

Dear Editor and Reviewers,

Thank you for considering our manuscript entitled “*Persistent Deformation in a Post-Collisional Stable Continental Region: Insights from 20 Years of cGPS in Romania*” for publication in *Solid Earth*.

We have carefully revised the manuscript in response to the reviewers’ constructive comments and suggestions, which we believe helped to clarify and strengthen our work. Please find below our detailed response. The reviewers’ comments are reproduced in **black**, our responses appear in **blue**, the original manuscript text is shown in *italics*, with new manuscript text in ***bold italics***.

Reviewer #1:

The manuscript provides a substantial contribution to the research and understanding of the tectonic processes in the area of Romania and also in the context of the surrounding tectonic setting. This is based on a thorough analysis of GNSS data and respective time series analysis. I assess the scientific significance with respect to data as excellent (1), as well as the presentation quality. Since in GNSS data processing, time series analysis, and velocity and strain field parameter estimation standard methods are used I rate the scientific quality as good (2). I very much appreciate the high quality of the manuscript in terms of language and understandability which makes reading a pleasure and the review easy. I would like to mention that my expertise is in the field of GNSS data analysis and parameter estimation techniques, but less in geophysics.

We sincerely thank the reviewer for the positive assessment of our manuscript and for highlighting the clarity and quality of the presentation. We appreciate your careful evaluation and your feedback regarding the GNSS data analysis and parameter estimation. Below, we provide point-by-point responses to your comments.

Figure 1: Abbreviation AM is not explained/introduced.

We have now introduced and explained the abbreviation “AM” (which stands for Apuseni Mountains) in the figure caption and in the main text, we added the abbreviation.

4. Analysis of the GNSS position (coordinates) time series: It seems that the estimation of velocities, jumps, annual and semi-annual parameters is based on a white-noise assumption, i.e. assuming no time correlation. However such time series usually contain significant stochastic time-correlation. Neglecting this might lead to over-optimistic confidence regions (as maybe the case in figure 3) and implausible statements about the significance of parameters. Have the authors considered this aspect?

In the current analysis, the method is quite straightforward. Our algorithm estimates the velocity, seasonal signals, and jumps in a single batch solution for each time series. In the revised manuscript

we added: *“The accuracies (σ) at the 95% confidence level were estimated using the weighted root mean square (WRMS) of the fit according to the formula:*

$$\sigma = \frac{2 \cdot WRMS}{2.4 \cdot T_{span}}$$

where T_{span} is the length of the time series in years. This approach provides robust and realistic estimates of velocities and their uncertainties, and yields results comparable with those obtained using other commonly used algorithms, such as HECTOR and MIDAS. For some stations, our uncertainties are smaller than literature values, which is primarily due to the longer time series used in this study. We also compared our results with those of Piña-Valdés (2022) for stations with common solutions and found an average difference of less than 0.08 mm/yr. This level of agreement demonstrates general consistency, although earlier processing campaigns did not benefit from the reprocessing strategies and data quality controls applied here. The results for the 99 accepted sites are summarized in Table S1 of the Supplementary Material.”

4.1 For the modeled jumps in the time series: Have the authors tried to identify the reasons behind (antenna change or some local effect, ...)?

The possible causes of the modeled jumps, including antenna changes and local site effects, are discussed in the manuscript. In Section 4.1, we wrote: *“**However, a small number of these sites still exhibit anomalous velocities. These are likely caused by local effects such as landslides, station instability, local geological conditions, subsurface compaction, undocumented antenna changes, and multipath interference. The main reason is that most GPS antennas are mounted on buildings and unstable steel rods. In our analysis, we try to model them as position jumps. The dates of these “unknown” jumps were established using a manual process by examining the “raw” time series plots for each station.**”*

4.2: A reference to table S3 in the supplementary material is missing.

Added, as suggested.

4.4: The reference mentioned for the STRAINTOOL is not given in the References of the manuscript.

We have now included the full references for STRAINTOOL in the References section as shown below.

Anastasiou D.G., Papanikolaou X., Ganas A., and Paradissis D.: StrainTool: A software package to estimate strain tensor parameters. Zenodo; Version 1.1, <https://doi.org/10.5281/zenodo.5501234>, 2021.

Shen, Z. K., Wang, M., Zeng, Y., and Wang, F.: Strain determination using spatially discrete geodetic data, Bull. Seismol. Soc. Am., 105, 2117-2127, <https://doi.org/10.1785/0120140247>, 2015.

Figure 3: Some large velocity vectors seem to deviate significantly from their neighbors (their motion pattern), e.g. ORAD, CLU2, TGMS. Shouldn't these deviations be addressed?

As discussed in Section 4.2, we applied an outlier detection method to identify stations exhibiting significant deviations from the regional trend. This includes stations such as ORAD, CLU2, and TGMS. These stations are now represented as red vectors in an updated version of Figure 3.

Reviewer #2:

1. This well-written paper describes results from 20 years of continuous GPS measurements in Romania. This is a useful addition to the existing literature. Some improvements can be made to the reference frame definition and the uncertainty estimation. The comparison with previous results is there, but could be expanded. I am not fully convinced by the vertical velocities and their interpolation. The interpretation is fine, with a range of hypothesis provided in terms of the underlying dynamics.

Thank you for your thoughtful and constructive comments. We appreciate your careful reading of the manuscript and the suggestions provided, which have helped us improve the clarity and robustness of our analysis. Below, we address each of your points in detail.

2. It may be useful to show seismicity on Figure 1, together with the location of GPS stations but without colors and site names. This would be particularly useful to accompany the introduction and tectonic setting sections of the paper. The details of the cGPS site names and network affiliations can be shown in the supplements, as they are not essential to the understanding of the paper.

We have replaced Figure 1 with a map showing station locations and seismicity, as suggested.

3. My first significant comment concerns the definition of the Eurasia-fixed reference frame, briefly described in lines 148-150. First, the publication from which "the most recently published rotation pole solution for that plate" is used should be cited. Second, one needs to know how well the velocities presented here fit a Eurasia-fixed frame. In other words, do stations allegedly on stable Eurasia have velocities that do not statistically differ from zero? This is particularly important since the "residual" velocities discussed in the rest of the paper are very small, on the order of 1~mm/yr. For instance, I am surprised that sites located on the East European Platform - - supposed to be stable Eurasia? or perhaps not? -- show a consistent NW-direct residual on the

order of 1~mm/yr. The authors may be using the Eurasia-ITRF14 angular velocities from Altamimi et al. 2017 (ITRF2014 plate motion model), but the consistency between Altamimi's frame definition and the authors is unlikely to be accurate at the 1~mm/yr level since they are (most probably) not using the same models. The most rigorous way to determine such a frame would be for the authors to include in the analysis a number of sites located on stable Eurasia, for instance those used by Altamimi et al. 2017. Then the authors could compute their own Eurasia-ITRF14 angular velocity and use it to defined a Eurasia-fixed frame consistent with their analysis. But if the authors choose to use an independently-published angular velocity, then they must shown that residuals are close to zero at sites located on stable Eurasia.

Thank you for this comment. We agree that using an accurate reference frame is important for interpreting GPS velocities. For our analysis, we converted the reference plate of our solutions to the Eurasian tectonic reference plate using the ITRF14 rotation parameters of that plate (Altamimi et al., 2017). This plate model has proven to be a reliable model for our analyses since it also works perfectly for other areas in Europe. Therefore, consider it suitable for the purposes of this study.

We were also surprised by the general NE motion of the EEP. But our results are consistent with Piña-Valdés et al. (2022), showing the same trend for a larger area around Romania. However, that is not a subject of our study.

In response to your question, we made a careful comparison of our results versus Piña-Valdés. For a few stations, we find differences of up to 2.0 mm/yr. These are probably due to the differences in the length of the time series. But on average, the differences are less than 0.08 mm/yr. We added a short note in the revised manuscript: *“We also compared our results with those of Piña-Valdés (2022) for stations with common solutions and found an average difference of less than 0.08 mm/yr. This level of agreement demonstrates general consistency, although earlier processing campaigns did not benefit from the reprocessing strategies and data quality controls applied here. The results for the 99 accepted sites are summarized in Table S1 of the Supplementary Material.”*

We also note that the residual velocities on nominally stable Eurasian sites are on the order of ~1 mm/yr with formal uncertainties of ~0.2 mm/yr, making them statistically significant at approximately the 5σ level. These velocities are derived from multi-year (>4 years) GPS time series, which substantially reduce random noise and provide robust linear trends. Even in a globally defined Eurasia-fixed frame, small non-zero residuals can naturally arise from minor intraplate deformation, local geophysical effects, or slight differences between our network realization and the published rotation parameters. The observed residuals are therefore consistent with expected variations within the Eurasia-fixed frame and support the broader patterns of intraplate motion discussed in the manuscript.

4. My second significant comment concerns the estimation of velocity uncertainties, not explicitly described in the paper. One can assume from lines 156-158 that the uncertainty is that of a least-squares fit to the time series, assuming linear and seasonal terms plus offsets. If it is the case, then how do the authors deal with the well-known issue of time-correlated noise in cGPS time series

and its impact on velocity uncertainty estimates? I suspect that the uncertainties estimated here actually underestimate more realistic ones that would account for time-correlated noise. That is an important issue in a region where residual velocities are very small.

Several published options exist to fit GPS time series using a noise model that accounts for time-correlated noise, for instance with CATS (Williams GPS Solutions 2008) or Bos et al. J. of Geodesy 2008.

As a side note, the authors should quote the confidence interval they choose instead of using "sigma". Note that a 1-sigma uncertainty in 2 dimensions (east-north velocities for instance) correspond to a 39% confidence interval. A much safer 95% confidence interval would correspond approximately to 2.4-sigma in 2 dimensions.

We thank the reviewer for raising this important point regarding the estimation of velocity uncertainties and the effects of time-correlated noise. In the revised manuscript, we added: ***“The accuracies (σ) at the 95% confidence level were estimated using the weighted root mean square (WRMS) of the fit according to the formula:***

$$\sigma = \frac{2 \cdot WRMS}{2.4 \cdot T_{span}}$$

where T_{span} is the length of the time series in years. This approach provides robust and realistic estimates of velocities and their uncertainties, and yields results comparable with those obtained using other commonly used algorithms, such as HECTOR and MIDAS. For some stations, our uncertainties are smaller than literature values, which is primarily due to the longer time series used in this study.”

Thus, we have clarified in the manuscript that the reported σ values correspond to a 95% confidence interval for consistency and transparency. While time-correlated noise can influence velocity uncertainties, the use of multi-year (>4-year) time series, along with WRMS-based estimation, reduces the impact of random noise and seasonal signals, providing realistic uncertainty estimates for the present study.

We also compared our results with those of Piña-Valdés (2022) for stations with common solutions and found an average difference of less than 0.08 mm/yr. This level of agreement demonstrates general consistency, although earlier processing campaigns did not benefit from the reprocessing strategies and data quality controls applied here. The results for the 99 accepted sites are summarized in Table S1 of the Supplementary Material.

5. Section "Estimating a gridded, smoother horizontal velocity field": how is the uncertainty on actual GPS velocities propagated to the gridded velocities? By the way, Figure 3 should show uncertainty ellipses for the gridded velocities.

The formal uncertainties of our GPS horizontal velocities are on the order of 0.02 mm yr^{-1} , whereas the velocities themselves are typically $1\text{-}2 \text{ mm yr}^{-1}$. The propagation of such small uncertainties would have a negligible effect on the resulting velocities. For this reason, we did not explicitly compute or display uncertainty ellipses for the gridded field. We will, however, clarify in the text that the station-level uncertainties are small relative to the observed signal and therefore do not significantly influence the derived deformation pattern.

6. I understand in section 4.4 that the strain rates are computed from the raw GPS velocities, not the gridded ones. Is that right? If so, what is the point of calculating gridded velocities? By the way, I assume that STRAINTOOL estimates velocities on a regular grid as well. Perhaps those should be shown on Figure 3? But I may be misunderstanding section 4.4 -- which perhaps calls for some clarification in the text

The gridded velocity field was used as input for the STRAINTOOL. We modified the text to clarify this. In section 4.4, we mentioned: "*we further estimate strain rate from the interpolated horizontal vector field of GPS velocities [...] This algorithm interpolates **our gridded solution** to derive horizontal strain rates across the region, using a weighted least squares approach on a **more dense regular grid.***"

7. Section 5.1 and throughout the paper: I suggest replacing "appear to show" by simply "show". The agreement between solutions seems ok in the horizontal, but there seems to be some scatter in the vertical?

We have revised the text in Section 5.1 and throughout the paper to replace "*appear to show*" with "*show*", where appropriate. This change improves clarity and strengthens the wording.

8. The comparison of the velocities in this work with those from Pina-Valdes et al. 2022 and Serpelloni et al. 2022 on figure 7 must show the 3 different solutions in different colors.

Thank you for this suggestion. However, the purpose of Figure 7 is to present all datasets together to illustrate the relationship between GPS velocities, and a seismic tomography model of the regional lithospheric structure. Using different colors for each published dataset could distract from this intended comparison and interfere with the background information. We therefore prefer to maintain a single color for the reference vectors to preserve clarity and focus on the broader regional patterns.

9. I suggest the authors also review and cite D'Agostino et al. Earth and Planetary Science Letters 539 (2020), which intersects the study area presented here.

For D'Agostino et al. (2020), which indeed covers a region overlapping with our study area, we found the study to be relevant and informative. We did include these solutions in our analyses of the regions around Romania. Inside the Romanian territory, there are only 11 common solutions, with most of them with large uncertainties due to short time series included in D'Agostino.

10. Line 285: I see no complexity in the strain rate field, which is rather smooth, with a consistent NS extension throughout and significant EW compression in the SW corner. I would actually like

to read a complete interpretation of Figure 5. Is there consistency between the earthquake focal mechanisms and the GPS-derived strain rate principal axes? What do we learn about the sense of slip on the main active faults? Are compressional events in Vrancea consistent with the principal axes of the geodetic strain rates?

We described this relationship and added more details in the revised manuscript, Section 6.2 “*Correlation with fault systems and seismicity*”. While the GPS-derived strain field is indeed relatively smooth at the regional scale, earthquake focal mechanisms and previously published stress inversion results (Petrescu et al., 2021) reveal local pockets of compression and complex faulting. These focal mechanisms often occur on secondary or inherited faults and accommodate transitions between compressional, strike-slip, and extensional regimes away from Vrancea. The main active faults primarily accommodate the regional N-S extension through normal and oblique-normal slip, while smaller structures adjust locally to accommodate deviations from the regional strain.

It is a complex challenge to jointly interpret them, mostly because short-wavelength deformation is superimposed on the smooth, long-wavelength strain captured by GPS, yet there is no other attempt to do this in a region where destructive earthquakes occur in a seemingly stable continental setting, where previous GPS studies relied on short-term campaign data.

11. Figure 5 could be improved by making the "beachballs" smaller, as they are quite cluttered. Also, it would be useful to show on the figure the depth of these events, as I assume that some are crustal but others much deeper. Perhaps color-coding the black quadrants of the focal mechanisms as a function of hypocenter depth would help?

We have now coloured the focal mechanisms by depth, as suggested, and scaled them according to magnitude.

12. I am a bit puzzled by the vertical velocities shown on Figure 4 and its interpolation. For instance, the uplift observed on the East Carpathians essentially depend on 2 sites with vertical velocity significantly different from the neighboring sites. Can we be certain that one is not looking at local anomalies due to monument stability or other local process? It is a bit troubling that this does not appear in Pina-Valdez' solution, for instance. The same holds for the SE Carpathians foredeep.

We are not certain which specific stations the reviewer is referring to. The uplift observed in the Eastern Carpathians is supported by a cluster of stations (not just isolated ones), including ARYB, VAMO, VATR, TPLT, BICA, PTNT, and BIST, all located within a $2^\circ \times 2^\circ$ area. South of this cluster, another group of stations (GHE, CIUC, TGTS, BACA, SFGH) shows a consistent downward trend.

To ensure data reliability, we had excluded all stations displaying anomalous time series or evidence of monument disturbance. Additionally, we only retained stations with at least four years of stable observations, ensuring that the derived vertical velocities reflect long-term signals. These quality control criteria are detailed in Sections 4 and 4.1.

From a geological perspective, the observed uplift in the Vrancea region is consistent with independent thermochronological evidence reported by Necea et al. (2005, 2021). Analysis of river profiles indicates a difference between the knee points and the river divides, suggesting that the rivers are actively adjusting toward equilibrium. This geomorphic signal provides independent support for ongoing regional uplift, in agreement with the geodetic results presented here.

Necea, D., Fielitz, W., and Matenco L.: Late Pliocene–Quaternary tectonics in the frontal part of the SE Carpathians: Insights from tectonic geomorphology, Tectonophysics, 410, 137-156, <https://doi.org/10.1016/j.tecto.2005.05.047>, 2005.

Necea, D., Juez-Larré, J., Matenco, L., Andriessen, P. A.M., and Dinu C.: Foreland migration of orogenic exhumation during nappe stacking: Inferences from a high-resolution thermochronological profile over the Southeast Carpathians, Global Planet. Change, 200, 103457, <https://doi.org/10.1016/j.gloplacha.2021.103457>, 2021.

13. It seems to me that the authors are placing too much confidence in their interpretation on local vertical velocities, especially at local scale. This comment applies to the South Carpathians (lines 415-416), but also to the SE Carpathian foredeep.

Our interpretation of localized uplift in the South Carpathians is based on multiple independent datasets, including Bouguer gravity anomalies, thermochronology, and recent studies on neotectonics and geomorphology (Necea et al., 2005, 2013, and 2021; Merten et al., 2005, 2010; Poncos et al., 2022; Gailleton et al., 2021). We have carefully considered uncertainties in these observations, and the combined evidence consistently points toward differential isostatic compensation and/or lithospheric-scale processes as plausible mechanisms. To ensure clarity, we have slightly reworded the text to emphasize that these interpretations are well-supported but not uniquely constrained.

Gailleton, B., Sinclair, H. D., Mudd, S. M., Graf, E. L. S., & Matenco, L.C.: Isolating lithologic versus tectonic signals of river profiles to test orogenic models for the Eastern and Southeastern Carpathians. J. Geophys. Res. Earth Surface, 126, 8, <https://doi.org/10.1029/2020JF005970>, 2021.

Necea D., Fielitz W., Kadereit A., P.A.M. Andriessen P.A.M., and Dinu C.: Middle Pleistocene to Holocene fluvial terrace development and uplift-driven valley incision in the SE Carpathians, Romania, Tectonophysics, 602, 332-354, <https://doi.org/10.1016/j.tecto.2013.02.039>, 2013.

Merten, S., Andriessen, P. A. M., Juez-Larré, J., Bertotti, G. V., & Dunai, T. J.: Dating the exhumation of the Romanian Carpathians: first results from apatite (U-Th)/He thermochronology, Abstract from Geophysical Research Abstracts, 7, 08138, <https://meetings.copernicus.org/www.cosis.net/abstracts/EGU05/08138/EGU05-J-08138.pdf>, 2005.

Merten S., Matenco L., Foeken J. P. T., Stuart F. M., Andriessen P. A. M.: From nappe stacking to out-of-sequence postcollisional deformations: Cretaceous to Quaternary exhumation history of the SE Carpathians assessed by low-temperature thermochronology, Tectonics, 29, <https://doi.org/10.1029/2009TC002550>, 2010

Poncos, V., Stanciu, I., Teleaga, D., Matenco, L., Bozsó, I., Szakács, A., Birtas, D., Toma, S.A., Stanica, A., and Radulescu, V.: An Integrated Platform for Ground-Motion Mapping, Local to Regional Scale; Examples from SE Europe. Remote Sensing, 14, 1046, [10.3390/rs14041046](https://doi.org/10.3390/rs14041046), 2022.

14. The authors write "Our results mark a significant improvement in spatial coverage and resolution", "Our extended and more reliable data" -- I am wondering compared to which previous work?

The earlier geodetic work in the region, based on campaign-style GNSS measurements, is described in the Introduction (lines 62 - 68). However, we agree that the connection between that background and our claims of improvement was not clearly stated in the Conclusions section. A new sentence has been added in the Conclusions section to make this comparison explicit: "*Earlier studies in the region relied on campaign-style GPS observations, Van der Hoeven et al., 2005. In contrast, our dataset includes continuous GPS data from 130 stations spanning 20 years, providing improved spatial density and temporal resolution.*"

15. The authors write "The relative motions between these regions generate a complex strain rate pattern with zones of extension, compression, and shear, which closely align with observed regional seismic activity." What is meant by "align", especially since there is no seismicity map in the paper? For instance, do the authors mean "coincide" or also "share the same mechanisms"?

The seismicity map used to support this statement was included in the supplementary material (Figure S1). Still, we acknowledge that this may not have been sufficiently clear in the main text. As stated earlier, we have incorporated your suggestion and included the seismicity in the first figure.

16. Throughout the paper: I suggest toning down (or removing) terms such as "fundamental", "vital", "crucial", "complex", "fresh", "significant". After all, this is only geodesy!

We appreciate the reviewer's suggestion to tone down terms such as "complex" or "significant." However, we are keen to maintain these descriptors because the region genuinely presents a unique and scientifically challenging setting. Despite being far from plate boundaries and considered part of the stable Eurasian continent, this area exhibits persistent deformation detectable with modern geodetic methods and hosts destructive earthquakes, including in the Vrancea slab region. The last geodetic studies relied on short-term campaign data with large spatial variations and uncertainties, published more than 20 years ago, leaving substantial gaps in our understanding of the ongoing strain field. Moreover, the interactions between the subcrustal Vrancea Slab, the overlying crust, and regional plate motions generate stress and deformation patterns that are far from trivial to interpret. Given these factors, we believe it is appropriate to describe the region and our study as complex and significant, and we prefer to retain this terminology to reflect the scientific context and novelty of this work accurately.