

# Review Report

**Métois et al., Bayesian estimation of surface strain rates in the peri-Adriatic, Balkans and Aegean region, TEKTONIKA, 2025.**

**Manuscript submitted: 2 December 2024**

**Accepted 14 April 2025**

## Table of Contents

<b><i>1st Round of Revisions</i></b> .....	<b>02</b>
<b>Decision Letter (18 February 2025)</b> .....	<b>02</b>
<b>Comments by Reviewer 1</b> .....	<b>03</b>
<b>Comments by Reviewer 2</b> .....	<b>16</b>
<b>Comments by Reviewer 3</b> .....	<b>18</b>
<b>Authors' Reply to Reviewers (27 March 2025)</b> .....	<b>20</b>
<b><i>2nd Round of Revisions</i></b> .....	<b>39</b>
<b>Decision Letter (8 April 2025)</b> .....	<b>39</b>
<b>Authors answer (9 April 2025)</b> .....	<b>39</b>
<b><i>Acceptance Letter (14 April 2025)</i></b> .....	<b>40</b>

## 1<sup>st</sup> Round of Revisions

Decision Letter (18 February 2025)

Dear Marianne Métois, and co-authors,

Thank you for your submission to Tektonika on deriving strain rates in the peri-Adriatic, Balkans, and Aegan regions from GNSS data. We have now recieved three reviews on this submission, with all reviewers agreeing that this represents a robust and novel contribution. However, some changes are required to the address the reviewer's comment's, which are mainly concerned with the implications of this study and its context within the wider literature.

We look forward to receiving the revised submission. Together with the revised manuscript, please upload a rebuttal answering all points raised by the reviewers, and also a manuscript version with changed marked. Given the relatively minor nature of these revisions, we would ask that you return a revised submission within 1 month.

Best wishes

Jack Williams, associate editor

Robin Lacassin, executive editor

## Comments by Reviewer 1

### Reviewer 1

**Reviewer has also commented using the Tektonika review form - Please answer to all comments, those below and those in the form. Their review form is attached in the following pages.**

The manuscript investigates surface strain rates from GNSS velocities in the peri-Adriatic, Balkans, and Aegean regions using Bayesian trans dimensional inversion, implemented in the B-Strain software, discussed in detail in a previous paper from Pagani et al. (2021). This is a valuable contribution as it provides refined insights into surface deformation in a tectonically active area characterized by complex strain heterogeneities. The methodology, employing a probabilistic approach to address limitations of traditional techniques, such as interpolation artifacts and user-defined smoothing, provides probabilistic maps of strain rate invariants, divergence, vorticity, providing a thorough analysis of surface deformation. These results are compared with those obtained adopting a widely used method, VISR3, on the same GNSS velocity dataset, presented in a previous paper from *Pina-Valdes et al. (2022)*. Importantly, the authors make their results accessible via a web tool and archive, enhancing transparency and reusability. The results highlight tectonic features (e.g., segmentation of the Apennines, contrast between Nubian and Tyrrhenian domains, extent of the South Balkans Extension Region) and their implications for regional seismic hazard assessments. However, some sections are overly detailed, particularly in the methodology and result discussions. A more concise narrative would enhance readability. More importantly, the distinction between new insights and confirmation of previously known features is not always clear, and I think that highlighting novel contributions would improve the impact of this paper.

While the study is methodologically sound and offers valuable insights, improvements in clarity, conciseness, and the provision of additional methodological details would enhance its impact and accessibility. I recommend the manuscript for publication after minor revisions addressing the points mentioned above.

# Tektonika peer-review form

## Section A: Overview of manuscript

### A1) Overall evaluation, general comments & summary

#### A1.1) Reviewer's comments

##### **A1.1.1 ) General evaluation and publication suggestion – Required:**

*Please use this space to describe, in your own words, the core subject of the submission and your overall assessment of its suitability for publication.*

The manuscript investigates surface strain rates from GNSS velocities in the peri-Adriatic, Balkans, and Aegean regions using Bayesian trans dimensional inversion, implemented in the B-Strain software, discussed in detail in a previous paper from Pagani et al. (2021). This is a valuable contribution as it provides refined insights into surface deformation in a tectonically active area characterized by complex strain heterogeneities. The methodology, employing a probabilistic approach to address limitations of traditional techniques, such as interpolation artifacts and user-defined smoothing, provides probabilistic maps of strain rate invariants, divergence, vorticity, providing a thorough analysis of surface deformation. These results are compared with those obtained adopting a widely used method, VISR3, on the same GNSS velocity dataset, presented in a previous paper from Pina-Valdes et al. (2022). Importantly, the authors make their results accessible via a web tool and archive, enhancing transparency and reusability. The results highlight tectonic features (e.g., segmentation of the Apennines, contrast between Nubian and Tyrrhenian domains, extent of the South Balkans Extension Region) and their implications for regional seismic hazard assessments. However, some sections are overly detailed, particularly in the methodology and result discussions. A more concise narrative would enhance readability. More importantly, the distinction between new insights and confirmation of previously known features is not always clear, and I think that highlighting novel contributions would improve the impact of this paper.

While the study is methodologically sound and offers valuable insights, improvements in clarity, conciseness, and the provision of additional methodological details would enhance its impact and accessibility. I recommend the manuscript for publication after minor revisions addressing the points mentioned above.

**A1.1.2 ) What does the submission need to be publishable? (select as needed; comment for all cases)**

- No changes required
- Rewriting
- Reorganising
- More data/figures
- Condensing
- Reinterpretation
- Other

**Comments:**

Some sections are overly detailed, particularly in the results and discussions sessions. A more concise narrative would enhance readability. The distinction between new insights and confirmation of previously known features is not always clear and I think that highlighting novel contributions would improve the impact of this paper.

While the manuscript compares B-Strain results with VISR3D ones, it could better emphasize the practical implications of the observed differences (e.g., for seismic hazard modelling or geodynamic interpretations). Moreover, the authors limit the comparison with VISR3, but the same area has been investigated with other methods, that, in some cases, seems to overcome limitations of the VISR3 approach. For example, the multiscale approach described in Tape et al. (2009), which adopts a spherical wavelet representation of the continuous velocity field, seems to provide an efficient and objective way to generate a continuous velocity field from discrete velocity measurements, with limited user choices. Maurer and Materna (GJI, 2023) discuss the results from this method with results from the application of more “traditional” methods, including VISR3, for western U. Serpelloni et al. (2022) compare VISR3 with multiscale strains for about the same area investigated in this manuscript. I think that for some sectors, also discussed in this manuscript, a comparison with results from other methods should be considered. Clearly, there might be some difference in the GNSS velocity datasets, and these comparison with expanded literature are feasible only where the density of stations is comparable.

Figures, particularly those illustrating strain rate maps and tectonic profiles, need better labels and clarity. Some readers may find it challenging to interpret without clearer legends or annotations.

Certain technical terms and methodological steps (e.g., parameterization choices, hyperparameter scaling) require more explanation for accessibility to a broader audience.

**A1.1.3) Can the submission be improved by reducing/adding any of the following? (select as needed; comment for all cases)**

- Text
- Table
- Figures
- Supplementary material

**Comments:**

Some sections, particularly in the methodology and results, could be more succinct to enhance readability.

Ensure all figures are well-labeled and easily interpretable, possibly by adding more descriptive captions or annotations. See specific comments on figure section of this review form.

**A1.1.4) Please complete the following section if you recommend that the submission is NOT appropriate for publication (select as needed; comment if a box is selected)**

- Quality is poor
- Research is not reproducible
- Other

**Comments:**

[Free form box]

**A1.2) Author(s) Responses:**

## **A2) Summary of main merits and main points of improvement**

### **A2.1) Reviewer's comments**

*Please describe below in a few sentences (100 to 300 words) the main merits of the submission and suggestions for improvements.*

The probabilistic approach effectively addresses the limitations of traditional techniques, such as interpolation artifacts and reliance on user-defined smoothing parameters. By providing probabilistic outputs, the study quantifies uncertainties, enhancing confidence in the results. It identifies key tectonic features and explores their implications for regional seismic hazard assessments, offering valuable insights to the field. Furthermore, the authors promote transparency and accessibility by sharing their results via a web-based visualization tool and an online repository. However, the comparison with the VISR3D method could be more detailed, particularly regarding the practical significance of the observed differences, such as their impact on seismic hazard or geodynamic models. Additionally, the discussion could benefit from a more in-depth exploration of how these findings can be applied in practical scenarios, including seismic hazard assessment and geodynamic modeling.

### **A2.2) Author's responses:**

[Free form box]

## Section B: Detailed evaluation of manuscript

### B1) Title and abstract

#### B1.1) Reviewer's comments

*These statements are a **guide** to what good Titles and Abstracts include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Title* describes the main topic of the manuscript **accurately** — [YES]

The *Title* describes the main topic of the manuscript **succinctly** — [YES]

The *Title* includes **appropriate key terms** — [NO]

The *Abstract* includes a **clear aim and rationale** — [YES]

The *Abstract* supports the rationale with **sufficient background information** — [YES]

The *Abstract* includes a **well-balanced description of the methods** — [YES]

The *Abstract* describes the **main results sufficiently and adequately** — [YES]

The *Abstract* clearly describes the **importance/impact of the study** — [YES]

The *Abstract* clearly states the **conclusions of the study** — [YES]

The *Abstract* is **clear and well structured** — [YES]

#### **Comments:**

I suggest including the source of data, GNSS velocities, in the title. As regards the abstract, I suggest to more clearly specify what the authors mean with results being “more robust” than the those produced from “standard” techniques. How is robustness defined?

#### B1.2) Author's responses

[Free form box]

## B2) Introduction

### B2.1) Reviewer's comments

*These statements are a **guide** to what good Introductions include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Introduction* provides **sufficient background and context** for the study — [YES]

The *Introduction* describes the **aim/hypothesis/rationale** clearly, providing **sufficient context** — [YES]

The *objective/hypothesis/rationale* **flows logically from the background** information — [YES]

The *Introduction* describes the study's **objective and approach** (last paragraph) — [YES]

The *Introduction* contains **relevant, suitable citations** — [YES]

The *Introduction* is **organized effectively** — [YES]

#### **Comments:**

[Free form box]

### B2.2) Author's responses

[Free form box]

## B3) Data and methods

### B3.1) Reviewer's comments

*These statements are a **guide** to what good Method sections include and good practices for Dataset accessibility. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Methods* are described **concisely and with enough detail** for reproducibility — [NO]

Necessary information about **data sources/acquisition/processing** is included — [YES]

**Data used are accessible** via either supplementary files or links in the data availability statement — [YES]

The *Dataset and/or Methods* are **organized effectively** — [YES]

#### **Comments:**

The GNSS velocity dataset is clearly identified, with appropriate references to prior studies and an online repository providing access to the cleaned and filtered data. The methods are described concisely and with sufficient detail to allow for reproducibility. The probabilistic approach is well-articulated, particularly in its ability to address limitations of traditional methods, such as interpolation artifacts and fixed smoothing parameters. Additionally, the results are made accessible via a web-based visualization tool and an archived repository, enabling users to interact with the probabilistic strain rate maps.

It would be helpful for the authors to elaborate on what they mean by “traditional” methods and clarify how they classify strain-rate inversion methods as either traditional or non-traditional. For instance, VISR3D is referred to as a “traditional” method due to its reliance on a fixed level of smoothing, but other methods, such as those proposed by Tape et al. (2009), adapt smoothing parameters to the density of GNSS stations. Are these adaptive methods considered traditional or non-traditional? The manuscript would benefit from a more precise discussion of this classification.

Although the use of Delaunay triangulation and hyperparameter inversion is mentioned, the manuscript does not adequately explain how parameterization choices, such as node density or bounds, were determined, nor does it discuss how these decisions influence the results. Similarly, the rationale behind applying a 10% uncertainty scaling remains insufficiently detailed, which could hinder reproducibility. While the results are accessible via a web tool, a user seeking to reproduce the findings might encounter challenges due to the lack of a detailed workflow. Since this is a methodological paper, it would be valuable to provide a step-by-step guide for applying the B-Strain code, either as supplementary material or in an online resource. This addition would greatly enhance the accessibility and usability of the methodology for the research community.

### B3.2) Author's responses

[Free form box]

## B4) Results

### B4.1) Reviewer's comments

*These statements are a **guide** to what good Result sections include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Results* findings are **supported by data** — [YES]

The *Results* findings are presented **clearly and succinctly** — [NO]

The text in the *Result* section **cites tables and figures appropriately** — [YES]

The *Results* directly **relate to the study objectives** — [YES]

The *Results* present **data for all the approaches** described in the *Methods* section — [YES] / [NO]

The *Results* **text belongs to the Results section**, not to *Introduction*, *Methods*, or *Discussion*. — [YES] / [NO]

The *Results* section is **organised effectively** — [NO]

#### **Comments:**

Overall, the findings are well-supported by the data, with strong connections between GNSS-derived inputs and the computed strain rate patterns. The use of a probabilistic framework enhances the credibility of the results, and the key features align with established geodynamic knowledge. However, there is room for improvement in the clarity and presentation of the findings. This includes refining overly detailed sections, improving figure labels and annotations, and placing greater emphasis on novel contributions. Additionally, providing more accessible quantitative comparisons would make the results more impactful for the geoscience community. Enhanced visualization (e.g., as outlined in section B6) and an expanded discussion of limitations would further strengthen the overall presentation.

Although the study discusses practical applications, these are not explored in sufficient depth or supported with specific examples. To improve this connection, the authors could elaborate on how the probabilistic findings enhance geodynamic models or, for instance, contribute to refining seismicity rate estimations constrained by geodetic data models. This would highlight the broader relevance of the results.

The results are presented as maps and cross sections, with cross sections including comparisons to VISR3 results from Pina-Vidales et al. (2022). However, the inclusion of a modeled continuous velocity field would be highly beneficial. Since strain rates concentration represent at first order the steepness of horizontal velocity gradients, showing velocity profiles (e.g., profile-parallel velocities) would provide additional context. It would be valuable to evaluate how the B-Strain modeled continuous velocities align with the observed velocities, offering a more complete picture of the model's performance.

## B5) Discussion and conclusions

### B5.1) Reviewer's comments

*These statements are a **guide** to what good Discussions and Conclusions include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Discussion* is **focused on the objectives** of the study — [YES]

The *Discussion* **addresses all major results** of this study, which are shown in *Results* — [YES]

The *Discussion* section makes **comparisons with other studies** that are relevant and informative — [NO]

The *Discussion* section properly identifies all **speculative statements** — [YES]

The *Discussion* section presents the **implications of the study** persuasively — [NO]

The *Discussion* section **highlights novel contributions** appropriately — [NO]

The *Discussion* section **addresses the limitations** of the study appropriately — [NO]

The *Discussion* section is **organised effectively** — [YES]

The *Conclusions* are **consistent** with and **summarise** the rest of the manuscript — [YES]

The *Conclusions* are **supported by the data** in *Results* and **follow logically** from the *Discussion* — [YES]

The *Conclusions* are **clear and concise** — [YES]

#### **Comments:**

The discussion aligns with the study objectives, focusing on evaluating the methodological advancements and linking the results to geodynamics and regional kinematics. However, it could better emphasize the novel contributions of B-Strain compared to confirmations of known strain-rate field features derived from other methods. The discussion currently centers on comparisons with results from Pina-Valdes et al. (2022) using the VISR3 approach, but the study area has been analyzed in numerous other papers. While a comprehensive comparative analysis, as done in Maurer and Materna (2023) or Nucci et al. (2024), is not strictly requested (but would be really interesting) the discussion could be significantly improved if the authors compare their findings, especially in areas where B-Strain outperforms VISR3 in resolving sharp gradients (e.g., the North Anatolian Fault), with results from studies employing alternative approaches (in particular Tape et al., 2009).

Although the discussion highlights some novel contributions, such as the advantages of B-Strain and the improvements in strain rate mapping, it does not sufficiently emphasize the significance of these findings. To better showcase the novelty, the authors should clearly distinguish between new insights and confirmations of previous work. Contextualizing the novel contributions within the broader fields of

geophysics and seismic hazard research would strengthen the narrative. Specific examples of how B-Strain results advance practical applications (e.g., seismic hazard modeling) or geodynamic interpretations would also enhance the discussion.

The discussion appropriately addresses several limitations, particularly those related to sparse GNSS coverage and data heterogeneity. However, certain areas need further attention: 1) provide a more detailed analysis of how sparse data and parameterization choices affect the results; 2) compare B-Strain with other advanced methods (beyond VISR3, which can be considered a standard) to present a more balanced view of its strengths and weaknesses; 3) offer potential solutions or future directions to address data accessibility issues, such as the lack of raw GNSS data in certain regions. Addressing these points would not only improve the clarity and comprehensiveness of the discussion but also enhance the study's impact by demonstrating the broader relevance of B-Strain in geophysical research and practical applications.

## **B5.2) Author's responses**

[Free form box]

## B6) Figures, tables and citations

### B6.1) Reviewer's comments

*These statements are a **guide** to what good Figures and Tables include and how they are presented. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

*Tables and Figures are **ordered logically** and **numbered sequentially** — [YES]*

*Tables and Figures have **captions that explain** all their major features — [YES]*

*Tables and Figures have **captions that complement** the information in the main text — [YES]*

*Tables and Figures present data that **relate** to the study objective — [YES]*

*Tables and Figures present data that are **consistent** with and support the description of results — [YES]*

*Tables and Figures have **succinct and informative titles** — [YES]*

*Figures are **accessible** (elements are clearly labelled, accessible colour palettes, colour contrasts, font size legible, etc....) — [NO]*

*Please, check our [\[Figure guidelines\]](#)*

*Figures with **maps or cross-sections** contain all **elements to be understood** (north arrow orientation, scale, visible coordinates, sufficient coordinate grid intercepts) — [NO]*

*Figures with **maps** have **sufficient location information** (in the map or caption) — [YES]*

*Cross-sections have clear labels for **scale and coordinates** at ends and within-section kinks — [NO]*

*All georeferenced elements are provided in common format (.shp, .geotiff, .kml) [in an open-access repository] — [YES]*

*Citations throughout are relevant, suitable, and comprehensive — [YES]*

### **Comments:**

Figure 1: tracks of profiles are not clearly visible. What about plot these profiles in Figure 4?

Figure 2: the symbols representing GNSS stations are not visible in panel B.

Figure 3: increase green line thickness.

Figure 4: increase green line thickness. I suggest adding a label to the black arrows for features discussed in the maintext.

Figure 7: the colours representing tectonic style from EFSM are indeed not easily visible in the panel B. Focal mechanisms are already shown in Figure 1, so I suggest remove FM from Figure 7 and increase the fault line thickness (possibly on top of the layers).

## **B6.2) Author's responses**

[Free form box]

## **Section C: Additional comments**

### **C1) Minor/line-numbered comments**

#### **C1.1) Reviewer's comments**

Page 8 Line 315: I think it is Figure S3 not S2.

Page 9 Line 388: remove “?”

Page 10 Line 429: remove “?”

Page 10 Line 430: remove “?”

Page 10 Line 434: remove “?”

#### **C1.2) Author's responses**

[Free form box]

### **C2) Other remarks**

#### **C2.1) Reviewer's comments**

[Free form box]

## Comments by Reviewer 2

Review of Metois et al., 2025, Tektonica

This paper applies a recently published Bayesian technique (B-strain) for determining strain rates from geodetic velocities to data from the Eastern Mediterranean and the Balkans. This area presents major seismic hazards and yet parts of it are frequently viewed as a part of the stable European continent from a GNSS strain point of view. Typical strain rate methods require smoothing to be defined by the user, but this would pose a challenge in the Balkans because certain areas are very active shear zones (like the North Anatolian fault), others are diffuse deformation zones, while others are relatively more rigid. Combined with variable station coverage, these two factors make it difficult to select one appropriate smoothing parameter for the entire region using traditional techniques. B-strain handles it more adaptively through sampling of posterior PDFs.

The authors present the results of strain rate in the Eastern Mediterranean and interpret the resulting strain rates. Most of the tectonic features of the results are quite similar to previous studies, but the new results have somewhat more robust boundaries between tectonic provinces without the need to manually impose them before the analysis. Certain areas that have been previously assumed to have uniform strain rates, like the Apennines, are revealed to have higher-order variations in their strain fields. The authors have also devised some clever web viewers to visualize the full PDFs of strain tensor quantities.

This is a great paper about an important topic and a very complex region where better strain rates could enable new science on several fronts. It should be published soon with very minor revision.

Minor points:

Why are the references listed in such an order? We have a reference to [40] in the first paragraph because the references are listed and numbered in alphabetical order while they appear disordered when they are called out.

Line 214: “remarkably well” is subjective language; you can just remove those two words and the meaning of the sentence is basically the same.

Line 268: This is about half way through the paper. This is too early to be using the word “Conclusion” in a heading.

Line 316: Is “Inverse” supposed to be “reverse”?

Line 322: “EFSM” is used several times including in Figure 1, but never defined

In general, the paper has a lot of place names. I think this is okay, but it can get challenging to read if you’re not familiar with those places. Perhaps some of the more obscure of them, such as the Moesian plain, could be replaced with other descriptors or labeled. I don’t think it’s possible to label all of them. Figure 1: I guess this should have a scale bar somewhere; just one is enough for the whole paper.

Figure 2C: Why not truncate the color scale and contour more finely?  
Currently the only places with resolvable data and contours are offshore,  
which is not interesting.

Figure 3: A lot of the offshore areas with low confidence have the highest  
strain rates of the entire figures. Can you explain this in the text? What does  
“MOY” mean?

## Comments by Reviewer 3

Review of Metois et al., 2025, Tektonica

**Reviewer 3 (We have pasted below their comments as it has been uploaded on Tektonica web site. However, there are some strange formatting. We hope you will understand...)**

This paper uses the Bstrain method presented by Pagani et al. (2019) to the area of southeastern Europe. As such, this is a straightforward paper with nice results. I have no comments about the method or results but I do have some comments about the presentation.

I don't understand the adopted way to express the style of the strain rate tensor:  $0.75 + \text{atan}(e2/e1)/\pi$  (credited to Greg Houseman). The legend in figure 7 suggests it goes from 0 (reverse) to 1 (normal), but that is not what I get when plugging in some values for  $e1$  and  $e2$  (with, by definition,  $e1 \geq e2$ ):

e1  
e2  
houseman  
kreemer  
1  
0  
1.25  
1  
1  
-1  
1.5  
0  
0  
-1  
1.75  
-1  
1  
1  
1  
2  
-1  
-1  
0  
-2

I also added the definition used in the Global Strain Rate Map (Kreemer et al., 2014):  $(e1+e2)/\max(|e1|,|e2|)$

The Kreemer definition goes from 1 (normal) to 0 (strike-slip) to -1 (reverse) (it exceeds the -1:1 bounds when both axes are the same sign, and this can be truncated). However, the Houseman definition goes from 1.75 (reverse) to 1.5 (strike-slip) to 1.25 (normal), and becomes smaller when both axes have the

same sign. This is not only non-intuitive but also not what Figure 7 shows. What am I missing?

A comparison is made between style of strain rate tensor and focal mechanisms. However, looking at Figs 1 and 7, there is no depth distinction being made. I base this on the figures showing a cluster of events in the Vrancea zone, which I believe are mostly deep. If I am not wrong, please limit the plots and the comparison to shallow events only.

Minor comments:

Line 122-123 – this sentence needs rewriting to make it clearer and grammatically correct

Line 290 – “and heavy” seems like a redundant descriptor

Line 326 – “happens there” should be “exist there”

Line 388 – What is meant with the “(?)”. Are you expressing that you are speculating? You should simply state that explicitly

Line 429 – another “(?)”. I am starting to think that this is a formatting error instead. Another question mark is on line 430 and 434

Line 452 – You might want to add Nyst and Thatcher (2004) <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2003JB002830>

Line 464 – similarly, you might consider adding papers by the Le Pichon group:

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2012GC004289>

<https://www.annualreviews.org/content/journals/10.1146/annurev-earth-040809-152419>

<https://academic.oup.com/gji/article/157/3/1377/2110555>

<https://www.sciencedirect.com/science/article/pii/S0012821X04004078>

Line 474 – You might also want to add studies that have compared strain rates to earthquake numbers, or made forecasts, or constrained maximum expected seismic moment. For example:

<https://academic.oup.com/gji/article/205/1/509/2594847>

<https://academic.oup.com/gji/article/157/3/1331/2110266>

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014JB011751>

<https://doi.org/10.1016/B978-0-323-95507-2.00010-4>

Reference 14 is a 2019 submitted paper. Has it been published?

Figure 1 – The text in the legends of Figure 1 is too small. Also, indicate the depth extent of the earthquakes in Fig 1c (also in Fig 7). See my comment about depths above.

Figure 1b – Does this include the outlier velocities? If yes, perhaps give them a different color.

Figure 2b – The legend suggests that the GNSS locations are plotted but I do not see them.

Figure 7 - The number labeling of the style in Figure 7 is also much too small. Finally, opposite to this paper, I personally prefer red colors for extension/normal faulting and blue for contraction/reverse faulting, but I have not done a survey of what the dominant choice is in our research community.

Authors' Reply to Reviewers (27 March 2025)

**The authors have provided their rebuttal as a comprehensive PDF document. It is attached in the following pages.**

**Dear editorial team,**

We are glad to resubmit our paper for consideration to Tektonika's editorial team after this round of reviews. We would like to thank the three reviewers who took time to give interesting feedback and comments on this study, in general positive. In the new version of the manuscript, we took into account most of their comments regarding missing details, or unclear writing. We have tried to make the discussion simpler and have paid attention to correctly cite previous works. However, we could not conduct the detailed comparison asked by reviewer 1 with the results coming from Serpelloni et al. 2022 paper since the supplementary material appears corrupted. We are in contact with the authors to get their data, but this may take some time and we don't think it will deeply change our results. In the manuscript, we conduct a detailed comparison with results coming from a very widely used inversion method, VISR, which is representative of the issues existing when using spatially varying smoothing approaches. Based on reviewers' suggestions, we have added 4 new supplementary figures in order to describe the fit to the data of the median velocity field (figure S1), the median velocity field itself (figure S2) and profiles of the PDF for  $V_{east}$  and  $V_{north}$  in figures S5 and S6. We provide a revised version of the manuscript where changes are highlighted in blue, together with a detailed response to the reviewers' comments.

We have published the full data set on the Easy Data platform, under the DOI <https://doi.org/10.57932/75c45007-1832-4d07-ad9f-cefe5dc1119d>. Maps of the results can also be downloaded from the Bstrain Plotter webtool as geotiffs (<https://bstrainplotter.univ-lyon1.fr>).

We used the Latex Tektonika template to write this manuscript, compiled on the Overleaf tool, but apparently some problems remain in the bibliography style that we hope could be resolved in future editing steps.

We hope that this new version of the manuscript is now suitable for publication in Tektonika.

Marianne Métois,  
On behalf of the coauthors.

**Answer to reviewers :**

\*\*\*\*\* Reviewer 2 \*\*\*\*\*

This paper applies a recently published Bayesian technique (B-strain) for determining strain rates from geodetic velocities to data from the Eastern Mediterranean and the Balkans. This area presents major seismic hazards and yet parts of it are frequently viewed as a part of the stable European continent from a GNSS strain point of view. Typical strain rate methods require smoothing to be defined by the user, but this would pose a challenge in the Balkans because certain areas are very active shear zones (like the North Anatolian fault), others are diffuse deformation zones, while others are relatively more rigid. Combined with variable station coverage, these two factors make it difficult to select one appropriate smoothing parameter for the entire region using traditional techniques. B-strain handles it more adaptively through sampling of posterior PDFs.

The authors present the results of strain rate in the Eastern Mediterranean and interpret the resulting strain rates. Most of the tectonic features of the results are quite similar to previous studies, but the new results have somewhat more robust boundaries between tectonic provinces without the need to manually impose them before the analysis. Certain areas that have been previously assumed to have uniform strain rates, like the Apennines, are revealed to have higher-order variations in their strain fields. The authors have also devised some clever web viewers to visualize the full PDFs of strain tensor quantities.

This is a great paper about an important topic and a very complex region where better strain rates could enable new science on several fronts. It should be published soon with very minor revision.

We would like to thank the reviewer for these encouraging comments.

Minor points:

Why are the references listed in such an order? We have a reference to [40] in the first paragraph because the references are listed and numbered in alphabetical order while they appear disordered when they are called out.

We used the bibliography settings in the Latex template provided by Tektonika. This may change during final editing.

Line 214: “remarkably well” is subjective language; you can just remove those two words and the meaning of the sentence is basically the same.

Agreed and modified.

Line 268: This is about half way through the paper. This is too early to be using the word “Conclusion” in a heading.

Agreed and modified : we now propose a short conclusion and perspective section at the end of the paper.

Line 316: Is “Inverse” supposed to be “reverse”?

Yes, indeed. Modified

Line 322: “EFSM” is used several times including in Figure 1, but never defined

This has been specified in the caption of figure 1.

In general, the paper has a lot of place names. I think this is okay, but it can get challenging to read if you’re not familiar with those places. Perhaps some of the more obscure of them, such as the Moesian plain, could be replaced with other descriptors or labeled. I don’t think it’s possible to label all of them.

Yes, indeed, we reckon that the numerous names make the reading fastidious in some places, however, we cannot overload Fig1 presenting the overall context. We added some labels in the topographic profiles of figures 5 and 6 (and fault names on strain invariants profiles), to help the reader. We added the “mp” label in figure 1 standing for Moesian platform. In general, we have shortened the discussion section as suggested by reviewer 1.

Figure 1: I guess this should have a scale bar somewhere; just one is enough for the whole paper.  
Done on fig 1a.

Figure 2C: Why not truncate the color scale and contour more finely? Currently the only places with resolvable data and contours are offshore, which is not interesting.  
We now truncate the color scale to 8mm/yr in order to see more clearly the variations of resolution inland.

Figure 3: A lot of the offshore areas with low confidence have the highest strain rates of the entire figures. Can you explain this in the text? What does “MOY” mean?  
Moyenne “moy”, is the french for average, this has been changed. Where there is no (or few) constraint on the strain rates, we expect the code to explore the entire range of a priori values for velocity, divergence, strain rates etc.

Regarding your comment on highest strain rates in poorly constrained zone, we propose to add the following paragraph in the main text (end of section 3.1):

“Note that the offshore areas with poor data coverage depict the highest strain rates. This is because we parametrize and invert for velocities rather than their spatial derivatives (i.e. strain). In the regions where no data is available, the posterior probability distribution on velocities is similar to the prior. The Delaunay nodes then take completely random velocity values between  $V_{min}$  and  $V_{max}$ , without any spatial consistency, resulting in a white noise on velocity and producing extreme spatial derivatives, and thus high strain levels. In this way, while the posterior mean on velocities is zero, the posterior mean on strain quantities takes large values.”

\*\*\*\*\* Reviewer 3 \*\*\*\*\*

(We have pasted below their comments as it has been uploaded on Tektonika web site. However, there are some strange formatting. We hope you will understand)

This paper uses the Bstrain method presented by Pagani et al. (2019) to the area of southeastern Europe. As such, this is a straightforward paper with nice results. I have no comments about the method or results but I do have some comments about the presentation.

I don't understand the adopted way to express the style of the strain rate tensor:  $0.75 + \text{atan}(e2/e1)/\pi$  (credited to Greg Houseman). The legend in figure 7 suggests it goes from 0 (reverse) to 1 (normal), but that is not what I get when plugging in some values for  $e1$  and  $e2$  (with, by definition,  $e1 \geq e2$ ):

e1	e2	Houseman	Kreemer
1	0	1.25 0.75	1
1	-1	1.5 0.5	0
0	-1	1.75 0.25	-1
1	1	1	2
-1	-1	0 1	-2

I also added the definition used in the Global Strain Rate Map (Kreemer et al., 2014):  $(e1+e2)/\max(|e1|,|e2|)$

The Kreemer definition goes from 1 (normal) to 0 (strike-slip) to -1 (reverse) (it exceeds the -1:1 bounds when both axes are the same sign, and this can be truncated). However, the Houseman definition goes from 1.75 (reverse) to 1.5 (strike-slip) to 1.25 (normal), and becomes smaller when both axes have the same sign. This is not only non-intuitive but also not what Figure 7 shows. What am I missing?

When computing the  $s$  factor as defined by Houseman and England, 1986, we find different results than the reviewer, as shown in yellow in the table. The  $s$  factor ranges from 0.25 (reverse) to 0.75 (normal), and 0.5 for strike-slip, which corresponds to the color scale of figure 7. When both axes have the same sign,  $s$  is close to 1.

$$s = \frac{3}{4} + \frac{1}{\pi} \tan^{-1}(\epsilon_2/\epsilon_1)$$

A comparison is made between style of strain rate tensor and focal mechanisms. However, looking at Figs 1 and 7, there is no depth distinction being made. I base this on the figures showing a cluster of events in the Vrancea zone, which I believe are mostly deep. If I am not wrong, please limit the plots and the comparison to shallow events only. Fair enough. We plot all the catalog in figure 1-C (overall seismotectonic context) in order to keep the subduction signature. We discuss the Vrancea cluster briefly at lines 327-328. However, we reckon that for supplementary figure S9, we could restrict the depth range. This is what we have done in the new version considering only events that occur at depths lower than 50km, which does not change the overall picture. For consistency with the surface tectonic styles and clarity in figure 7, we have removed the focal mechanisms that were too small to be correctly depicted.

Minor comments:

Line 122-123 – this sentence needs rewriting to make it clearer and grammatically correct

We propose this new phrasing : *“As in California where we used the MIDAS data set to invert for strain rates (45), we parametrize B-Strain so that the formal uncertainties given by (48) could be increased by up to 10%.”*

Line 290 – “and heavy” seems like a redundant descriptor

agreed.

Line 326 – “happens there” should be “exist there”

agreed.

Line 388 – What is meant with the “(?)”. Are you expressing that you are speculating? You should simply state that explicitly

Line 429 – another “(?)”. I am starting to think that this is a formatting error instead. Another question mark is on line 430 and 434

Yes, indeed, references were badly inserted in the text, sorry for that, we corrected the problem in the new version.

Line 452 – You might want to add Nyst and Thatcher (2004)  
<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2003JB002830>

Line 464 – similarly, you might consider adding papers by the Le Pichon group:

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2012GC004289>

<https://www.annualreviews.org/content/journals/10.1146/annurev-earth-040809-152419>

<https://academic.oup.com/gji/article/157/3/1377/2110555>

<https://www.sciencedirect.com/science/article/pii/S0012821X04004078>

Line 474 – You might also want to add studies that have compared strain rates to earthquake numbers, or made forecasts, or constrained maximum expected seismic moment. For example:

<https://academic.oup.com/gji/article/205/1/509/2594847>

<https://academic.oup.com/gji/article/157/3/1331/2110266>

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014JB011751>

<https://doi.org/10.1016/B978-0-323-95507-2.00010-4>

Thank you for these valuable suggestions to enrich the references. We have added most of them in the revised version of the manuscript.

Reference 14 is a 2019 submitted paper. Has it been published?

Yes, the bibliography has been corrected accordingly.

Figure 1 – The text in the legends of Figure 1 is too small. Also, indicate the depth extent of the earthquakes in Fig 1c (also in Fig 7). See my comment about depths above.

Figure 1b – Does this include the outlier velocities? If yes, perhaps give them a different color.

We represent on figure 1b the so-called “cleaned” velocity field proposed by Pina-Valdes et al (as stated in the caption). Outliers should have been removed.

Figure 2b – The legend suggests that the GNSS locations are plotted but I do not see them.

Yes indeed, this was a mistake. The figures have been corrected to plot again GNSS stations.

Figure 7 - The number labeling of the style in Figure 7 is also much too small

Figure 7 has been enlarged.

Finally, opposite to this paper, I personally prefer red colors for extension/normal faulting and blue for contraction/reverse faulting, but I have not done a survey of what the dominant choice is in our research community.

Neither do we, but it appears quite non consensual. We keep to our initial choice which is also consistent with the previous papers published by the group (in particular Métois et al. 2015).

# Tektonika peer-review form

## Section A: Overview of manuscript

### A1) Overall evaluation, general comments & summary

#### A1.1) Reviewer's comments

##### **A1.1.1 ) General evaluation and publication suggestion – Required:**

*Please use this space to describe, in your own words, the core subject of the submission and your overall assessment of its suitability for publication.*

The manuscript investigates surface strain rates from GNSS velocities in the peri-Adriatic, Balkans, and Aegean regions using Bayesian trans dimensional inversion, implemented in the B-Strain software, discussed in detail in a previous paper from Pagani et al. (2021). This is a valuable contribution as it provides refined insights into surface deformation in a tectonically active area characterized by complex strain heterogeneities. The methodology, employing a probabilistic approach to address limitations of traditional techniques, such as interpolation artifacts and user-defined smoothing, provides probabilistic maps of strain rate invariants, divergence, vorticity, providing a thorough analysis of surface deformation. These results are compared with those obtained adopting a widely used method, VISR3, on the same GNSS velocity dataset, presented in a previous paper from Pina-Valdes et al. (2022). Importantly, the authors make their results accessible via a web tool and archive, enhancing transparency and reusability. The results highlight tectonic features (e.g., segmentation of the Apennines, contrast between Nubian and Tyrrhenian domains, extent of the South Balkans Extension Region) and their implications for regional seismic hazard assessments.

However, some sections are overly detailed, particularly in the methodology and result discussions. A more concise narrative would enhance readability. More importantly, the distinction between new insights and confirmation of previously known features is not always clear, and I think that highlighting novel contributions would improve the impact of this paper.

While the study is methodologically sound and offers valuable insights, improvements in clarity, conciseness, and the provision of additional methodological details would enhance its impact and accessibility. I recommend the manuscript for publication after minor revisions addressing the points mentioned above.

##### **A1.1.2 ) What does the submission need to be publishable? (select as needed; comment for all cases)**

- No changes required
- Rewriting
- Reorganising
- More data/figures

- Condensing
- Reinterpretation
- Other

**Comments:**

Some sections are overly detailed, particularly in the results and discussions sessions. A more concise narrative would enhance readability. The distinction between new insights and confirmation of previously known features is not always clear and I think that highlighting novel contributions would improve the impact of this paper.

While the manuscript compares B-Strain results with VISR3D ones, it could better emphasize the practical implications of the observed differences (e.g., for seismic hazard modelling or geodynamic interpretations). Moreover, the authors limit the comparison with VISR3, but the same area has been investigated with other methods, that, in some cases, seems to overcome limitations of the VISR3 approach. For example, the multiscale approach described in Tape et al. (2009), which adopts a spherical wavelet representation of the continuous velocity field, seems to provide an efficient and objective way to generate a continuous velocity field from discrete velocity measurements, with limited user choices. Maurer and Materna (GJI, 2023) discuss the results from this method with results from the application of more “traditional” methods, including VISR3, for western U. Serpelloni et al. (2022) compare VISR3 with multiscale strains for about the same area investigated in this manuscript. I think that for some sectors, also discussed in this manuscript, a comparison with results from other methods should be considered. Clearly, there might be some difference in the GNSS velocity datasets, and these comparison with expanded literature are feasible only where the density of stations is comparable.

Figures, particularly those illustrating strain rate maps and tectonic profiles, need better labels and clarity. Some readers may find it challenging to interpret without clearer legends or annotations. Certain technical terms and methodological steps (e.g., parameterization choices, hyperparameter scaling) require more explanation for accessibility to a broader audience.

**A1.1.3) Can the submission be improved by reducing/adding any of the following? (select as needed; comment for all cases)**

- Text
- Table
- Figures
- Supplementary material

**Comments:**

Some sections, particularly in the methodology and results, could be more succinct to enhance readability.

Ensure all figures are well-labeled and easily interpretable, possibly by adding more descriptive captions or annotations. See specific comments on figure section of this review form.

**A1.1.4) Please complete the following section if you recommend that the submission is NOT appropriate for publication (select as needed; comment if a box is selected)**

- Quality is poor
- Research is not reproducible
- Other

**Comments:**

[Free form box]

**A1.2) Author(s) Responses:**

We would like to thank the reviewer for his careful reading and his interesting suggestions. In the new version of the manuscript, we have taken some suggestions into account to make the reading easier.

A major point of the reviewer is to provide a better comparison with a previous work from Serpelloni et al. 2022, who computed strain rates over a much broader area and a different GNSS data set. We indeed share the scientific motivation of these authors in understanding current day deformation in the area, and we would have liked to provide a quantitative comparison with the wavelet based approach they use. However, we have failed in getting the data from the supplementary material that is unfortunately incomplete, which strengthens the need for the community of making data and result calculations FAIR.

We have completed our discussion on strain rate estimates strategies (section 2.2) by referring to the wavelet approach proposed by Tape et al. 2009. We now add at lines 103-106 this sentence “Wavelets decompositions (61; 59) other approaches to manage varying smoothing but remain highly dependent on the network density and regularization choices”. Indeed, as discussed later in this answer, the wavelet approach is a smart way to deal with heterogeneous GNSS velocity fields but it is not free from regularization and it does not provide uncertainties on the strain rate tensor, at least not directly. The latter are estimated by Serpelloni et al. 2022 by taking the minimum and maximum values given by each wavelet inversion. The B-strain method allows for estimating the uncertainties on strain rate invariants based on their full PDF that may be useful to assess geodetic moment rates for instance or/and to be included in PSHA logic trees.

As stated by the authors “The density of the geodetic points controls the selection of spherical wavelets”, and therefore the local roughness degree, whereas in the transdimensional approach, we aim at having more complexity where it is needed by the data, independently of their density (as illustrated in fig 2b for instance). You can have very dense velocity fields in places where no significant strain is occurring (in the Paris basin for instance).

## **A2) Summary of main merits and main points of improvement**

### **A2.1) Reviewer's comments**

*Please describe below in a few sentences (100 to 300 words) the main merits of the submission and suggestions for improvements.*

The probabilistic approach effectively addresses the limitations of traditional techniques, such as interpolation artifacts and reliance on user-defined smoothing parameters. By providing probabilistic outputs, the study quantifies uncertainties, enhancing confidence in the results. It identifies key tectonic features and explores their implications for regional seismic hazard assessments, offering valuable insights to the field. Furthermore, the authors promote transparency and accessibility by sharing their results via a web-based visualization tool and an online repository. However, the comparison with the VISR3D method could be more detailed, particularly regarding the practical significance of the observed differences, such as their impact on seismic hazard or geodynamic models. Additionally, the discussion could benefit from a more in-depth exploration of how these findings can be applied in practical scenarios, including seismic hazard assessment and geodynamic modeling.

### **A2.2) Author's responses:**

[Free form box]

We agree with the reviewer that the strain rate calculations proposed in our study could be used to refine geodynamical models and seismic hazard estimates in some places. We do discuss this in section 5 (now the conclusion of this paper), but going further in the modeling is beyond the scope of this study.

## Section B: Detailed evaluation of manuscript

### B1) Title and abstract

#### B1.1) Reviewer's comments

*These statements are a **guide** to what good Titles and Abstracts include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Title* describes the main topic of the manuscript **accurately** — [YES]

The *Title* describes the main topic of the manuscript **succinctly** — [YES]

The *Title* includes **appropriate key terms** — [NO]

The *Abstract* includes a **clear aim and rationale** — [YES]

The *Abstract* supports the rationale with **sufficient background information** — [YES]

The *Abstract* includes a **well-balanced description of the methods** — [YES]

The *Abstract* describes the **main results sufficiently and adequately** — [YES]

The *Abstract* clearly describes the **importance/impact of the study** — [YES]

The *Abstract* clearly states the **conclusions of the study** — [YES]

The *Abstract* is **clear and well structured** — [YES]

#### **Comments:**

I suggest including the source of data, GNSS velocities, in the title. As regards the abstract, I suggest to more clearly specify what the authors mean with results being “more robust” than the those produced from “standard” techniques. How is robustness defined?

#### B1.2) Author's responses

[Free form box]

We modified the title to include GNSS velocities “Bayesian estimation of surface strain rates in the peri-Adriatic, Balkans and Aegean region from GNSS velocities”. By “more robust” we mean less subject to artifacts coming from interpolation issues, and not depending from regularization choices. We now precise this in the abstract.

## B2) Introduction

### B2.1) Reviewer's comments

*These statements are a **guide** to what good Introductions include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Introduction* provides **sufficient background and context** for the study — [YES]

The *Introduction* describes the **aim/hypothesis/rationale** clearly, providing **sufficient context** — [YES]

The *objective/hypothesis/rationale* **flows logically from the background** information — [YES]

The *Introduction* describes the study's **objective and approach** (last paragraph) — [YES]

The *Introduction* contains **relevant, suitable citations** — [YES]

The *Introduction* is **organized effectively** — [YES]

#### **Comments:**

[Free form box]

### B2.2) Author's responses

[Free form box]

## B3) Data and methods

### B3.1) Reviewer's comments

*These statements are a **guide** to what good Method sections include and good practices for Dataset accessibility. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Methods* are described **concisely and with enough detail** for reproducibility — [NO]

Necessary information about **data sources/acquisition/processing** is included — [YES]

**Data used are accessible** via either supplementary files or links in the data availability statement — [YES]

The *Dataset and/or Methods* are **organized effectively** — [YES]

#### **Comments:**

The GNSS velocity dataset is clearly identified, with appropriate references to prior studies and an online repository providing access to the cleaned and filtered data. The methods are described concisely and with sufficient detail to allow for reproducibility. The probabilistic approach is well-articulated, particularly in its ability to address limitations of traditional methods, such as interpolation artifacts and fixed smoothing parameters. Additionally, the results are made accessible via a web-based visualization tool and an archived repository, enabling users to interact with the probabilistic strain rate maps. It would be helpful for the authors to elaborate on what they mean by “traditional” methods and clarify how they classify strain-rate inversion methods as either traditional or non-traditional. For instance, VISR3D is referred to as a “traditional” method due to its reliance on a fixed level of smoothing, but other methods, such as those proposed by Tape et al. (2009), adapt smoothing parameters to the density of GNSS stations. Are these adaptive methods considered traditional or non-traditional? The manuscript would benefit from a more precise discussion of this classification.

Although the use of Delaunay triangulation and hyperparameter inversion is mentioned, the manuscript does not adequately explain how parameterization choices, such as node density or bounds, were determined, nor does it discuss how these decisions influence the results. Similarly, the rationale behind applying a 10% uncertainty scaling remains insufficiently detailed, which could hinder reproducibility. While the results are accessible via a web tool, a user seeking to reproduce the findings might encounter challenges due to the lack of a detailed workflow. Since this is a methodological paper, it would be valuable to provide a step-by-step guide for applying the B-Strain code, either as supplementary material or in an online resource. This addition would greatly enhance the accessibility and usability of the methodology for the research community.

### B3.2) Author's responses

We fully agree with the reviewer: the results have to be reproducible. This is why we do provide an online documentation for the code, including a step by step guide to use the code (that remains however far from user friendly since it requires parallelization) <https://bstrain.readthedocs.io/en/latest/about.html>

In the online repository (<https://www.easydata.earth/#/projects/d217746b-f51d-4911-af1f-2e85a4710dd0>), we do provide the full parameters.in file in order to use the exact same parametrization on other machines. The bounds applied to the uncertainties scaling factor, here chosen to be 10% max, can be adjusted in this parameters.in file. We rephrase this description in the new version of the manuscript (l.124-127).

[Free form box]

## B4) Results

### B4.1) Reviewer's comments

*These statements are a **guide** to what good Result sections include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Results* findings are **supported by data** — [YES]

The *Results* findings are presented **clearly and succinctly** — [NO]

The text in the *Result* section **cites tables and figures appropriately** — [YES]

The *Results* directly **relate to the study objectives** — [YES]

The *Results* present **data for all the approaches** described in the *Methods* section — [YES] / [NO]

The *Results* **text belongs to the Results section**, not to *Introduction*, *Methods*, or *Discussion*. — [YES] / [NO]

The *Results* section is **organised effectively** —[NO]

### Comments:

Overall, the findings are well-supported by the data, with strong connections between GNSS-derived inputs and the computed strain rate patterns. The use of a probabilistic framework enhances the credibility of the results, and the key features align with established geodynamic knowledge. However, there is room for improvement in the clarity and presentation of the findings. This includes refining overly detailed sections, improving figure labels and annotations, and placing greater emphasis on novel contributions. Additionally, providing more accessible quantitative comparisons would make the results more impactful for the geoscience community. Enhanced visualization (e.g., as outlined in section B6) and an expanded discussion of limitations would further strengthen the overall presentation. Although the study discusses practical applications, these are not explored in sufficient depth or supported with specific examples. To improve this connection, the authors could elaborate on how the probabilistic findings enhance geodynamic models or, for instance, contribute to refining seismicity rate estimations constrained by geodetic data models. This would highlight the broader relevance of the results. The results are presented as maps and cross sections, with cross sections including comparisons to VISR3 results from Pina-Vidales et al. (2022). However, the inclusion of a modeled continuous velocity field would be highly beneficial. Since strain rates concentration represent at first order the steepness of horizontal velocity gradients, showing velocity profiles (e.g., profile-parallel velocities) would provide

additional context. It would be valuable to evaluate how the B-Strain modeled continuous velocities align with the observed velocities, offering a more complete picture of the model's performance.

## **B4.2) Author's responses**

Following reviewer's suggestions, we propose now in supplementary figure 2 maps of average velocity. These are downloadable on the Bstrain Plotter webtool as geotiffs. We also plot in the new supplementary figure 1 the residuals between the median of the interpolated velocity field and the observed velocities. We propose profile views of East and North components of the velocity field on figures S5 and S6. It is not trivial to get the full PDF of velocity components in ad-hoc directions such as along-strike or across-strike since it cannot be equivalent to a linear combination of PDF for East and North components. To do so, you have to implement the strike of the velocity direction directly in the code. Since we chose profile lines that have very different strikes, we think these plots are sufficient at the first order for the reader. These profile views show that velocities are much less uncertain than strain rates, as discussed in the text (i.e. PDF are very narrow).

Regarding the suggestion to provide more quantitative comparison with other solutions, we find it difficult to go further than what is actually proposed, i.e. scatter plots in fig. S1 for divergence, invariant and vorticity, map views of residuals in fig S2. Histograms of the residuals would be difficult to interpret since the residual value highly depends on the location considered. We agree that extensive comparisons with other solutions, including Serpelloni et al. 2022, on specific targets, would be interesting, but we failed in recovering the strain rate grid from the supplementary material for the moment. We also chose not to compare our findings with the calculation from Métois et al. 2015, since we are not using the same GNSS data set.

[Free form box]

## B5) Discussion and conclusions

### B5.1) Reviewer's comments

*These statements are a **guide** to what good Discussions and Conclusions include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Discussion* is **focused on the objectives** of the study — [YES]

The *Discussion* **addresses all major results** of this study, which are shown in *Results* — [YES]

The *Discussion* section makes **comparisons with other studies** that are relevant and informative — [NO]

The *Discussion* section properly identifies all **speculative statements** — [YES]

The *Discussion* section presents the **implications of the study** persuasively — [NO]

The *Discussion* section **highlights novel contributions** appropriately — [NO]

The *Discussion* section **addresses the limitations** of the study appropriately — [NO]

The *Discussion* section is **organised effectively** — [YES]

The *Conclusions* are **consistent** with and **summarise** the rest of the manuscript — [YES]

The *Conclusions* are **supported by the data** in *Results* and **follow logically** from the *Discussion* — [YES]

The *Conclusions* are **clear and concise** — [YES]

### **Comments:**

The discussion aligns with the study objectives, focusing on evaluating the methodological advancements and linking the results to geodynamics and regional kinematics. However, it could better emphasize the novel contributions of B-Strain compared to confirmations of known strain-rate field features derived from other methods. The discussion currently centers on comparisons with results from Pina-Valdes et al. (2022) using the VISR3 approach, but the study area has been analyzed in numerous other papers. While a comprehensive comparative analysis, as done in Maurer and Materna (2023) or Nucci et al. (2024), is not strictly requested (but would be really interesting) the discussion could be significantly improved if the authors compare their findings, especially in areas where B-Strain outperforms VISR3 in resolving sharp gradients (e.g., the North Anatolian Fault), with results from studies employing alternative approaches (in particular Tape et al., 2009).

Although the discussion highlights some novel contributions, such as the advantages of B-Strain and the improvements in strain rate mapping, it does not sufficiently emphasize the significance of these findings. To better showcase the novelty, the authors should clearly distinguish between new insights and confirmations of previous work. Contextualizing the novel contributions within the broader fields of geophysics and seismic hazard research would strengthen the narrative. Specific examples of how B-Strain results advance practical applications (e.g., seismic hazard modeling) or geodynamic interpretations would also enhance the discussion.

The discussion appropriately addresses several limitations, particularly those related to sparse GNSS coverage and data heterogeneity. However, certain areas need further attention: 1) provide a more detailed analysis of how sparse data and parameterization choices affect the results; 2) compare B-Strain with other advanced methods (beyond VISR3, which can be considered a standard) to present a more balanced view of its strengths and weaknesses; 3) offer potential solutions or future directions to address data accessibility issues, such as the lack of raw GNSS data in certain regions. Addressing these points would not only improve the clarity and comprehensiveness of the discussion but also enhance the study's impact by demonstrating the broader relevance of B-Strain in geophysical research and practical applications.

## **B5.2) Author's responses**

[Free form box]

- As stated before, we chose to compare our results with published calculations made on the same exact data set, in order to get rid of the inherent bias of using distinct initial data sets. The VISR3D method is widely used in the community and is therefore broadly representative of existing issues in methods using adaptive smoothing. However, we reckon that broader comparisons with other results would be interesting, in particular when geodetic strain rates are thought to enter into PSHA logic trees. This kind of intercomparison requires each strain rate calculation to be published and reusable, which is not the case for many papers, including the one mentioned by the reviewer. We now dedicate a full section to discuss the potential applications for geophysics and seismic hazard assessment of our results (section 5), without entering in the details of modeling since it appears out of the scope of this study. We refer explicitly now to PSHA calculations, and stress the fact that for this very specific application, many other calculations have to be considered.
- In the discussion we pay more attention to citing previous work and identifying new findings.
- The full B-Strain method is described in Pagani et al. 2021 who provide tests on network density and explain in detail how the parametrization choices allow us not to impose any node geometry. We add a sentence in the main text to recall that nodes do not have to be on data points.
- We address the problem of data access in section 2.1 (l.73-75) and in section 4.1 (lines 327-330).

## B6) Figures, tables and citations

### B6.1) Reviewer's comments

*These statements are a **guide** to what good Figures and Tables include and how they are presented. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

Tables and Figures are **ordered logically** and **numbered sequentially** — [YES]

Tables and Figures have **captions that explain** all their major features — [YES]

Tables and Figures have **captions that complement** the information in the main text — [YES]

Tables and Figures present data that **relate** to the study objective — [YES]

Tables and Figures present data that are **consistent** with and support the description of results — [YES]

Tables and Figures have **succinct and informative titles** — [YES]

Figures are **accessible** (elements are clearly labelled, accessible colour palettes, colour contrasts, font size legible, etc....) — [NO]

Please, check our [[Figure guidelines](#)]

Figures with **maps or cross-sections** contain all **elements to be understood** (north arrow orientation, scale, visible coordinates, sufficient coordinate grid intercepts) — [NO]

Figures with **maps** have **sufficient location information** (in the map or caption) — [YES]

Cross-sections have clear labels for **scale and coordinates** at ends and within-section kinks — [NO]

All georeferenced elements are provided in common format (.shp, .geotiff, .kml) [in an open-access repository] — [YES]

Citations throughout are relevant, suitable, and comprehensive — [YES]

### Comments:

Figure 1: tracks of profiles are not clearly visible. What about plot these profiles in Figure 4?

The profiles are now more clearly visible on fig.1, we prefer not to overload fig 4.

Figure 2: the symbols representing GNSS stations are not visible in panel B.

Corrected

Figure 3: increase green line thickness.

Done.

Figure 4: increase green line thickness. I suggest adding a label to the black arrows for features discussed in the maintext.

Agreed and done.

Figure 7: the colours representing tectonic style from EFSM are indeed not easily visible in the panel B. Focal mechanisms are already shown in Figure 1, so I suggest remove FM from Figure 7 and increase

the fault line thickness (possibly on top of the layers).

Based on a comment of another reviewer, we chose to remove the focal mechanisms from Figure 7, which should make the figure clearer. We increase the fault line thickness to make their style more apparent.

## **Section C: Additional comments**

### **C1) Minor/line-numbered comments**

#### **C1.1) Reviewer's comments**

Page 8 Line 315: I think it is Figure S3 not S2.

Page 9 Line 388: remove “?”

Page 10 Line 429: remove “?”

Page 10 Line 430: remove “?”

Page 10 Line 434: remove “?”

#### **C1.2) Author's responses**

Agreed and corrected

### **C2) Other remarks**

#### **C2.1) Reviewer's comments**

[Free form box]

#### **C2.2) Author's responses**

## 2<sup>nd</sup> Round of Revisions

### Decision Letter (8 April 2025)

Dear Marianne Métois and co-authors,

Thanks for submitting a revised version of your manuscript. After reading this new version and the answers to the reviewers' comments, we concluded that your manuscript is now in principle ready to be accepted for publication in Tektonika. However, before formally validating the "accept" decision, we will ask you to upload a new revision that will include the comparisons conducted with the Serpelonni et al (2022) strain rate maps. We think this new analysis would be good to include in your paper. You may include it in the core of the manuscript or in the supplement, at your convenience. In any case you will have to add a few additional sentences describing these results. By the way, you should also correct these very minor points noted by Franck: "I note too that in the revised manuscript, there is some inconsistency between describing 'reverse' and 'inverse' faulting (e.g., Figure 7 caption, Figure S9) that would be good to rectify. Also, 'focal mechanisms' should be removed from the title of Figure 7 as they're no longer depicted in that figure."

Also, to help for the next step (copy-editing), I suggest you upload a clean manuscript (without changed marked) both in PDF and Latex, with the references in a Bibtex file. You should also separate the supplement from the core manuscript and provide these supplementary information as a separate PDF document properly formatted. There will not be copy-editing on the suppl., so you need to generate your own final document. As this is a very minor revision step, we hope you will be able to upload it within 2 weeks at most.

All the best, Robin Lacassin, Tektonika executive editor  
Franck Williams, Tektonika associate editor

### Authors answer (9 April 2025)

Dear Robin and Franck,

We are glad that our modifications make the manuscript suitable for publication. We have just uploaded a new version of the manuscript that now includes the comparison with Serpelonni's work in supplementary figure 5 and references associated in the main text. We also corrected the inconsistencies arose by Franck. We have uploaded the original figures as a zip file.

I am looking forward to see this work published in Tektonika !

Best, Marianne, for the coauthors

## Acceptance Letter (14 April 2025)

Dear Marianne Métois, Cécile Lasserre, Aïmine Méridi, Maxime Henriquet, Thomas Bodin:

We have reached a decision regarding your submission to *tektonika*, "Bayesian estimation of surface strain rates in the peri-Adriatic, Balkans and Aegean region".

Our decision is to: Accept Submission