework forward, but this is not the main purpose of the manuscript: the main purpose is to highlight that fractal dimensions are a poor descriptor of particle size distributions which should not be used to compare results across studies due to the large inherrent errors (which to our knowledge has never previously been discussed in the literature). In that way, we feel we are bringing an important issue to the community's attention, and thus providing incentive for additional examination. As such, we respectfully dispute the notion that the paper is not "positive".

As we suggest in our conclusions, it is the authors' opinion that perhaps we as a community have fundamentally misthought our approach to these data sets. We cannot reasonably solve that issue in this short contribution. Indeed, we do not know what the "answer" is. But not having a specific alternative to suggest does not seem sufficient grounds for failing to notify the community of a clear problem with a common methodology. So we do take the reviewer's point on this issue, but all we can say for now is that we are actively working on it. Noah Phillips has recently hired a new PhD student (starting this fall) who will be working with the authors on this topic, and hopefully providing a new framework for describing and comparing particle size distributions. We do feel, however, that the issues we highlight here are sufficiently important that they should be made public in the meantime.

We offer the following responses to the reviewer's bullet point suggestions:

- Point 1: This table already exists in Ord et al (2022) and we specifically cite that work as having proposed other distributions which may describe fault rock PSDs. We would rather not recreate such an effort here.
- Point 2: Respectfully, this paper is not actually about constrained comminution or its alternatives, though we do see how someone informed on the topic might make that link. Our introduction specifically frames this effort as being focused on whether the examined methodologies provide any utility as quantitative metrics describing fault-rock PSDs, *even if one recognizes that the underlaying data are not power law in form* (a power-law distribution being a specific prediction of the constrained comminution model). As stated above, our preference is to keep this contribution more streamlined by highlighting some serious flaws within a commonly utilized methodology (we count no less than 5

separate papers that have employed it since 2021). There will be more work conducted by an incoming PhD student who will be focused on exactly the types of question the reviewer is asking here.

- Point 3: Yes, we agree and have added a line in the discussion to highlight this point (line 264).

RC: ***Line 46. Misplaced ;)?***

AR: Fixed.

RC: ***Lines 97 -105. Discussion of the binning problem misses out on a third approach. This is to plot nx > xi at every value of xi where (nx > xi) < (nx-1 > xi-1). This allows for particles of equal size. Hypothetical distributions with some equal sizes are shown below. This third approach is slightly different from the way in which the Clauset approach is described in the text, because it allows for equal sized particles. It is the approach taken in Blenkinsop (1991), which did not use the fixed increment method. In any case this section needs to be changed to introduce the Clauset method at this point and not later in the text. The Clauset method is not binning, and since it overcomes binning problems, it should be included in this section.***

I'm not sure if the other papers cited may also have used this approach.

AR: We have reworked this paragraph in detail (see lines 97-113 in updated text). As described by the reviewer, several of the citations for linear binned data were, in fact, unbinned. We have reassigned these citations. We now refer to "Clauset style binning" as "unbinned" as recommended, and the description of this style of data processing has been moved to this section. The approach described by Blenkinsop and that we describe here as an example of "unbinned" data processing techniques are very similar outside of a few edge cases of particles that are numerically equal in size. As such, we limit our description to the original conditional (nx > xi at every value of xi), but also note that some methodologies depart slightly from that simplified approach in an effort to better account for equally-sized particles.

RC: ***Work on the lower tail should reference Ord et al.***

AR: Done.

RC: ***Line 151. Why "For completeness,…." This approach should be taken anyway.***

AR: Removed.

RC: ***Line 156. " using variably processed representations of data rather than the data themselves" As mentioned above, the Clauset approach could be said to use the data themselves.***

AR: We have revised this line for clarity.

RC: ***Line 163. "Clauset-style binning". The Clauset approach does not involve binning.***

AR: We now refer to "Clauset-style binning" as "unbinned" throughout the manuscript as recommended.

RC: ***Line 208. At least Blenkinsop 1991 did not use linear binning.***

AR: We no longer reference Blenkinsop as an example of linear binning. We have updated several of the other citations to better reflect the state of the literature as well.

RC: ***Line 318. After all***

AR: Fixed

RC: ***Conclusions: "It does seem likely, however, that we are neglecting important information (or perhaps even forming spurious conclusions) about fault-rock character and the physical processes that shape it by forcing data to conform to our preconceived notions of cataclastic deformation and a desire for expedient analysis."***

This sentence suggests further aspects of the paper that could be followed up. Surely it is valid to conceive of particle size distributions from theory or models or even experiments, and then to test natural samples for fits to these predictions? It would be a really useful addition to the paper to introduce some mechanisms and associated expected distributions. The sentence could also be used to point out the difference between the inductive approach that seems to be favoured by the authors, and deductive approaches that start with a model distribution from a theoretical model. Testing such models is not necessarily "forcing data to conform to our preconceived notions…"

AR: We have added a paragraph break after our line regarding "forcing data to conform to preconceived notions" in order to better separate it from the subsequent discussion of the more deductive approach the reviewer references here. This statement is in reference to the long standing community practice of extracting "fractal dimensions" from PSD data sets as a de facto matter of course. We feel the lines leading up to this statement make clear that we are providing commentary on a dominant methodology and its implications, not the merits of deductive vs inductive reasoning. To our knowledge, virtually none of the studies cited in this manuscript (with the lone exceptions of Williams and Phillips, 2021 and Ord et al., 2022) attempted to ascertain whether other models were a better (or even comparable) fit. In that way, we feel it is indeed true that the data have quite often been forced to conform with a preconceived notion, so we stand by that statement. That does not in itself imply a specific condemnation of the more deductive (i.e. starting theorizable distributions) approach the reviewer describes, in part because a scientifically-robust deductive approach would also test multiple competing models derived from theoretical considerations.

See comments above regarding proposing new mechanisms and/or expected distributions. As for the mechanisms of cataclasis, we are actively working on that problem and Phillips has a new PhD student starting soon who will be focused on it. As for expected distributions, it is true we have become skeptical of this notion in general, as alluded to in our conclusions and as the reviewer is correctly inferring in this comment. Whether or not an a priori focus on specific distributions is "valid" is something that the reader will ultimately have to decide for themselves. That approach has certainly been the norm for several decades, but as we have tried highlight here and in our previous paper, it does leave some things to be desired. We think it is reasonable for us to be able to propose that perhaps this approach is not the path forward.

We would rather not add specific commentary on the nature of inductive vs deductive reasoning. We certainly agree that is part of the question here when examined through a more philosophical lens, but we also feel that the commentary provided makes these two contrasting views of the problem clear using more general language. For the record, we did make some effort to implement this suggestion in a more explicit way, but the result consistently ended up feeling overly philosophical in a way that was inconsistent with the broader manuscript.

Round 2

Reviewer B (Tom Blenkinsop)

Completed: 2026-07-14 03:22 AM

Recommendation: Accept Submission

The authors have responded to the review thoroughly. Just a few typos, identified by line numbers in the clean version.

Lines 126, 127: Either “researchers… have” or “research….has”

Line 175: histograms not historgrams

Lines 213, 283: e.g. not e.g

Line 284. “which has found” should be “which have found”