tektonika

Review Report

Métois et al., What Can GNSS Tell About Physical Processes in Slowly Deforming France? Insights From a Community Benchmark Exercise, TEKTONIKA, 2026.

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1st Round of Revisions

Decision Letter

13 February 2026

Dear Marianne and co-authors,

We have now received two reviews on your submitted manuscript. Both the reviewers, the associate editor and I agree that your paper will be an excellent contribution to Tektonika after moderate revision. I copy below the associate editor and reviewers' comments. Please address their comments and suggestions thoroughly, documenting all changes made.

We look forward to receive a revised version of your manuscript in less than two months. In case you think you would need more time don't hesitate to contact us. Please upload the revised version together with a rebuttal letter and a manuscript file with all the changes marked.

Best regards

Robin Lacassin, Tektonika executive editor

ASSOCIATE EDITOR:

I have received two reviews of the manuscript "What can GNSS tell about physical processes in slowly deforming France? Insights from a community benchmark exercise," by Métois et al. Both reviewers agree that the manuscript is an important contribution for evaluating and improving strain calculations, especially in low-strain-rate regions. I agree with both reviewers that moderate to minor revisions prior to publication are appropriate.

One criticism worth highlighting is the lack of definition of the theoretical strain rate model. Both reviewers were skeptical that such a model could be different from the "test players". It seems that a more thorough description of the theoretical strain values and a discussion of how those choices influence the results could improve the manuscript.

The reviews are pasted below.

Devin McPhillips, Tektonika associate editor

Comments by Reviewer 1 (Kathryn Materna)

This paper performs a synthetic analysis of strain rate recovery techniques in very-low-strain-rate regions, such as the interior of major plates, through the framework of a community benchmark experiment. The exercise is conceptualized as a game, with known velocity fields as inputs and “players” attempting to recover the known strain rate fields either with or without realistic data noise. Eight strain rate conversion techniques are used, some utilizing horizontal velocities and some utilizing full 3D velocities. The results show the performance of various strain rate techniques in resolving strains in the presence of noise, which also provides a way of estimating uncertainties on strain rate quantities. Because the target signals are very small, the differences between strain techniques are quite noticeable on these scales. The best-performing methods are not always the same methods – they change depending on the characteristics of the target signal, such as whether it is spatially broad or sharp. The conclusions drawn by the authors are balanced and objective; I appreciate the way the pros/cons and strengths/limitations are presented. The paper clearly presents the work and the outcomes. These results are important for the interpretation of strain rates and their incorporation into seismic hazard calculations for intraplate regions, and should be published in Tektonika pending a minor revisions below.

Medium points:

Vertical velocities – it seems that some techniques use them, and some don’t. This is not typical in my experience. This also violates the idea that strain is independent of reference frame, does it not? (differences between center of mass and center of figure type definitions of vertical reference frames. Different vertical velocity fields are computed with different amounts of processing of non-tidal atmospheric and oceanic loads, for example.) How exactly are the vertical velocities used?

The organization of the manuscript is a little circular – the methods early in Part 2 assume knowledge of the methods later in Part 2. Can this part be better explained to someone uninitiated in this project?

Line 291: How is the “theoretical” value for strain rate calculated? For the noise models, the theoretical true strain is zero – this should be explained earlier. For the other ones, you need either a very dense velocity model or to choose an interpolation technique, making the “theoretical” answer not different from one of the test players. Most of these models are able to predict velocities at arbitrary points in space, but at the least, this part of the game seems under-explained.

The second invariant and the divergence/dilatation should be defined, either in words or equations. Dilatation is pretty standard but there are several ways to define I2, one is the square root of the other.

One result is that reasonable GIA can be resolved in the strain signal, even in the horizontal components. Can the authors include any thoughts on whether GIA-related strain should be included in applications in seismic hazard?

Small Points:

Abstract: Line 36: I would suggest for clarity: “able to retrieve the strain rate signals associated with active faults slipping at rates of 0.3-1mm/yr (i.e., larger than those expected for mainland France).”

Line 75: You may strengthen this with a few more general references about how we don’t expect stress and strain to necessarily agree. Some examples: Wang, 2000; Townend and Zoback, 2006; Allmendinger et al., 2009. Wang (2000) is called the “Stress strain paradox”, in Tectonophysics.

Line 132: For D and E, make sure that the first few words of these paragraphs have the name of the technique, rather than just jumping into the description. This makes it easy to skim without looking back to Table 1 for context.

Figure 2: Are these actually corresponding to models 4-9? Some of those models have noise and these velocity fields do not. In the caption, please clarify that these are individual modeled components that contribute in various linear combinations to the models 4-11.

Somewhere: you should cite similar work by Sandwell (2016) in the SCEC strain rate benchmarking exercise. SCEC community geodetic model V1: Horizontal velocity grid. It's a bit hard to find the real citation in the format that a given journal will accept, but it is a nice "prior art" on comparing many strain rate techniques using the same velocity field.

Figure 4: Caption should have vorticity (omega) so that the reader can understand the omega in the legend. Is the y-axis in nanostrain/yr? In the rest of the paper, it appears as lowercase omega, but here it is uppercase.

Figure 8: In the caption, can you please say “model 7 (Blocks+Noise)”

The data repository provided in the link in “Data Availability” is not currently available.

Comments by Reviewer 2 (Corné Kreemer)

This manuscript presents an important exercise that evaluates how different methods perform in recovering potential strain rate signal in the low straining area of France. The potential signals that are tested are mostly reasonable, although I have my doubts on the usefulness of testing cases with faults slipping at 1 mm/yr or the block model (with implied relative high velocity contrasts at block boundaries). Overall, I think this is an important effort and I am not surprised by the outcomes. I definitely appreciate the list of questions in section 5.2 that address ways forward.

My comments below are mostly minor, the most important one is about omitting any mention or use of the "data" fit. This should be a critical aspect for any strain rate method and a comparison between different methods should take that into consideration.

It probably doesn't matter for the conclusions, but there is some inconsistency in how the locations are determined/defined for which to calculate synthetic velocities. In the text (line 179-180) it is stated that it is done for all known GNSS stations in mainland France. In the caption of Figure 1 it says that all shown "other" station locations (which presumably are all used) are those for which there is a published velocity. That raises a number of issues: a) those two things are not the same, b) there are a number of stations shown outside "mainland France", c) that is ok, but it contains some obvious omissions; e.g., the velocity solution of Kreemer et al (2020) (which should have been considered) includes stations/velocities for Luxembourg and Walloon area, d) if it is based on known locations, why not include Centipede-RTK network? It would be nice, but not critical, if this could all be cleaned up a bit.

How is the theoretical strain rate model determined? Directly from theory? From a dense grid of predicted velocities? If the latter, you had to chose a method; which one? And I suppose in that case, that method should not used anymore in the testing.

Some methods seem to do well and some very poorly. The bad performing methods are particularly alarming for the noise-only cases. But what is missing here, and from the entire exercise, is what the misfit to the synthetic data is. My intuition is that the methods that do bad (e.g., C) are simply over-fitting the data. And perhaps the opposite is true is the case for methods like A or H where the noise does not "explode" into "signal". The data fit should be reported. And I suspect that the reason some methods over-fit (or under-fit) the data is the parameter settings related to smoothing. The authors acknowledge in section 5 that some methods (i.e., those require an operator-set smoothing parameter, such as in method B and C) probably could do better because of that. I suggest that the noise-only model(s) (or really a set of realizations) should be used to calibrate the various method such that the data fit is close to being the same (if possible!) for all the methods. If that is possible, then that may result in a) some bad methods performing better, c) a better comparison, and c) not needing to set “confidence thresholds” in presenting the results from the different methods. After that, you still want to report the data fit for each case and each method.

How come there are quite large theoretical dilatational strain rates in some blocks (for the block model)? The second invariant instead is near zero there, as expected.

Line 126 – fix “and and”

Line 193 – I2 has not been defined yet.

Line 373-374 - “several methods are designed for continuous velocity fields (A,B and C)”. Aren’t really most of them? Certainly D.

Line 443-452 – I don’t follow this paragraph. Where do values reported on line 445-446 come from? And how does that lead to values reported on line 448?

Figures and captions:

Don’t show outline of large lakes in the Alps area (and elsewhere) in your figures. They could be mistaken for vectors.

In general, most figures are too small. I understand that there are many model results to show, and the ones showing strain rate colors may be ok. But the readability is really affected for the figures that show vectors (Fig 2, S15,S19..)

In caption Figure 1, change Grabben to Graben

In Figure 3, why are some parts grey? Did some methods not cover the whole area? But why are some areas grey in the middle of the study area for method E?

Figure 5 and others: related to the previous comments, is the mask grey for the second invariant and white for the divergence?

There are some French words in some legends; “senestre” (Fig 1, right), “blocs” (Fig2 but also supplement, as well as captions for Table S2, Fig S6)

Authors' Reply to Reviewer 1

This paper performs a synthetic analysis of strain rate recovery techniques in very-low-strain-rate regions, such as the interior of major plates, through the framework of a community benchmark experiment.The exercise is conceptualized as a game, with known velocity fields asinputs and "players" attempting to recover the known strain rate fields either with or without realistic data noise. Eight strain rate conversion techniques are used, some utilizing horizontal velocities and some utilizing full 3D velocities. The results show the performance of various strain rate techniques in resolving strains in the presence of noise, which also provides a way of estimating uncertainties on strainrate quantities. Because the targetsignals are very small, thedifferences between strain techniques are quite noticeable on these scales. The best-performing methods are not always the same methods – they change depending on the characteristics of the target signal, such as whether it is spatially broad or sharp. The conclusions drawn by the authors are balanced and objective; I appreciate the way the pros/cons and strengths/limitations are presented. The paper clearly presents the work and the outcomes. These results are important for the interpretation of strain rates and their incorporation into seismic hazard calculations for intraplate regions, and should be published in Tektonika pending a minor revisions below.

Thank you for your time and suggestions that we address in the following.

Medium points:

Vertical velocities – it seems that some techniques use them, and some don't. This is not typical in my experience. This also violates theidea thatstrain is independent of reference frame, does it not?(differences between center of mass and center of figure typedefinitions of vertical reference frames. Different vertical velocity fields are computed with different amounts of processing of non-tidal atmospheric and oceanic loads, for example.) How exactly are the vertical velocities used?

Fair enough, using vertical velocities is not straightforward to compute 2D strain rates. Some attempts have been published however in Mazzotti et al. 2005 (based on Malvern, 1969) and Goudarzi et al. 2015 (Geostrain method). It is done by assuming that vertical displacement (velocities) on a sphere induce isotropic extension/shortening and therefore adding the ratio Vz/R to the diagonal components of the strain rate tensor, R being the radius of the Earth (see l.140 of the manuscript).

It is indeed well known that vertical velocities depend on the reference frame, with potential biases of ~0.5 mm/yr in the latest geodetic frame realizations (ITRF2014 and later). In our case, the maximum vertical velocities are ~2 mm/yr in the Western Alps (GIA model), i.e. that would end up in horizontal extension rates of 0.3 x 10^{-9} yr^{-1} or less. Much smaller than strain rates due to horizontal velocities. We therefore believe that their impact is negligible on the calculations in our region of study.

The organization of the manuscript is a little circular – the methods early in Part 2 assume knowledge of the methods later in Part 2. Can this part be better explained to someone uninitiated in this project?

We added some clarifications in section 2.1 that should help understanding the overall strategy before digging into the details of the methods in section 2.2.

Line 291: How is the "theoretical" value for strain rate calculated? For the noise models, the

theoretical true strain is zero – this should be explained earlier. For the other ones, you need either a very dense velocity model or to choose an interpolation technique, making the "theoretical" answer not different from one of the test players. Most of these models are able to predict velocities at arbitrary points inspace, but at the least,this part of the game seems under-explained.

Agreed, it has been clarified in the new version.

We compute theoretical strain rates either directly using analytical formulations when available (i.e., fault dislocation and plate flexure models) or using a dense regular velocity grid (0.1°x0.1°) on which we apply standard direct gradient calculations (i.e., velocity difference over grid size for neighboring grid points). The theoretical strain rates are therefore very different from those derived by the methods tested by the different players, who use various techniques to perform spatial interpolations from uneven GNSS station locations as well as spatial smoothing / filtering to extract long-wavelength signal over short-wavelength noise.

In the former version of the manuscript, the calculation method was implicitly referred to at lines 281 and 285 stating :
"For models 8 and 9, the theoretical vorticity is computed by derivation of the interpolated velocity field." and *"Note that the theoretical strain rates invariant and vorticity are computed and not given analytically for these combined models."*

We now state at the end of section 2.1 :

"For all models except 7 and 11 (i.e, block rotation models), the theoretical strain rate tensors are computed at each grid node based on analytical formulations from elastic plate flexure and fault dislocation equations. We compute the vorticity from a dense and regular 0.1°x0.1° velocity field using standard gradient computation in order to minimize interpolation artifacts. The same strategy is used to compute the strain rate tensors and their invariants for velocity fields computed using rigid blocks. Vorticity is computed analytically for model 7 (rigid blocks and noise)."

The second invariant and the divergence/dilatation should be defined, either in words or equations. Dilatation is pretty standard but there are several ways to define I2, one is the square root of the other.

The definition of the invariant was provided in table S1 in the former version of the manuscript. We now define them in the main text in section 2.3 for clarity based on your suggestion.

One result is that reasonable GIA can be resolved in the strain signal, even in the horizontal components. Can the authors include any thoughts on whether GIA-related strain should be included in applications in seismic hazard?

Agreed, we added the following in the Discussion:

"Horizontal strain rates associated with Alpine GIA are among the signals that can be best estimated in our analyses (Fig. 8). This is in agreement with studies using actual geodetic data in the Western Alps showing that GIA contributes significantly to observed vertical velocities and horizontal velocities and strain rates (e.g., Nocquet et al., 2016; Masson et al., 2019; Sternai et al., 2019). Such results also have strong implications for fault activity and seismic hazard, with a current debate regarding the potential impact of GIA fault activation (e.g., Walpersdorf et al., 2018; Grosset et al., 2023)."

Small Points:
Abstract: Line 36: I would suggest for clarity: "able to retrieve the strain rate signals associated with

active faults slipping at rates of 0.3-1mm/yr (i.e., larger than those expected for mainland France)."

ok

Line 75: You may strengthen this with a few more general references about how we don't expect stress and strain to necessarily agree. Some examples: Wang, 2000; Townend and Zoback, 2006; Allmendinger et al., 2009. Wang (2000) is called the "Stress strain paradox", in Tectonophysics.

We added references to Wang (2000) and Townend & Zoback (2006) to strengthen this stress / strain rate paradox.

Line 132: For D and E, make sure that the first few words of these paragraphs have the name of the technique, rather than just jumping into the description. This makes it easy to skim without looking back to Table 1 for context.
Agreed and corrected.

Figure 2: Are these actually corresponding to models 4-9? Some of those models have noise and these velocity fields do not. In the caption, please clarify that these are individual modeled components that contribute in various linear combinations to the models 4-11.
Yes indeed, we chose to represent the meaningful geophysical signal for models 4 to 11 without noise, as stated in the caption "Synthetic horizontal velocity field for models 4 to 9 without noise". We propose to make it clearer based on your suggestion.

Somewhere: you should cite similar work by Sandwell (2016) in the SCEC strain rate benchmarking exercise. SCEC community geodetic model V1: Horizontal velocity grid. It's a bit hard to find the real citation in the format that a given journal will accept, but it is a nice "prior art" on comparing many strain rate techniques using the same velocity field.

Fair enough, this work has been an important source of inspiration for us, it is now cited in the main text (l.91).

Figure 4: Caption should have vorticity (omega) so that the reader can understand the omega in the legend. Is the y-axis in nanostrain/yr? In the rest of the paper, it appears as lowercase omega, but here it is uppercase.
Agreed, it has been corrected.

Figure 8: In the caption, can you please say "model 7 (Blocks+Noise)"

Added and corrected.

The data repository provided in the link in "Data Availability" is not currently available.

Yes, we were waiting for the first round of review to ask for the DOI and validate the submission of the data set to the EasyData repository. The final process is on its way and the data sets and models will be accessible online : https://doi.org/7685771d-eee6-462e-8757-f28e60023c55.
The contents of this repository are available for reviewers here : https://cloud.osu-lyon.fr/s/6QFgJrf472t8ERc

This DOI points out toward the synthetic velocity fields built for this study and to the outputs of each method (A to H) presented as grid files for each model

(1-11). We attach to this resubmission the PDF of the metadata from this repository.

Authors' Reply to Reviewer 2

This manuscript presents an important exercise that evaluates how different methods perform in recovering potential strain rate signal in the low straining area of France. The potential signals that are tested are mostly reasonable, although I have my doubts on the usefulness of testing cases with faults slipping at 1 mm/yr or the block model (with implied relative high velocity contrasts at block boundaries). Overall, I think this is an important effort and I am not surprised by the outcomes. I definitely appreciate the list of questions in section 5.2that address ways forward.

My comments below are mostly minor, the most important one is about omitting any mention or use of the "data" fit. This should be a critical aspect for any strain rate method and a comparison between different methods should take that into consideration.
Thank you for your constructive comments and time on this review. We address them in this new version and think they do help improve the manuscript.

It probably doesn't matter for the conclusions, but there is some inconsistency in how the locations are determined/defined for which to calculate synthetic velocities. In the text (line 179-180) it is stated that it is done for all known GNSS stations in mainland France. In the caption of Figure 1 it says that all shown "other" station locations (which presumably are all used) are those for which there is a published velocity. That raises a number of issues: a) those two things are not the same, b) there are a number of stations shown outside "mainland France", c) that is ok, but it contains some obvious omissions; e.g., the velocity solution of Kreemer et al (2020) (which should have been considered) includes stations/velocities for Luxembourg and Walloon area, d) if it is based on known locations, why not include Centipede-RTK network? It would be nice, but not critical, if this could all be cleaned up a bit.

Based on your comment, we clarified the text in several places. We may have missed some networks and stations that are included in Kreemer et al. 2020, and it is now specified in the discussion section (l.510-511).
"Would we get better strain rate recovery if we had a denser velocity field with the same noise amount ? Some locally denser velocity fields have been published recently (Kreemer et al. 2020), suggesting room for improvement."

In section 3.1, we now state :

"We therefore chose to sample our synthetic velocity fields on a reasonably optimistic available network, i.e., we associate a velocity value to each GNSS station in mainland France that has already been used to build these secular velocity fields, whether the station is still operating or not, and including all

known benchmark networks (hereafter called semi-permanent). We did not include stations from the recent Centipede network (Bosser et al. 2024, Ancelin et al. 2025), since it remains unclear up to now whether we can use these stations to derive robust long-term velocities. Added to permanent stations from other networks abroad, that have not been considered exhaustively, this results in a theoretical network composed of 1600 stations covering France and neighboring countries."

How is the theoretical strain rate model determined? Directly from theory? From a dense grid of predicted velocities? If the latter, you had to chose a method; which one? And I suppose in that case, that method should not used anymore in the testing.

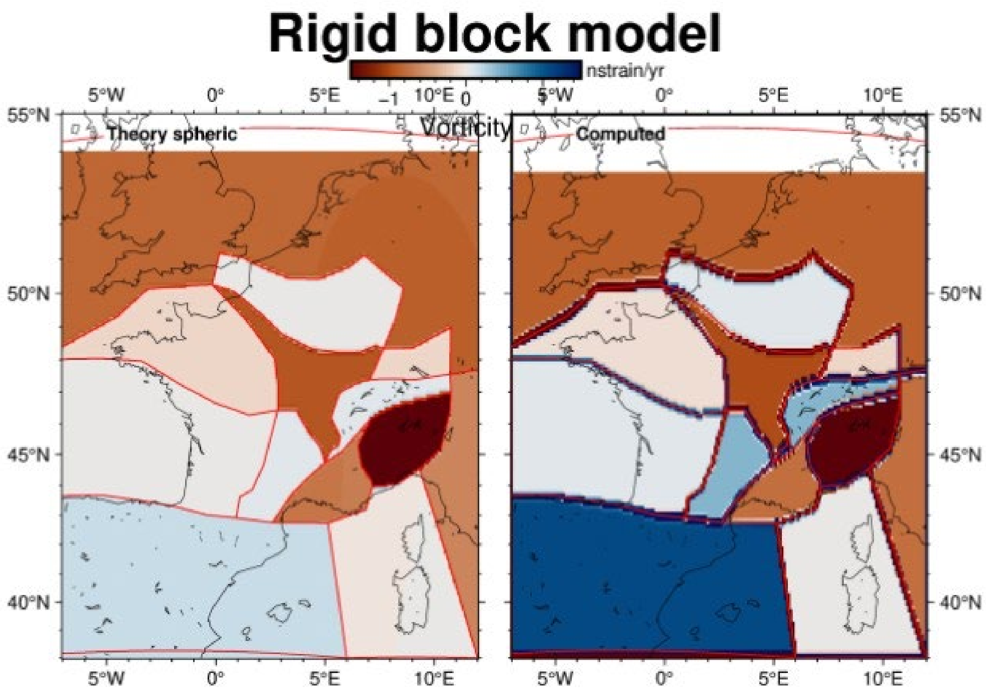
Yes, this is a crucial point, indeed. Most of the geophysical models allow for computing strain rates

analytically, which is done for flexural models and fault models. However, we had to compute vorticity manually by computing gradients based on a 0.1° resolution velocity grid, using discrete numerical approximation in python.

Theoretical strain rate and vorticity are computed the same way for block motion models (models 7 and 11) based on a 0.1°x0.1° grid. This non-analytical solution leads to artifacts at block boundaries for vorticity in Fig.8 while the proper computation was made to compute Fig.S3 (see comparison of theoretical vs computed vorticity on the figure below). This error has been corrected in the new version of the manuscript.

We also now detail our strategy for synthetic strain rate tensor computation at the end of section 2.1 :

"For all models except 7 and 11 (i.e., block rotation models), the theoretical strain rate tensors are computed at each grid node based on analytical formulations from elastic plate flexure and fault dislocation equations. We compute vorticity from a dense and regular 0.1° x 0.1° velocity field using standard gradient computation in order to minimize interpolation artifacts. The same strategy is used to compute the strain rate tensor and its invariants for velocity fields computed using rigid blocks. Vorticity is computed analytically for model 7 (rigid blocks and noise)."

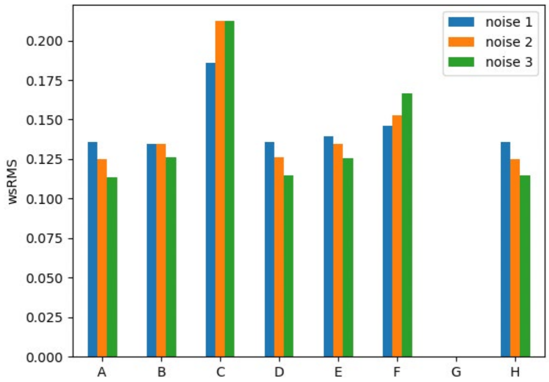


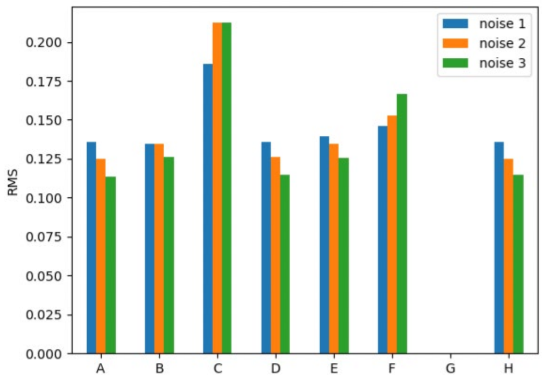
Some methods seem to do well and some very poorly. The bad performing methods are particularly alarming for the noise-only cases. But what is missing here, and from the entire exercise, is what the misfit to the synthetic data is. My intuition is that the methods that do bad (e.g., C) are simply over-fitting the data. And perhaps the opposite is true is the case for methods like A or H where the noise does not “explode” into “signal”. The data fit should be reported. And I suspect that the reason some methods over-fit (or under-fit) the data is the parameter settings related to smoothing. The authors acknowledge in section 5 thatsome methods (i.e., those require an operator-set smoothing parameter, such as in method B and C) probably could do better because of that. I suggest that the noise-only model(s) (or really a set of realizations) should be used to calibrate the various method such that the data fit is close to being the same (if possible!) for all the methods. If that is possible, then that may result in a) some bad methods performing better, c) a better comparison, and c) not needing to set “confidence thresholds” in presenting the results from the different methods. After that, you still want to report the data fit for each case and each method.

When conceiving the benchmark exercise, we indeed did not ask for predicted velocities as outputs of

the strain rate estimations, which limits our capacity in answering the reviewer’s point. The instructions for the exercise were to propose the best results (as judged by each player) for the whole set of models (1-11) with the same parametrization. Therefore, we were relying on the subjective choice of the player rather than statistical indicators on the fit to the data. However, yes, we agree with the reviewer that this fit may vary significantly from one method to the other, and we partially address this point in the reviewed version of the manuscript.

To do so, we compute the weighted RMS (wRMS, figure R1) between the synthetic and predicted velocities that were made available by all methods but G for the 3 noise models. Most methods predict a continuous velocity field, which was interpolated at GNSS stations, while others predict velocities at GNSS locations. It is also important to note that some methods do not optimize the fit to the velocities as part of the strain estimations, e.g., Gausstrain (Mazzotti et al., 2004) or STIB (Masson et al., 2014). The latter actually uses the velocity gradient along baselines and does not provide a velocity output.





From Figure R1, contribution C performs poorly with respect to the others in terms of adjustment to the input velocities, which seems to rule out the overfitting hypothesis for its bad performance on the noise models. Similarly, the differences in velocity fit are low between contributions A, B, D, E, F and H, while they differ significantly in strain rate performances. This first order analysis argues against using the fit to the velocities as a confidence indicator of the contribution ability to capture original strain rates. This is likely because a similar overall fit to the observations could mask a significant deviation from their associated “true” derivatives.

In the new version of the manuscript, we have added these sentences to clarify our thought :
In section 2.1 :
"*Computing the strain rate tensors from discrete velocities is a highly non-unique problem, and most (but not all) methods developed to do this first interpolate the velocity field before taking its derivatives. They therefore provide predicted velocities at observation points, allowing estimations of data fit that are usually very good (few tenth of mm/yr on average). However, because the goodness of the velocity fit is a necessary but not sufficient condition to recover the correct strain rate patterns, we do not compare directly observed and predicted velocities in this exercise, but rather focus on estimating indicators derived from velocity gradients (see section 2.3).*"

In section 3.1 :
"*We compute the wRMS between observed and predicted velocities for methods allowing this analysis, as shown in figure S24: no significant overfitting is observed for method C, nor systematic misfit for methods A, F or H, that are thought to be performing slightly better than others. It confirms that the fit to the observed velocities is not efficient to discriminate between methods.*"
We also added Figure R1 as supplementary figure S24.

How come there are quite large theoretical dilatational strain rates in some blocks (for the block model)? The second invariant instead is near zero there, as expected.

Divergence is indeed zero inside blocks, as shown in Fig.S3. The confusion should come from the fact that this is vorticity plotted in Figure 8 and not divergence. We recompute correctly vorticity so that it appears clearer on the theoretical panel (see answer to previous comment). Thanks for pointing this out.

Line 126 – fix "and and"

ok

Line 193 – I2 has not been defined yet.

Yes, corrected. It is now defined on section 2.3.

Line 373-374 - "several methods are designed for continuous velocity fields (A,B and C)". Aren't really most of them? Certainly D.

Yes, most of them, it has been corrected.

Line 443-452 – I don't follow this paragraph. Where do values reported on line 445-446 come from? And how does that lead to values reported on line 448?

In this paragraph, we are commenting on the “confidence thresholds” defined in table 3. We add in the caption of table 3 this sentence to help the reader *“Maximum thresholds for tuned methods are in bold, and can be considered as conservative values.”* In the aforementioned paragraph, we refer now to these conservative values (i.e. maximal, all methods considered).

Figures and captions:
Don’t show outline of large lakes in the Alps area (and elsewhere) in your figures. They could be mistaken for vectors.
Ok, corrected.
In general, most figures are too small. I understand that there are many model results to show, and the ones showing strain rate colors may be ok. But the readability is really affected for the figures that show vectors (Fig 2, S15,S19..)
We enlarge the figures when possible.

In caption Figure 1, change Grabben to Graben

Done.

In Figure 3, why are some parts grey? Did some methods not cover the whole area? But why are some

areas grey in the middle of the study area for method E?

Some methods do not compute strain rate at grid nodes too far from the stations, resulting in NaNs plotted in gray. A threshold of 0.2mm/yr on the standard deviation of the PDF of horizontal velocity is used for method F, resulting also in areas with no value. On the contrary, you are right in pointing out a problem in NaN values in Eastern France in the E-method results. It occurs to result from very low uncertainties (lower than 0.01mm/yr) at some stations preventing proper calculation of strain rates. The player in charge of this method has redone all his calculations setting all uncertainties lower than 0.01mm/yr at this threshold value (as already done in method F). This is now stated in the method E description. Because of this new calculation, threshold values presented in table 3 have slightly changed. Method E now performs slightly better, but not sufficiently to change the conclusion of the paper. As a result, method E in the new Figure 3 does not exhibit NaNs in central Eastern France.

We now also choose to mask offshore areas.

Figure 5 and others: related to the previous comments, is the mask grey for the second invariant and white for the divergence?

Yes. In the new version, we plot every NaN in grey and specify it in the figures’captions.

There are some French words in some legends; “senestre” (Fig 1, right), “blocs” (Fig2 but also supplement, as well as captions for Table S2, Fig S6)

Thanks, it has been corrected.

Acceptance Letter

30 March 2026

Dear Marianne and co-authors,

We have read your responses to the reviewers and revised manuscript. We appreciated the work you did on the manuscript and the way you carefully addressed the reviewers' comments. Our conclusion is that you have responded to the reviewers' concerns adequately, and that you manuscript is now suitable for publication in Tektonika.

We are happy to tell you that the manuscript is now accepted for publication.

There are few typos that you will be able to correct at the copy-editing / proof stage. Your paper contends many figures, in particular in the supplementary file. Please check that all figure calls (right numbers) are correct, both in the main text and suppl. [Associate editor has for ex. detected that "SFigures 11 and 12 have bad references to other figures in my PDF ("??")].

You will be contacted by our copy editing team in the coming weeks. They will ask you to provide files in the most suitable format for production.

Thanks for submitting to Tektonika !

Best regards,

Devin McPhillips, Tektonika associate editor
Robin Lacassin, Tektonika executive editor