

Dear Editor,

We were pleased with the positive comments from you and the reviewers, and we feel that the comments brought up valid concerns. We hope that we have addressed these concerns sufficiently in our revision. In order to fully address the comments, we have included a supplementary material document. We have also made some changes to the manuscript which were not specifically requested by the reviewers. Most of these changes were stylistic and do not change the content of the manuscript. One of the more substantial changes was that we merged Figures 7 and 14, which are both showing observed and predicted radial components of postseismic displacements, facilitating direct comparison of the figures.

We added supporting figures S3 and S4, which break down the predicted displacements of our preferred model into an elastic and viscoelastic component. We feel that these figures help support our discussion on lines 509 to 523, where we attribute inferences of fault slip and viscoelastic relaxation to different aspects of the observed postseismic displacements.

On line 500 to 502 of our previous version of this manuscript we make the claim that “After one year, afterslip is inferred to be deeper down on the Sierra Cucapah segment, which is describing much of the sustained near-field postseismic deformation.” This sentence is not accurate of the preferred model we present in the manuscript. As can be clearly seen in the new figures S3-S4, afterslip is describing some of the later near-field deformation, although most of it is being described by viscoelastic relaxation. We have reworded this passage and updated the introduction and conclusion to accurately describe our interpretations of the mechanisms driving later near-field deformation. This does not significantly alter the main conclusions of our manuscript, because we are very clear, both in the original and revised versions, that the deformation mechanism driving later near-field deformation cannot be definitively resolved with these data.

Each of the reviewer comments are included below followed by our response and a description of how these comments were addressed in this revised manuscript.

Reviewer #1:

Review of the paper : Revealing transient strain in geodetic data with Gaussian process regression

Overview of the work: The paper focuses on a new non-parametric Bayesian method to estimate transient strain rate from GNSS data. It is based on Gaussian Process regression. The method also includes an outlier removal processing step. This technique is then applied to the detection of slow slip events in Cascadia. The validity of the detected transient signals is accomplished via comparison with the seismic tremor recorded along the Cascadia range.

The article is well-written and your algorithm is very interesting. I think the use of Gaussian Process Regression is novel and open new roads in GNSS data analysis. I believe that you spend a lot of efforts in trying to justify most of the assumptions behind your model, together with the effects of using different kind of prior. Perhaps, that overshadows slightly your results. You could consider to reorganize the article to make it more readable for geophysicists.

I have rewritten the methods section to contain more examples and I elaborated on concepts that may not be familiar to a reader that does not have a strong background in stochastic processes. I sincerely hope that my revised draft is more readable to geophysicists.

I also require that a comparison between your outlier removal model and one other algorithm could help to support your claims about the efficiency of your method.

(See my response to the comment “Also, the paper would be improved if the proposed outlier detection ...”)

Otherwise, I have underlined a few clarifications needed about the stochastic model and some paragraphs within the text.

General Comments:

About the method:

The method is well established. The assumptions to justify the various hypothesis on the deter-

ministic models of the GNSS time series are generally well supported (seasonal signal, tectonic rate, offsets).

1- Now, the correlation of the stochastic processes is not completely clear. When the authors justify the model behind equation 3, w_{ij} is a normally distributed, uncorrelated noise. In statistic, it is equal to a WGN process. Then, the authors introduce a separate parameter (η) which models the temporally correlated noise. This parameter is also following a Gaussian distribution, with a zero-mean and a covariance matrix C_η (i.e. line 10 p5). This approach is not common in modelling GNSS time series. The standard is based on Williams 2003, where the author established a stochastic model with a covariance matrix as sum of identity matrix for the white noise and another matrix which represents the coloured noise. The coloured noise covariance matrix is defined differently (see the literature – Bock and Melgar Reviews of Geophysics 2016, He et al., Journal of geodynamics 2017).

I have changed my description of the data so that it includes a single term, η , that encompasses correlated and uncorrelated noise. I then break η down into correlated and uncorrelated components in Section 3.1 (eq. 25), where I go into detail about the noise model for the Pacific Northwest.

2- First, the author should give a summary of stochastic noise modelling in GNSS. I also think that a discussion is needed to relate to previous models. Why using separated stochastic models? Also I would comment on the possibility that the (low) spatial correlation between the parameters in your deterministic model (i.e. secular velocities) could be absorbed in the estimation of the covariance matrix C_η .

In Section 3.1, I acknowledge that noise models for GNSS data have been thoroughly researched and my FOGM noise model is not a new concept. I fear that a full summary of stochastic noise modelling would be too much of a distraction to the reader. However, I have referred the reader to He et al 2017 for such a review TODO.

We agree that the spatial correlation between parameters in the deterministic model can be incorporated into our noise model. We acknowledge this possibility in Section when we say “the secular velocities, b_i , are spatially correlated and we could invoke a tectonic model to form a prior on b_i . However, in our application to the Pacific Northwest, we will be using displacement time series which are long enough to sufficiently constrain b_i for each station, avoiding the need to incorporate a prior. Likewise, seasonal deformation is spatially correlated (Dong et al. 2002; Langbein 2008), and it may be worth exploring and exploiting such a correlation in a future study.”

Now, I am confused When looking at p.8 Section 4.1, the description of the noise models should be given as a subsection of Section 2 p.4. I would also improve the literature review. Several other models have also been discussed (i.e. Montillet et al., 2014 uses a fractional Brownian motion model; ARMA, ARFIMA, GGM or Band pass noise – see Het et al., 2017).

The paper is laid out so that Section 2 contains a general description of the method, and Section 3 contains details that are specific to each application. The choice of noise model is specific to the application, and so we feel that it should remain in Section 3. To help clarify any confusion caused by this layout, we say in Section 2, “The appropriate noise model may vary depending on the application, and we hold off on specifying the covariance matrix, C_{η_i} , until Section 3.1”.

We feel that a full review on stochastic noise models would be a distraction to the reader, but we refer the reader to He et al 2017 for such as review.

Outlier detection:

1- P7 : I would give a longer summary in the introduction about all the efforts in outlier detection in GNSS time series. The first paragraph p.7 is not enough.

In Section 2.2, we have expanded our discussion of previous efforts to detect outliers in GNSS data.

2- Also, the paper would be improved if the proposed outlier detection method is compared with existing ones such as the Hector software package (Bos et al., 2013). For example, the comparison between this method and another one could be done when applying the algorithm in p. 13 (last paragraph below equation 18).

My outlier detection algorithm is quite similar to what is used in Bos et al 2013 (and many other papers), in that the outliers are determined based on the residuals for a best fitting model. The only difference is that I am fitting the data with a stochastic model rather than a parametric model consisting of a linear trend and sinusoids. When using a parametric model, I found that deformation from slow slip events would sometimes be erroneously identified as an outlier. The stochastic model used in my outlier detection method is more

flexible and able to describe this transient deformation, and so only high frequency anomalous observations get identified as outliers. I have included Figure A1 to demonstrate this.

Of course, a parametric model that includes, for example, B-splines would be just as effective at detecting outliers without removing geophysical signal. So I am not suggesting that my outlier detection method is a significant advancement or even a novel method. Instead, I am only describing my outlier detection method for the sake of completeness. The previous version of my paper seems to have put too much emphasis on the outlier detection method, when the main focus should remain on the method for estimating transient strain rates. To help keep my paper focused, I placed the description of the outlier detection method in the appendix.

Results:

The results are generally well explained. However, the use of different priors and the comparison in the text is sometimes confusing. Perhaps, a table could summary the different priors and the main results. It would ease the reading of Section 4.2, 4.3 and then the discussion in Section 5.

In the previous draft, I was discussing two different priors: a prior for the outlier detection algorithm, and a prior used in computing transient strain rates. I acknowledge that this was convoluted. In the revised draft, all the details of the outlier detection algorithm are confined to the appendix. I hope this clarifies any confusion.

Minor issues:

P2 line 30 : “fidelity” replace with “reliability”.

Done

**P2 line 30-31 “ Developing and improving upon methods for deriving secular...area of research.”
Need references.**

p.2 line 43 :”too large of an area” ... perhaps “ a very large area”

Done

P5 line 32 : “formal data uncertainty”: can you define it?

By “formal data uncertainties” I meant “the derived uncertainties that accompany the GNSS displacement solutions”. The text has been clarified.

P 6 : Need reference to show Equation 15.

I have shown a few more steps in the derivation of eq. 15.

P 7 line 54-60: This paragraph can be further enhanced with references (Bock and Melgar, 2016, Gazeaux et al., 2013).

P 8 : first paragraph. add references on Cascadia range (Aguiar, Melbourne, Scriver 2009), (Melbourne and Szeliga 2005), (Melbourne and Webb, 2003, 2002).

P.8 line 20 delete www.unavco.org (only in Acknowledgment)

Done

p. 11 line 45 “common mode error” you need to add some references and define it properly.

p.13 : “Wendland functions have compact support and hence their corresponding covariance.. sparse”. I would just mention that the associated covariance matrices are sparse.

p.13 line 22 what is an “isotropic Gaussian process”? please add a definition.

P.20 : “ Using a compact covariance function ..” Again I would just say that the covariance matrix is sparse and that s why you have the mathematical simplification.

p.20 line 33 “ computational burden” I think that refers to p.20 line 60 “prohibitive when using sveral years of daily GNSS”. It would interesting to have some number. How do you quantify the computation time to process longer and longer time series?

Reviewer #2:

Comments to the Author(s) Hines/Hetland GJI Review 08/2017

This is a well written paper and I have just a few minor clarifications and suggestions below. One point that might be of interest to address is would it be possible to “approximate” this approach so that it can run much faster? I gather from the text that the method is intensive for CPU and memory use.

Page 3: Last line: $\min(t, t')$ needs as scaling factor to make units for covariance e.g., m^2/yr

Page 4: Stochastic model: It would seem that the assumption of no covariance between the north and east components is not a very good one since there is a tectonic framework in which the transients occur and its unlikely this framework aligns along the Cardinal directions. Maybe add a statement of impact of the neglect and possibly the idea of reorienting the “axes” to align with the tectonic framework e.g., perpendicular to the subduction zone interface in this case?

Page 4/5 Equation (3): Is it worth discussing at this point whether the estimates of u and n (eta) can be separated without explicit prior knowledge (which we may not have). For example, orbit modeling errors on one satellite (e.g., due to an unknown yaw problem in the spacecraft) will generate a spatially and temporally correlated error in d_{ij} . How does this (stochastically non-modeled) error not project in the u estimates?

Page 6: Equation 10: Maybe it is discussed later in the paper but it is probably worthwhile stating at this point how the inverse is performed with a block 0 in the matrix?

Page 6: Equation 12: Maybe some additional explanation is needed here. How are these partial derivatives formed. My understanding the u estimates at this point correspond one-to-one with the positions and times of the GNSS position determinations. Did I miss something?

Page 8: Partly addresses issue raised about eqn 3. It probably should be noted that wether conditions east of -121 deg longitude are different to the coast so there could be possible problems with this noise model.

Editor:

Editor’s Review of GJI MS 17.0593 “Revealing transient strain...” by Hines and Hetland This is an novel algorithm applied to a problem of great current interest. Based on the reviewer’s comments and my own reading, I am recommending moderate revision. The authors’ response should include a description of their changes, and also a version of the paper on which these can be seen (just as highlights in the text).

Many of the reviewers’ comments address places where the paper could be more clearly written, and my overall comment would be the same. The current version perhaps spends a little too much time on the background and not enough on explaining the method and its derivation. As shown by the references below (these include some the authors cite), and the Dermanis paper, which I have also attached, strain estimation from scattered data is an area in which there has been a lot more prior work than the authors mention. I am including these references not because the authors need to cite many or even most of them, but only to make the point that the problem of estimating strain, and of looking for transients, is a familiar one: for many readers, myself among them, much more familiar than the statistical methods applied here. So a slightly fuller discussion of these methods (perhaps a little more background and some reminders of, eg, what a hyperparameter is) would be welcome to many readers; and increased clarity of presentation usually means that the paper will be used and cited more.

I feel the same way about the section on detecting outliers: again, a well-worked area. Iterative fitting and outlier detection, as an overall strategy, have been used for a long time. I appreciate that by imposing spatial and temporal conditions you can identify outliers not otherwise obvious, but it would help to show an example of this, rather than the current Figure 4, in which most of the identified cases are ones that any method could easily find.

A few specifics (numbers are page+line):

2+26: possibility not risk: the latter is a technical term in seismic hazard.

2+47-48: I do not think this description of Shen’s method (which I’ve used and programmed) is right, since it allows the deformation gradients to vary in space. I’ve always thought of it as a kind of adaptive kernel smoother, somewhat like applying 2-D loess; so it is only dependent on nearby points, and has no assumption of uniformity anywhere. (Actually this raises a more

general question: is the method given here use local or global support?)

3+7: could also

3+32: redo the sentence; as it stands, you are saying that the SCEC exercise calculates strain rates.

3+37: a geophysical signal

3+47: function not signal

3+56: by a Gaussian, not with

4+37: displacement (not plural)

6+56: I suspect that equation (15) took some effort to derive; could some details go into an Appendix?

Figure 6: I like that the strain uncertainties can be shown, but is there a reason not to plot principle strains using arrows, showing errors by an ellipse around the tips?

Thank you for your continued
consideration,

Trever T. Hines