General comments

Overall, the manuscript is of very high quality. The methodology is novel, seems robust, and could be applied to many other study cases. The results from the Afar region are globally relevant and allow to better understand the late phase of rifting. One of the main interests of the paper is that it links different scales of time, liking processes happening over millions of years with processes happening over decades or even shorter. The paper is very well written and has a very clear and coherent structure. The figures and supplementary figures look good and are clear. Except for one aspect to be discussed, the results support the conclusions.

I mainly have minor comments. Some uncertainties of the methodology could be discussed in more details, even though they won't change the conclusions. I also wonder why you didn't calculate more geological strain rates from existing datings. This parameter is very interesting and allows a direct comparison with Recent geodetic data. Finally, I question one point of the conclusion regarding Manda Inakir which should be discussed and argued before being presented in the conclusion, or be toned down.

I also made several suggestions that do not relate to scientific quality, but to style or clarity, which is subjective and personal. They are just suggestions, don't hesitate to not follow them without justification.

Specific comments

l.88-90: "*Faster spreading rates occur between Arabia and Nubia with velocities increasing from ~10 mm/year at latitude N14.5° to ~20 mm/year at latitude N12.7° causing counterclockwise rotation of the Danakil Block*". This sentence is a bit confusing and not 100% correct. 10 mm/yr does not represent the velocities between Arabia and Nubia at 14.5°N, but the velocity between the Nubian plate and the Arrata Microplate (or Danakil Block). The full velocity between Nubia and Arabia is approx. 17 mm/yr at this latitude. But I think part of the problem comes from the fact that you did not yet introduce the Danakil block in your manuscript at that point. It is thus difficult to explain these velocities without the prior explanation of this concept. In my opinion, this is important to make clear because it is relevant for one of your points of conclusion. I think this concept would also be easier to explain with a bigger context map which leads me to my next point.

Figure 1: The figure is great but I think the insert with the full map of the Afar is quite small. It makes it even difficult to see the rotation of the Danakil Block. One possibility (but that's just a suggestion) would be to make a full standalone figure just with the context maps of the Afar Depression, allowing you to zoom out, better show the kinematics of the region

and the relation to the Red Sea and Gulf of Aden. Another possibility (again, just a suggestion), would be to just zoom out the existing insert, maybe leaving out the seismicity to not over-clutter the map.

L. 189-194 and methods: A question about methodology here. You assume that the horizontal distance between the two pick-up points directly corresponds to the extension. You mention that this method works well with lateral variations towards the tips of the fault. What about the influence of surface processes? Erosion of the footwall and deposition in the hangingwall will artificially reduce the angle α, increasing the measured heave (and maybe decreasing slightly the measured throw). If you restrict your measurement to the steepest part, you might alleviate this bias on the heave, but you would artificially reduce the throw. Did you check this? A good way would be to look at the distribution of α values. If they are mostly above 60°, that would be a good sign. If a lot of them are below, that might indicate a bias. Regardless of the distribution of α values, I think it would be good to mention this source of uncertainty, how you managed it and your assessment of its impact on the results.

Another point on the methods: I see that the process fails to identify very large faults in the southern part of the Immino Graben (around 12.13N, 41.69E and 12.17N, 41.88E). Looking at it on satellite images, it seems clear that these are faults (with very large throws of approx. 600m and 200m, respectively). In your opinion, what is causing this? It might again not be bad to discuss this source of uncertainty and its impact on the results.

Figure 3: I am a bit surprised by the rose-diagram of fault strike (b). There is this big NW trend coherent with the main orientation of the Afar rift. The second WSW to SW trend surprises me. If you look at the southern Afar (Karrayyu, Adda'Do) on the map, the faults rather seem to trend SSW. And there is almost nothing in this sector of your rose diagram. This effect is even stronger on the rose diagram of figure S4. I'm also surprised you don't have more of the NNW orientations which you can observe on the map in Tat'Ali, Makarassou (and which are more present in the rose diagram of Polun et al., 2018). I did not do any quantitative assessment of it, but it "looks" strange to me. Could you check upon this? Just to make sure there was not a small mistake in the algorithm calculating the azimuths or plotting the rose diagram.

L. 270-272: rates. *"The comparison was possible only where well-constrained rock dating was available. Therefore, we used rock ages available close to the profiles (Courtillot et al., 1984; Kidane et al., 2003; Lahitte et al., 2003; Feyissa et al., 2019) but it was not possible to apply this approach to the entire strain map."* I don't really understand why you didn't calculate more points. We see only 9 points on fig. 4b. There are much more datings available, from the sources you mention and from others (e.g. Kidane et al., 2003, Courtillot et al., 1984, Manighetti et al., 1998, Zumbo et al., 1995 [I have a shapefile with a compilation (and corrected coordinates) here if you want to check: https://doi.org/10.5281/zenodo.7410073]). I guess it would not be difficult for you

to calculate geological strain rates for more points where datings are available. These strain rates are very interesting and allow a better comparison with Recent geodetic strain rate, and they would allow to strengthen considerably your interpretation. But maybe there is a good reason why you only restricted this quantification to a few points?

L. 426: Where does this 1000-1600 m value come from? Previously, you mentioned 200-1000 m from Abbate et al. (1995).

L. 452-453: *"During the last 0.6 Ma, the external sectors of the Afar rift, including Makarassou and MI are not the main locus of strain following the migration and focusing of extension to the rift axis."* I think that this conclusion point deserves more discussion, in particular concerning the Makarassou and Manda Inakir region. I was also always intrigued by this area and by this process of strain focusing. In Manda Inakir, the strain is not huge, but it's still clearly faulted. However, the volcanic rocks there are quite Recent. Lahitte et al. (2003) and Manighetti et al. (1998) show age <0.6 Ma, down to 0.03 Ma. So, what is the geological strain **rate**? I'd guess that with these recent ages, it's not that low. This would mean it was still a locus of strain in the last 0.6 Ma.

Let's take another example: the West Harak graben (12.67N, 41.09E). It is also very recent (0.62-0.07 Ma, Lahitte et al., 2003), heavily faulted and accommodates significant strain according to your results. So, I'd guess that the geological strain rate is also quite high. This suggests, again (maybe even more clearly than MI), that the switch to Dabbahu - Manda Hararo happened more recently than 0.6 Ma.

One could even hypothesize that these peripheral regions became inactive very recently (100s, 1000s, 10000s of years) [but that's a wet-finger guess]. And this would have important implications. It would mean that this change is not necessarily representative of a long-term trend of strain localisation at long-lived and final rift segments, but rather a short-term alternation between different areas of strain accommodation. And that these areas of strain accommodation might have changed in the past and might change again in the future. At larger time and spatial scale, the Afar region has already seen multiple rift axis jumps and abandonments. So, I'm wondering whether what we see here is a (final) rift localisation, or just one rift jump among many others.

Without quantification, this is difficult to answer. And this brings me back to my comment above: why didn't you calculate more geological strain rates points? With more points in Makarassou, Manda Inakir, but also elsewhere in Afar, you could significantly strengthen your point. With only 9 data points, it's hard to be very conclusive.

But I'm probably falling down a rabbit hole, and it's not the main point of the paper. Material for further reflections in the future.

To sum up this comment, I think you should discuss this topic more explicitly in the discussion, taking into account the aspects mentioned above, before presenting it in the

conclusion. Or just mention what we know for sure: the MI (and other peripheral regions) currently does not accommodate extension, but we don't know exactly when this change happened.

Technical corrections

L. 21: mail → main?

L. 20-21-22: Maybe separate in two sentences for clarity? ...*locus of strain*. *Rifting processes*... Just a suggestion.

L. 91: Maybe a small modification like "*The rifting stage in the Afar was accompanied by two further flood basalt*..." to make clear that these are not the initial flood basalt event. Just a suggestion.

L. 95: maybe you can also cite Tesfay et al., 2003 https://doi.org/10.1130/B25149.1bv (just a suggestion).

L.116: I think Varet's book was published in 2018.

L. 260: 93% is a very good result! Your algorithm is doing a very good job.

Figure 4: A nice-to-have (but not needed) improvement of your figure could be the addition of your calculated geological strain rates (white dots on b) projected on the cross-sections (c-h). It would help to link both datasets and showcase these nice results a bit better. Just a suggestion.

L. 390: "... *faster angular velocity*..." *faster velocity* rather, no? The angular velocity is constant but the further away from the Euler pole you are, the faster absolute velocity is.

L.420: "*On one hand, the faults that we mapped at might*..." typo.

L.431-433: If you want, you might want to add Wang et al. (2021) https://doi.org/10.1016/j.tecto.2021.228857 also showing this interpretation. Just a suggestion.

If there is anything unclear in my review or if you have any question, don't hesitate to post a quick message in the discussion.

Best regards,

Valentin Rime