

Interactive comment on “Extracting small deformation beyond individual station precision from dense GNSS networks in France and Western Europe” by Christine Masson et al.

Anonymous Referee #1

Received and published: 23 June 2019

This paper presents a strain rate field model for the Greater France area estimated from GNSS-derived velocities. The inference of strain rates at or below the limit of the data precision is a challenging task and this is the reason why strain rate modeling for intraplate is difficult, at least at the spatial scales the authors aim at (~100-200 km). The main issue is that outlier velocities can cause large strain rate artifacts, because strain rates are spatial derivatives, causing noise to amplify. I commend the authors for tackling this problem and for trying to create a “robust” solution. This study approaches the problem through a combination of cluster analysis (which replaces observed velocities with the dominant velocity for a local cluster (or so I understand)) and Gaussian smoothing. The resulting velocities are then used to infer spatial gradients and define

C1

strain rates. The results look a bit puzzling with there being a number of significant elevated strain rate zones at places where they were unexpected (except for the Alps and Pyrenees). I am very concerned that the clustering approach, instead of having revealed systematic strain rate signals that were buried in the original data, has actually created signals that weren't there. The study also presents vertical rates but that analysis seems a bit disjointed from the rest of the paper.

I have many comments, listed below in descending order of approx. significance. Before I discuss those I want to point out that I was expected to see reference to the (Kreemer et al., 2018) paper. While the authors could in general do better with citing references (see comments below), that particular paper had the same aim as this study (to present a robust procedure to pull signal out of noisy data) but applied to intraplate North America. Perhaps the authors were unaware of that paper, and I strongly encourage them to check it out.

1) While I don't think I fully understand the clustering analysis, it seems to me that the resulting velocity field (Fig. 4a) is much more clustered than the original velocity field seen in Fig. 3a. My slight hesitation comes from the fact that a clear comparison is hard to make since the original data (Fig 2,3) is presented for a much larger geographic area (and a different scale) than the rest of the paper (see comment 8), which makes the observed velocities in the France area hard to see. Could the authors either reduce the area for Fig 2,3 or add a figure that shows observed velocities for the same geographic area as the other figures? In any case, Fig 4a is ultimately being used as input to the Gaussian smoothing, and it has various curious traits. Is it still in the same “France reference frame” as the data? If so, the fact that there is a dominant eastward component to most velocities suggests that the clustering changed the essential characteristic of the velocity field in this frame. How come? Obviously this velocity field is not in a new “France reference frame”, because then that eastward motion should not be there. While the reference frame of the velocity field ultimately doesn't matter (because the purpose is to investigate strain rates, which are reference frame inde-

C2

pendent (although see comment 5), this seemingly change in reference frame by the cluster analysis points to a possible problem with the clustering analysis. Secondly, from Fig 4a it is clear that the clustering analysis broke up the velocity field in domains and that there are rather discrete boundaries between these domains. Some of the main features in the strain rate field (Fig 7a) are directly related to these cluster boundaries; the NS zone in Aquitaine Basin, the NW trending zone in the Paris Basin, the NS zone in northernmost France, and the three related zones in Eastern France: the EW compressional zone in NE France, the EW extensional zone in eastern France and the NS compressional zone that connects them.

2) I'll leave it up to the authors to find out what may be wrong with the clustering analysis, but I have two immediate suggestions that may further exemplify problems with the clustering: a) show the (vectorial) difference between the original velocities and those obtained from clustering. Right now the authors only show the difference between the velocities from the clustering and those from the subsequent smoothing (Fig. 8) and they don't show them vectorially, which is important. b) derive a strain rate model from the Gaussian smoothing but based instead on the original horizontal velocities. I expect many differences. While the authors may argue that those differences point to the clustering pulling out spatially coherent strain rate signals, I would argue that the clustering seemingly creates signals that are inconsistent with the original data.

3) Because of the concerns expressed above, I have little confidence in the validity of the observed strain rate features and the discussion thereof (Section 4 and 5). Part of the discussion is the comparison with seismicity. The authors indeed find no or confusing correlation (which the authors call "surprising"). The relationship between intraplate deformation and seismicity is a hot science topic, and I am worried that that the general discussion on this topic does not benefit from comparisons being made on the basis of a strain rate model that has some serious problems. The authors also don't offer a good explanation for the various strain rate features in eastern France (except the Alps); I suggest this is because there isn't any good tectonic explanation and

C3

that these features are modeling artifacts.

4) The authors don't question their results because they have faith in their uncertainties which they derived from a synthetic test in which strain rate model was inferred when observed velocities were nominally set to zero (but velocity uncertainties were kept). This may be a good test, as it shows how data uncertainty and network geometry map into model uncertainties. The clustering approach may also work well when velocities are set to zero, as it would be hard to make clusters out of such data. The clustering may however fail when it starts to determine median velocities from actual velocities.

5) I am quite confused by the strain rate estimation as part the Gaussian smoothing. Here are the reasons: a) it appears this is done on a flat-Earth approximation, which is may be ok, but given that this study tries to infer very small strain rates it is worth investigating what magnitude of error a flat-Earth approximation would introduce over a fully spherical treatment. b) equation (3) is quite similar to equation B2 of (Mazzotti et al., 2011 which they reference, but some curious differences exist: in the current study the azimuthal weighting function is missing (why?), and the velocity in the latter half of both components is here given as that of a station and in Mazzotti et al as that of the grid point (the latter appears correct). c) In general, I am puzzled how the strain rate field is parameterized as being the product of distance and velocity, because strain rate is ultimately related to velocity divided by distance. This explanation was missing in Mazzotti et al as well and I would suggest deriving and/or explaining this better. d) the velocities contain a translation/rotation (which is particularly a problem in light of the reference frame problem discussed in comment 1). The way it reads now is that any rotation gets mapped into strain rate. Please clarify. e) Is there any fundamental difference between this method and the VISR method of (Shen et al., 2015) or even the SSPX method of (Cardozo & Allmendinger, 2009)?

6) For the outlier detection, some questions came up: a) it is not mentioned, but are the detected outliers the red vectors in Fig 3a? b) How many of the added campaign velocities are identified as outliers? It seems like a lot. Is it still worth including those?

C4

c) I don't understand line 22-23 (page 6) "Stations for which DM is greater than the network 95% confidence interval are considered as outliers and rejected". Does this mean that the outlier detection is based on distance as well? Why? d) Note that Kreemer et al. (2018) also introduced an algorithm to identify outliers. e) To test the "robustness" of the presented strain rate model one would need to show that the model is not affected by outlier data (if that is what was indeed meant with the model being "robust"). I understand why they flagged outliers, but to prove robustness they should also show a model that was based on data that included outlier velocities. Ideally the resulting model would be mostly the same.

7) the vertical velocities are also subjected to the clustering analysis and subsequent smoothing. The results are only sporadically mentioned in the discussion, which makes one wonder about that part of the presented data in light of this study's goals. There have been other recent attempts to obtain a "smooth" vertical velocity field (either as a continuous grid and/or by "despeckling" the original rates, as is done here). Examples are: (Hammond et al., 2016; Husson et al., 2018; Serpelloni et al., 2013) The authors should consider discussing and/or comparing the various approaches.

8) It is not clear why the authors present data over an area much larger than the ultimate study area. The study and most figures are focused on greater France but Figs. 2 and 3 show a much larger area, which notably includes a lot of data in Italy. Why is this presented if it isn't used? Does the number of stations mentioned in the text include those in Italy? If yes, I would find that misleading. While I understand that the authors would want to add a little buffer to the area show in most figures, the current presentation is confusing and doesn't allow for a good comparison between data and model in the actual study area.

9) In the introduction (line 21-22) of page 1, some studies of intraplate strain rate are mentioned (Canada, India). It would be better if the mentioned studies would be previous attempts to model intraplate strain rates in the same area, which are currently not even mentioned, particularly (Tesauro et al., 2006).

C5

10) Abstract, first sentence. Authors say "we use dense geodetic networks and large GPS datasets". What is the distinction between these two? They seem the same.

11) The paper uses the word "technics" twice. Ironically, the English language uses the French word: "techniques". Please correct.

12) The details on the GPS data analysis do not mention the minimum duration of the considered time-series. Is it 2.5 years? If not, what is it? If less, why?

13) page 3, line 11: "only a small percentage of stations is associated with reliable equipment logs". I suppose this hinges on the word "reliable" but I would have thought that the majority of the stations would have logs.

14) page 3, line 15-16. Here the bias is mentioned of undetected jumps on velocities (I think) but only for long time-series (>8 years). How about short(er) time-spans?

15) Was the common-mode also removed from the campaign data. It should be, but wasn't explicitly mentioned

16) page 7. the authors say that a spatial scale of 100-200 km corresponds to the interseismic deformation on a (vertical?) fault with a seismogenic thickness of 10-25. Of course, that would totally depend on the slip rate (and the precision in the data), so I think it would be better to omit this statement.

17) what are the orange colored points in Fig 4a and Fig 6a?

18) With the chance of sounding like a curmudgeon; the last author's contribution is solely in the realm of GPS data processing. Does that warrant authorship? (Note that this comment is not affecting my assessment of this paper)

Response to formal review criteria:

Scientific significance: Fair Scientific Quality: Poor Presentation Quality: Fair

+ Does the paper address relevant scientific questions within the scope of SE? yes

C6

- Does the paper present novel concepts, ideas, tools, or data? yes
- â€” Are substantial conclusions reached? No
- â€” Are the scientific methods and assumptions valid and clearly outlined? No
- â€” Are the results sufficient to support the interpretations and conclusions? No
- â€” Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No
- â€” Do the authors give proper credit to related work and clearly indicate their own new/original contribution? No
- â€” Does the title clearly reflect the contents of the paper? Yes
- â€” Does the abstract provide a concise and complete summary? Yes
- â€” Is the overall presentation well structured and clear? Yes/No
- â€” Is the language fluent and precise? Yes/No
- â€” Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes/No
- â€” Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No
- â€” Are the number and quality of references appropriate? Yes/No
- â€” Is the amount and quality of supplementary material appropriate? Yes

Cited References:

Cardozo, N., & Allmendinger, R. W. (2009). SSPX: A Program to Compute Strain from Displacement/Velocity Data. *Comput. Geosci.*, 35(6), 1343–1357. <https://doi.org/10.1016/j.cageo.2008.05.008>

C7

Hammond, W. C., Blewitt, G., & Kreemer, C. (2016). GPS Imaging of vertical land motion in California and Nevada: Implications for Sierra Nevada uplift. *Journal of Geophysical Research: Solid Earth*, 121(10), 7681–7703. <https://doi.org/10.1002/2016JB013458>

Husson, L., Bodin, T., Spada, G., Choblet, G., & Kreemer, C. (2018). Bayesian surface reconstruction of geodetic uplift rates: Mapping the global fingerprint of Glacial Isostatic Adjustment. *Journal of Geodynamics*, 122, 25–40. <https://doi.org/10.1016/j.jog.2018.10.002>

Kreemer, C., Hammond, W. C., & Blewitt, G. (2018). A robust estimation of the 3-D intraplate deformation of the North American plate from GPS. *Journal of Geophysical Research-Solid Earth*, 123, 4388–4412. <https://doi.org/10.1029/2017JB015257>

Mazzotti, S., Leonard, L. J., Cassidy, J. F., Rogers, G. C., & Halchuk, S. (2011). Seismic hazard in western Canada from GPS strain rates versus earthquake catalog. *Journal of Geophysical Research*, 116(B12), B12310. <https://doi.org/10.1029/2011JB008213>

Serpelloni, E., Faccenna, C., Spada, G., Dong, D., & Williams, S. D. P. (2013). Vertical GPS ground motion rates in the Euro-Mediterranean region: New evidence of velocity gradients at different spatial scales along the Nubia-Eurasia plate boundary. *Journal of Geophysical Research: Solid Earth*, 118(11), 6003–6024. <https://doi.org/10.1002/2013JB010102>

Shen, Z.-K., Wang, M., Zeng, Y., & Wang, F. (2015). Optimal Interpolation of Spatially Discretized Geodetic Data. *Bulletin of the Seismological Society of America*, 105(4), 2117–2127. <https://doi.org/10.1785/0120140247>

Tesauro, M., Hollenstein, C., Egli, R., Geiger, A., & Kahle, H.-G. (2006). Analysis of central western Europe deformation using GPS and seismic data. *Journal of Geodynamics*, 42(4–5), 194–209. <https://doi.org/10.1016/j.jog.2006.08.001>

C8

