nature portfolio

Peer Review File

Long-term Doppler imaging of the star XX Trianguli indicates chaotic nonperiodic dynamo

Editorial Note: This manuscript has been previously reviewed at another journal that is not operating a transparent peer review scheme. This document only contains reviewer comments and rebuttal letters for versions considered at *Nature Communications.*

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

The authors' responses to the comments posed were largely acceptable and addressed the concerns through clarifications and additional text. All of the attached movie files are also now accessible.

There are some concerns that remain.

-While the explanation about the north-south degeneracy is acceptable, more details on the star's inclination need to be given. The authors gave a thorough explanation in response to Referee #2's question of whether the inclination from previous study had been improved upon or not. The inclination is given in the text as though it was measured and not inferred through fits to the data. Please clarify this in the text. Additionally, on Page 3, inclination is included in a list of parameters that are considered "known with high precision," which is misleading given the inclination is not directly measured.

-In response to my comment in Section 1.3 APT, the authors suggest that it would be damaging to the data and results to provide a qualitative analysis comparing their surfaces to the photometric data. I disagree.

As the authors state, the non-cyclic, long-term trend seen in the photometry cannot yet be explained. While one approach taken in past studies was to eliminate this feature with a simple long-term trend and assess at the rotations individually, it is a stretch to consider this brutal when it is often largely a median subtraction from each observing period that occurs.

It should be possible to just assess consistency of the Doppler images with the photometry---even something as basic as the brightness in the Doppler surface goes up when photometry also indicate a brightness increase showing evidence that these are real structures. While the information to be garnered from photometry is limited, pointing out these consistencies makes the Doppler results more believable. Ignoring this in the text while including the photometry makes the reader suspicious that there are substantial disagreements that are being intentionally left out. Given the timespan of the data included in Figures 3c and 7 compressing the data so individual rotations cannot be seen, it is difficult for the reader to assess this on their own.

-The mass of the star is mentioned multiple times, but only the mass function can be known, by the authors' own words. While this is explored in Section 2.3 General Assumptions, the paragraph that starts on Page 2 asserts the primary star's mass is ~1.2 Msun, which in 2.3 is not given as the most probable mass. This may be resolved by including citations for the stellar parameters in that paragraph on Page 2 when the parameters are not going to be those determined by this work. For the value given in Table 1 and the description given in Section 2.3, it would be useful to understand what led to 1.1 Msun being the most probable value.

Reviewer #2 (Remarks to the Author):

Review of the manuscript entitled:

“A 16-yr movie of the spotted surface of the K-giant XX Trianguli: evidence for a chaotic stellar dynamo”

by Strassmeier et al.

This revised manuscript describes the mapping of the surface spot features on the K0III star XX Tri, covering ~16 years of observations. The authors have used the technique of Doppler Imaging to recreate 99 images of the stellar surface over this time making it the most heavily sampled study of stellar spot evolution to date.

General comments:

I thank the authors for their replies to the queries I raised in the previous version of the paper. Although the authors have answered most of the queries I previously raised there are a few further points that I feel could be expanded upon more in the paper as I mention below.

As I remarked previously, overall the paper is well written and provides an intriguing look at the evolution of the spot features of XX Tri. This initial paper focuses on the astrometric impact of the spots features on the surface of XX Tri, which have been mapped by the authors using the technique of Doppler imaging. The authors find that the photocenter of the star changes by up to ~10% of the stellar radius and is the main finding of the paper.

Main points:

- The photocenter calculations given by the authors are based on the reconstructed Doppler maps they create. The accuracy of the calculated photocenter offsets provided by the authors are reliant upon the accuracy of the reconstructed Doppler images in reproducing the stellar surface configuration. Given that the authors have access to contemporaneous photometry of XX Tri (Figure 7), is the change in flux measured from the Doppler maps matched by the changes in the photometry from the star? Can the author's comment on how well the Doppler maps are able to reproduce the photometric changes and thus the accuracy of the photocenter calculations?

- Given that the spots recovered in Doppler images are usually restricted to one hemisphere (normally nominated as the Northern hemisphere) of the star, can the authors comment on how the unrecovered spots from the southern hemisphere will impact the calculations of their photocenter? I assume that this will be mitigated somewhat by the projection effect and by the fact that latitudes below -60deg on the star are not visible, but some discussion on this would be useful.

Minor points:

- In the abstract the author's have included a mention about the impact the photocenter on the detection of exoplanets. I thank the authors for this, but where it has been included seems bit out of place and I would suggest some minor rewriting to fix this.

- On page 3 the author's mention that bright faculae are required to account for the observed brightness differences seen in the star's photometry. They then report on page 4, that they have found 5 such events in their recreated Doppler maps. Could the author's provide any indication if this would be enough to explain the brightness differences mentioned on page 3, at least for the time period of the reconstructed maps, or is this number likely to be an under (or over) estimate?

- In Section 2.3, the authors state that: "For a differentially rotating surface, like for XX Tri, and likely all cool stars…". It was my understanding that XX Tri was mapped assuming solid-body rotation, correct? If so, are the authors simply assuming that XX Tri is differentially rotating? Some comment on the assumption of solid-body rotation in the mapping should be mentioned at least here.

- In Section 2.7, the author's mention that the image photocenter is a point on the apparent disk of the star. When calculating this do the authors take into account the stellar inclination as the star is visible to us? This should be mentioned.

Reply to Referees

Pls note that the current submission was reformatted according to the ncomms-formatting-instructions as requested by the editor. The paper is now split into the main text/figs/tables/refs and the supplementary material with its own figs/tables. There are no section numbers anymore in the Method's section but we quote them below for easier orientation.

Reviewer #1 (Remarks to the Author):

-While the explanation about the north-south degeneracy is acceptable, more details on the star's inclination need to be given. The authors gave a thorough explanation in response to Referee #2's question of whether the inclination from previous study had been improved upon or not. The inclination is given in the text as though it was measured and not inferred through fits to the data. Please clarify this in the text. Additionally, on Page 3, inclination is included in a list of parameters that are considered "known with high precision," which is misleading given the inclination is not directly measured.

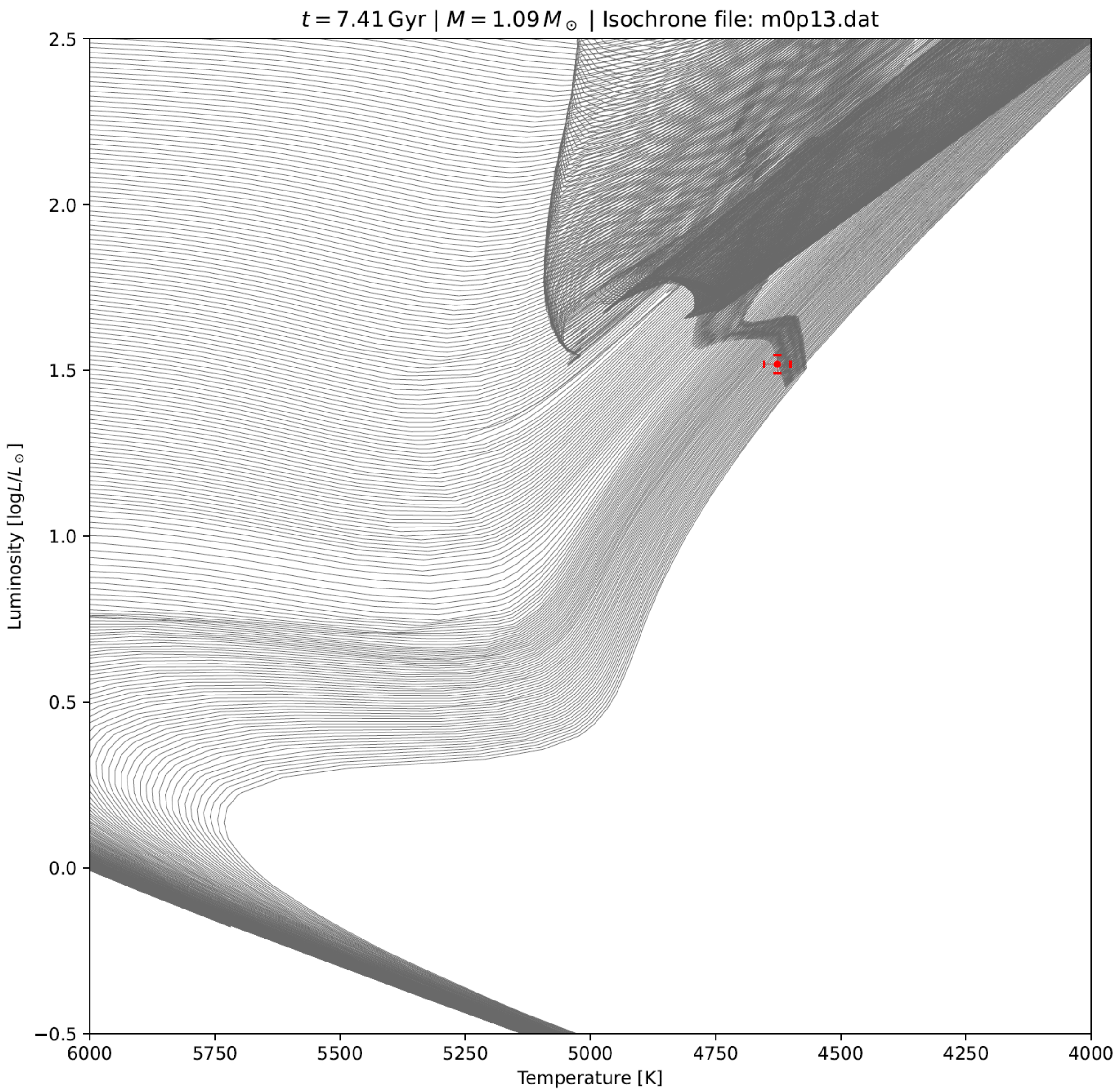
We added now a shortened version of our earlier reply to Referee #2 into the method's text in Sect. 2.3 ("General assumptions", p.23), and also added a sentence clarifying the indirect inclination "measurement" in the main text on p.3.

-In response to my comment in Section 1.3 APT, the authors suggest that it would be damaging to the data and results to provide a qualitative analysis comparing their surfaces to the photometric data. I disagree. As the authors state, the non-cyclic, long-term trend seen in the photometry cannot yet be explained. While one approach taken in past studies was to eliminate this feature with a simple long-term trend and assess at the rotations individually, it is a stretch to consider this brutal when it is often largely a median subtraction from each observing period that occurs. It should be possible to just assess consistency of the Doppler images with the photometry---even something as basic as the brightness in the Doppler surface goes up when photometry also indicate a brightness increase showing evidence that these are real structures. While the information to be garnered from photometry is limited, pointing out these consistencies makes the Doppler results more believable. Ignoring this in the text while including the photometry makes the reader suspicious that there are substantial disagreements that are being intentionally left out. Given the timespan of the data included in Figures 3c and 7 compressing the data so individual rotations cannot be seen, it is difficult for the reader to assess this on their own.

A new additional figure (now named Fig. 7 in the Supplementary material) shows a direct comparison of the predicted brightness variations from our Doppler images with the (existing) V-band APT photometry. As already stated, we cannot explain the observed long-term brightness trend with our (purely photospheric) Doppler images and must remove the V-band trend prior to a comparison on the scale of rotational modulation. This "de-trending" is not without concerns and is achieved with a simple normalization of the seasonal average brightness so that the seasonal averages are $\Delta V=0$ in the new figure. At this point we note that Fig. 3c already shows the predicted variations over time (relative flux in this case and normalized to a theoretical unspotted star). This figure also indicates the times when the variations were determined directly from the Doppler images or when they were determined from the interpolated images. Because the interpolated images are basically identical to the actual Doppler images for the time ranges when the Doppler images were obtained, we use only the interpolated images for this (long-term) light-curve comparison. The new figure shows that the reconstructed light curve follows the overall rotation changes relatively well, and that the differences are also due to the fact that in a number of cases the spectra used for DI cover more than one rotation cycle (listed in Supplementary Table 1 = previous Table 2), which essentially means time averaging of the rotational changes in the spectral lines (the figure additionally demonstrates the difficulty to include broad-band photometry in time-series Doppler imaging because the photometry is additionally influenced by chromospheric and circumstellar contributions as well). Therefore, and because already the seasonal de-trending is a hand-waiving procedure, our rotational-modulation comparison remains only qualitative. Nevertheless, the predicted light curves from the DIs match the photometric variations reasonably well. This info is added in Methods Sect. "Individual maps: fit quality and example result" (former Sect. 2.6) and also mentioned in the "Discussion".

-The mass of the star is mentioned multiple times, but only the mass function can be known, by the authors' own words. While this is explored in Section 2.3 General Assumptions, the paragraph that starts on Page 2 asserts the primary star's mass is ~1.2 Msun, which in 2.3 is not given as the most probable mass. This may be resolved by including citations for the stellar parameters in that paragraph on Page 2 when the parameters are not going to be those determined by this work. For the value given in Table 1 and the description given in Section 2.3, it would be useful to understand what led to 1.1 Msun being the most probable value.

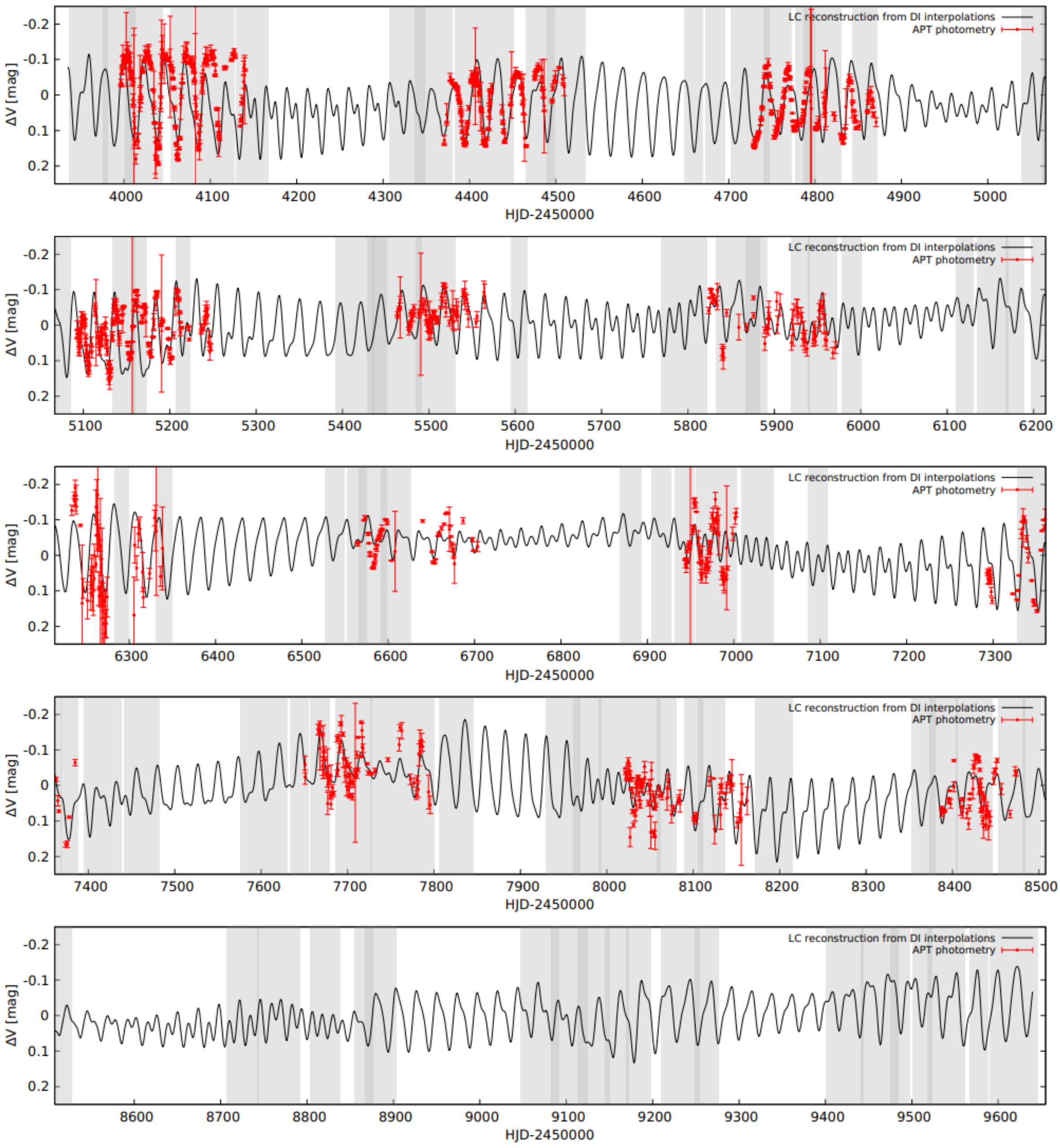
Oh, thanks. We indeed forgot to describe where the "most probable" mass (and age) in Table 1 came from. This is now added to the method's text in "General assumptions" (former Sect. 2.3). Both values and its errors were from a multiparameter track- and isochrone fit with the Teff, Fe/H, and luminosity from the DR3 parallax as listed in Table 1. The additional text required two new references (one for the isochrone-fitting code and one for the isochrones themselves). An H-R diagram with all isochrones considered exists (see below) but is not given in the paper because it is not a major result. Following the referee's suggestion the most-probable mass from this paper is now already mentioned in the first sentence of the main text on p.2 instead of our initial 20% orientation value from the average from "Künstler et al." (1.26 Msun) and "this paper" (1.1 Msun).



Working plot for the isochrone fit to the position (red dot with error bars) of XX Tri in the HRD. Isochrones are from PARSEC. XX Tri data from Table 1. Numerical result is listed on the top.

Reviewer #2 (Remarks to the Author):

- The photocenter calculations given by the authors are based on the reconstructed Doppler maps they create. The accuracy of the calculated photocenter offsets provided by the authors are reliant upon the accuracy of the reconstructed Doppler images in reproducing the stellar surface configuration. Given that the authors have access to contemporaneous photometry of XX Tri (Figure 7), is the change in flux measured from the Doppler maps matched by the changes in the photometry from the star? Can the author's comment on how well the Doppler maps are able to reproduce the photometric changes and thus the accuracy of the photocenter calculations?



A new figure (above, now Supplementary Fig. 7) has been added showing a direct comparison of the predicted light curves from the interpolated DIs (line) with the pre-whitened APT data (red dots with its error bars). The grey areas denote the times of the Doppler images. Pls see also our reply to Referee #1 who addressed the same issue.

- Given that the spots recovered in Doppler images are usually restricted to one hemisphere (normally nominated as the Northern hemisphere) of the star, can the authors comment on how the unrecovered spots from the southern hemisphere will impact the calculations of their photocenter? I assume that this will be mitigated somewhat by the projection effect and by the fact that latitudes below -60deg on the star are not visible, but some discussion on this would be useful.

Correct. Spots at higher-to-intermediate latitudes in the adjacent "southern" hemisphere are reconstructed only if the fit to the data needs to be improved for a short phase duration (spot visibility at restricted phases) while not affecting the fit at other phases. Spots are not *per se* restricted to a "hemisphere". We frequently see spots between 0° and -30°, once or twice even down to -50° (e.g., for rotations #93 and 113). Its detections are strongly S/N- and phase-coverage dependent. Of course, even if perfect data exist, we are left with a north-south latitude-to-latitude cross talk that, however, gets smaller and smaller the lower the inclination up to the point when the lack of Doppler velocity per latitude bin takes over and starts to dominate the signal which is easily recognized (to the extreme of zero Doppler effect when the star is seen pole-on, independent of vsini). At the time we discovered large polar spots the question was raised whether the other (hidden) pole also has a large spot and whether we could see its rim in higher inclination targets. No such detection is known until today. We refer to the discussion in Piskunov & Rice (1993, PASP, 105, 1415). Whether such unrecovered spots, temperature or chemical ones, really exist remains undetermined (we unconsciously tend to assume hemispheric symmetry like on the Sun, but face a qualitative different dynamo in XX Tri) and, in any case, the impact on the photocenter determination would be always very small due to the much larger foreshortening angles as compared to the reconstructed spots. Because in our case i=60°, and because our line profiles are of high S/N, we do not think that there is such an unnoticed residual cross talk that could affect the photocenters. But even if, it moreover could affect only the y-component of the photocenter (i.e., in the direction parallel to the projected rotational axis), and not the full radial extent.

A discussion of this in the text appears overemphasizing, as we can draw no conclusions nor present quantitative limits or there-alike but still need the text space of a full paragraph. We would prefer keeping it out of the text and leave it just to the referee's discretion. Just a sentence with reference to the Piskunov & Rice (1993) discussion is added in the main text where we also refer to the north-south degeneracy.

Minor points:

- In the abstract the author's have included a mention about the impact the photocenter on the detection of exoplanets. I thank the authors for this, but where it has been included seems bit out of place and I would suggest some minor rewriting to fix this.

The abstract had been rearranged due to the limitation to 150 words. The marked sentence was moved.

- On page 3 the author's mention that bright faculae are required to account for the observed brightness differences seen in the star's photometry. They then report on page 4, that they have found 5 such events in their recreated Doppler maps. Could the author's provide any indication if this would be enough to explain the brightness differences mentioned on page 3, at least for the time period of the reconstructed maps, or is this number likely to be an under (or over) estimate?

The statement on p.3 ("It requires bright faculae in order to explain the observed brightness differences by rotational modulation.") referred to the epochs of extreme V-band amplitudes due to *rotational modulation*, which could not be modelled anymore with just cool spots. No such extreme rotational-modulation amplitudes were observed in our current photometric data coverage during the Doppler imagery. The DIs recovered, independent of any photometry, just small (but numerically significant) bright/warm features with at most dT≈200K wrt the photosphere. Regarding the long-term brightness change, which is of the order of another 0.6 magnitudes on top of the rotational modulation, an explanation with concrete warm/hot features is impossible because then we must see larger rotational-modulation amplitudes when the star is overall brighter and necessarily also bluer (smaller B-V or V-I), but it appears that the opposite is the case (higher amplitudes when the star is fainter). When considering only the time period of the reconstructed maps, the amplitude is smaller but still large enough so that the effect is noticeable. And even there it the amplitudes are larger when fainter. So, the simple version of an answer to above question is -no-, it requires much larger and warmer features than those reconstructed on the five events in order to model just the 0.6-mag V-band amplitude due to rotational modulation in, e.g., 1998 or 2000 (Ref. 10 reconstructed a warm feature with dT=350±20K for 1998 and size about five times larger than the currently seen five features). The "number" is thus neither an under- nor an overestimate but just only part of the solution. We mention the situation now at the end of the Methods "Individual maps: fit quality and example result" (former Sect. 2.6).

- In Section 2.3, the authors state that: "For a differentially rotating surface, like for XX Tri, and likely all cool stars…". It was my understanding that XX Tri was mapped assuming solid-body rotation, correct? If so, are the authors simply assuming that XX Tri is differentially rotating? Some comment on the assumption of solid-body rotation in the mapping should be mentioned at least here.

Correct. The assumption of solid-body rotation for the inversion is justified even for a differentially-rotating star because we use typically only a single stellar rotation for the inversion, thus, no latitude-dependent phase smearing from one rotation to the next (=the "signal" for DR) is expected. Once we compare a particular DI to the next in the time sequence, e.g., with a 2D cross correlation, we can infer and quantify latitude-dependent DR. Weak, solar-like DR of XX Tri of shear α=0.016, was already detected in Ref. Künstler et al. (2015), so it is not just an assumption. We added a sentence in the Methods "General assumptions" section (former Sect. 2.3).

- In Section 2.7, the author's mention that the image photocenter is a point on the apparent disk of the star. When calculating this do the authors take into account the stellar inclination as the star is visible to us? This should be mentioned.

The photocenter is determined from the images, and the images are obtained with a fixed inclination of 60°. The apparent disk is just a projection of these images onto the plane of the sky (like the orthographic projection). Thus, yes, the inclination is implicitly included in the photocenter comp. We mention this now explicitly in Methods "Image photocenter" (former Sect. 2.7). Note that this is also noticable for the animated star in movie `radial_offset_anim.mp4`.

REVIEWERS' COMMENTS

Reviewer #1 (Remarks to the Author):

The revisions satisfy the issues I brought up in my previous reports. I have no further concerns.

Reviewer #2 (Remarks to the Author):

Review of the manuscript entitled:

“A 16-yr movie of the spotted surface of the K-giant XX Trianguli”

by Strassmeier et al.

The revised manuscript discusses the mapping of the surface spot features on the KOIII star XX Tri over a period of approximately 16 years using the technique of Doppler Imaging. The images produced by the authors are a significant contribution to our understanding of the evolution of spot features on such stars.

I once again thank the authors for their response to my queries and the changes implemented to the article. Given this, I am happy for the article in its current form to be accepted for publication.