



Review Report

Knight et al., Testing the Sensitivity of Anisotropy of Magnetic Susceptibility (AMS) to the Regional Tectonic Strain Field in Granite Plutons; Insights from two Orogen-scale Studies, TEKTONIKA, 2024.

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1st Round of Revisions

Decision Letter

Dear Hazel Knight and co-authors

We have now received 3 reviews of your manuscript “Testing the sensitivity of anisotropy of magnetic susceptibility (AMS) in granite plutons to the regional tectonic strain field; insights from two orogen-scale studies” submitted to Tektonika. Based on these reviews, along with our own assessment, our decision is: Moderate Revisions Required.

The three reviews, along with our own assessment agree that the manuscript is well written, well-illustrated and the methodology is well conceived. However, there are some important points that we feel need to be addressed before this paper can be published. The reviewers have commented on your use of terminology and definitions, have suggested some additional references to support your work, and have pointed out where the layout and structuring of the text could be improved. We ask that you carefully address and respond to each of the points raised by the reviewers in a response letter, and we ask you to submit a copy of your revised manuscript, with changes clearly marked, and a clean version, during your resubmission.

We hope that you will be able to address the reviewers' concerns and submit a revised manuscript within 2 months (please note that Tektonika does not impose a time limit on submitting revised manuscripts, this suggested timeline is provided as a guide only). If you require additional time for your resubmission, please don't hesitate to get in touch with the editorial team to discuss a revised timeline. Feel free to reach out at any stage if anything is unclear.

We wish to congratulate you on this impressive work and look forward to receiving a revised copy of the manuscript by 29th November.

Thank you for giving us the opportunity to consider your work.

Yours sincerely,

Andy Parsons, PhD, Associate Editor - Tektonika

Janine Kavanagh, PhD, Executive Editor - Tektonika

Comments by Reviewer 1 with Authors' Response

Dear Editors and Reviewers,

We wish to thank you for your detailed and helpful comments on our manuscript- we are very grateful for the time you have spent on this, and it has helped us significantly improve our manuscript. Below we provide individual replies to the reviewer's comments (comments in blue, replies in black), addressing all the points raised. Line numbers here are the same as those given in the original review and therefore refer to the originally submitted manuscript.

Thank you once again for your assistance.

Hazel Knight

Reviewer One (Helena Silva)

General

In my opinion the article is suitable for publication in Tektonika since it presents a well-designed original methodology in accordance with the objectives to be achieved.

The authors compiled a regional database of AMS data from multiple granite complexes across two orogens, the French Massif Central and the British and Irish Caledonides, and critically evaluated the degree to which the magnetic fabric of the granite plutons recorded the regional tectonic strain.

The approach and objectives of the work are well outlined in the article. The results are described exhaustively and the conclusions well sustained.

I don't think the results/conclusions bring much new in terms of the relationship between AMS fabric and regional stress/strain contexts, but as this is an article that is the result of a thesis toward an MSci in Geology, I think it is valuable for publishing.

However, in terms of layout (font size, paragraphs, captions...), the manuscript should be improved.

Also the manuscript could be simplified as it sometimes becomes repetitive.

Thank you, we have removed a few redundant or unnecessary sentences according to the reviewer's suggestion.

Specific

Line 117: Considers typo

Accepted

Line 135: formatted subscript (and thereafter)

Accepted

Line 191: Tables with caption top headed

Table caption moved.

Line 298: “statistical analysis”

Accepted

Line 354: “Lower hemisphere equal-area projections of K1 and K3”

Accepted

Line 361: Stereonets to “Stereos” (and thereafter)

Stereonets is a recognised term for these diagrams. The abbreviation seems unnecessary.

Line 368: K₃ not K₂

Accepted

Line 371: 3bi not 3ai

Accepted

Line 375: K₃ not K₂

Accepted

Line 378: but to “except”

Accepted

Line 384: added “No ellipsoid shape data is available for this complex”.

Accepted

Line 389: “Where is figure 3e???”

Fixed issue with Figure Fig 3d(i) and 3e(i) where correct dataset for 3di was missing.

Line 401: “Lower hemisphere equal-area projections of K1 and K3”

Accepted

Line 407: added “Data sources are as follows”.

Accepted

Line 414: changed low-angle to “sub-horizontal”

Accepted

Line 442: “Summary”

Accepted

Line 462: Caledonian spelling error (and thereafter)

Accepted

Line 472: “Lower hemisphere equal-area projections of K1 and K3”

Accepted

Line 618: “fabric does not dominate the ~~signal~~ orientation or appear to alter modify the mean orientation from the regional tectonic trend”

Accepted

Line 843: forces to “stress”

Accepted

Line 921: study to “research”

Accepted

Line 922: added “between K1 and K3”

Accepted

Comments by Reviewer 2 with Authors' Response

Reviewer Two

General

NB, the comments below focus on scientific aspects, there are quite a few spelling and grammatical errors that require attention during revision. Numerous sentences start with "As such....." and grate severely on my grammatical nerves, all of these should be amended.

We have thoroughly checked the grammar and made several minor amendments throughout the manuscript, which we hope will improve the readability of the paper.

Organisation – numbering of sub-sections within section 2 (Methods and Analytical Overview) needs looking at – in places it is incorrect and the number ordering does not follow. It is also the case that section 2 is rather jumbled. It would be MUCH better to have this section as being devoted entirely to ALL the theoretical palaeomagnetic methodology and context, to be followed by a section 3 containing ALL regional geological background (inc Table 1 and accompanying text). The 'Results' could then become section 4.

Errors with numbering structure have been corrected and the overall structure reorganised as suggested, thank you for your helpful input.

Specific

Line 40 to 41: really? Surely if the country rocks had been deformed prior to or during pluton intrusion why would these "traditional" strain markers be recording post-emplacement strain?

Reworded to make it clearer than this sentence is referring purely to strain markers within the plutons themselves.

Table 1:

- 1. More information needs to be presented with regard to each cited age, specifically the isotopic method used and the source reference. No error is attached to the cited age for the Ratagain pluton (it should be ± 3 Ma). Readers need to know where the ages come from and how reliable they are.**

Age data source and isotopic method columns added to Table 1. Missed error on Ratagain added, thank you for highlighting this accidental omission.

2. The “Data Source” column needs to be relabelled “Palaeomagnetic data source” or suchlike.

Column title changed to ‘AMS data source.’

3. There is a major omission with regard to the Ardara Pluton, the U-Pb zircon age of which has been revised to 431-424 Ma by Archibald et al. 2021 (Geological Society of America Bulletin, 133, 2335-2354). This has implications for the tectonic setting assigned to that pluton in the subsequent tectonic analysis later in the manuscript.

New age now included in Table 1 and considered for our proposed model. We completed the original data collection for this work in 2020 so thank you for bringing this more recent study to our attention.

Line 201: surely this should be 2.3.1?

Correct, numbering fixed.

Line 226: at some point, to provide brief additional context you need to mention that Iapetus is widely believed to have closed by northward directed subduction with the Southern Uplands representing the accretionary prism, and you need to name the colliding blocks, viz Laurentia/Baltica/Avalonia.

Specifics of names of colliding blocks and subduction direction added to the overview, as well as details on the Southern Uplands.

Line 237: the way the text is written it reads as if magma was *only* generated by Devonian slab break-off, whereas an age of 435 Ma is quoted as representing the start of this magmatic episode, so presumably the Silurian plutons are related to subduction? This needs to be clarified.

Reworded to make the relevance of Devonian slab-break off to magma generation clearer.

Line 238: Atherton & Ghani 2002 were the first to propose the slab break-off model in the Scottish Caledonides and should be cited here.

Citation added.

Line 241: there are many others who should be referenced here! For example, Jacques & Reavy 1994 (J Geol Soc London) were the first to really explore in any detail the inter-relationship between faults/shear zones/pluton emplacement in the Scottish Caledonides and need to be cited here.

Citation added.

Figure 5: misses out the Loch Loyal Complex and the Strath Halladale Granite (both substantial enough to be shown) and attend to spelling of “Ratagain”.

These have been added to Figure 5 and the spelling error fixed, thank you for highlighting.

Figure 6: there are some important labelling errors here in that the cited ages for the Ross of Mull and Ratagain plutons do not match those listed in Table 1! The authors also need to decide which of the two quoted ages for the former in Table 1 they are going to use.

Cited ages checked against Table 1. To better represent the uncertainty in ages highlighted by this helpful review an age range (from the oldest to youngest cited age) for each pluton is now shown in Figures 3, 4 and 6. This more fairly highlights the range of ages available for several of the plutons.

The Results and Discussion on the Massif Centrale plutons all seem to be well written and plausible. The structural setting is relatively simple and the palaeomagnetic data are consistent with the field observations. On the other hand, the section on the Caledonides is more problematic for various reasons which are detailed below.

Thank you. Individual issues with the Caledonides addressed below.

Figures 12 & 13:

1. As mentioned above, the Adara Pluton needs to be moved down into the 430-420 Ma area of the figures.

The Ardara Pluton has been moved down to this region of the figure based on this new dating, thank you for bringing it to our attention.

2. The Ratagain Pluton has a cited age of ~425 Ma in Table 1 and thus needs to be moved down into the 430-420 Ma area to be consistent. I presume its current position in these figures reflects the wholly erroneous view expressed in Lawrence et al. (2022) that this pluton was emplaced during the Devonian Acadian event. I reiterate my comments in my review of that paper – there is absolutely no objective evidence to support the suggestion that the cited ~425 Ma age (from Rogers & Dunning 1991) was obtained from xenocrystic zircons.

The full range of ages has been added to Table 1 and the full spread of ages for Ratagain plotted on Figures 12 and 13. The Ratagain pluton has indeed been assigned to the Arcadian event based on the work of Lawrence et al (2022), which might be arguable but still remains a valid alternative in the

absence of newer publications disproving that dating. Also, our AMS results from the Ratagain granite are consistent with AMS data from other plutons within the assigned time range, making the 425 Ma age for the Ratagain plausible. This paragraph however has been edited to make the uncertainty clearer and to more clearly state why this position has been assigned.

The assignment of Omeý, Ardara and Ratagain to the 430-420 Ma period is consistent with the independently obtained field evidence which relates their emplacement to strike-slip displacement along major faults. (NB: it is important to point out that these were likely emplaced into dilational/transensional jogs along these faults). However, I am not aware of any field evidence that links the Ross of Mull to any demonstrable structures that could be related to transpression/strike-slip.

Mention to transensional jogs added. The tectonic setting for the emplacement of the Ross of Mull granite was reported from Soper and Woodcock (2003) and it is beyond the scope of this paper to (re)discuss this further.

Line 731: why mention Criffel at this point? I thought you were discussing Omeý and Ross of Mull.

Criffel here was a typo, has been corrected to Omeý, thank you for highlighting.

Line 763-769: none of this makes any sense. The quoted ~425 Ma age for Ratagain indicates that it was emplaced in the earlier transpressional bracket, NOT the intervening transensional period.

Section reworded and the spread in ages better highlighted. The Ratagain granite has been placed in the Arcadian transpression based on our pluton scale AMS plot, which is comparable to other transpressional plutons and the work of Lawrence et al. (2022).

Figures 14-16: and associated discussion – all this needs to be amended/checked in various places given that some of the plutons (see above) were erroneously assigned to incorrect time slots.

The discussion of Figures 12 and 13 has been rewritten to address the revised ages and provide a better overview of differences in the proposed tectonic framework. We have made our assignment of plutons to each tectonic regime based on their AMS fabric clearer, showing how we are accounting for uncertainty in timing of tectonic changes and the dating of the individual plutons. Therefore, our overall assignment of the plutons to the tectonic regimes for Figures 14 and 16 remains unchanged- the revised age data has moved the Ardara pluton down, requiring us to support an earlier end of the Arcadian transpression (425 vs 420 Ma), in agreement with several authors

(see references on Figure 12 or 13). Ratagain remains within the Arcadian transpression based on the work of Lawrence et al. (2022).

Comments by Reviewer 3 with Authors' Response

Reviewer Three (Eric C. Ferré)

General

How is the regional tectonic strain known? If the strain markers of regional deformation are in the host-rock, then, these markers do not necessarily represent internal strain of the pluton. By analogy, in structural geology, we distinguish between active and passive markers of deformation and anything in-between. A pluton could be considered depending on the rate of crystallization (*i.e.*, solidification) as an active marker that deforms with the host-rock or alternatively, as a passive marker that may not experience internal deformation. Between these two members, a pluton could have variable degree of coupling between the host-rock and the intrusive body. In the literature, there are cases of so-called syntectonic plutons that are very much elongated parallel to regional foliation (*e.g.*, Ferré et al., 1995) and others that are barely elliptical (*e.g.*, Djouadi et al., 1997)

This study has chosen regions (*e.g.* Scottish and Irish Caledonides) where the regional tectonic evolution is relatively well known, or at least there is a fairly consistent consensus across the literature to compare the AMS fabric too. We agree that the internal strain within a pluton might not be the same as the regional tectonic strain but could also result from a combination of “magmatic” forces on the pluton, as outlined in Section 2.1, but this is actually what we tested in this study: can granitic plutons effectively record the regional tectonic stress active during their emplacement?

Specific

Line 35: while it makes sense to quote relatively recent investigations by leaders in the field of granite fabrics (Bouchez, Paterson), it would be preferable not to omit the foundations built by Pitcher, Marre and many others in this area (references suggested)

Various additional citations were added, thanks for the suggestion.

Line 38: by the same token, the work of Borradaile on magnetic fabrics is certainly worthy of recognition (reference suggested)

Citation added.

Line 81: How large is the range of temperature? Are you referring to the interval of crystallization here? If so, you should probably cite experimental petrology references.

Typically, around 800-1000 °C with 2-5 wt% H₂O gives a crystal-melt ratio that

permits Newtonian flow. This detail and suitable references (Vigneresse et al. (1996), Petford et al (2000), Vigneresse and Clements (2000), and Petford (2003)) have been added.

Line 85: Another good example of multi-stage emplacement is in the Toro Complex of Nigeria (Délérís, J., Nédélec, A., Ferré, E., Gleizes, G., Ménot, R.P., Obasi, C.K. and Bouchez, J.L., 1996. The Pan-African Toro Complex (northern Nigeria): magmatic interactions and structures in a bimodal intrusion. *Geological Magazine*, 133(5), pp.535-552.)

Thank you for the suggestion, but whilst examples of this phenomenon are interesting, we are just outlining the theory at this juncture, so Paterson is probably sufficient though we have added “e.g.” to it.

Line 117: Typographic error at “considers” ...

Corrected.

Line 134: I do not think that this claim that the AMS is a reliable indicator of strain history is true. At best the AMS integrate all steps of deformation but cannot provide information on incremental steps of deformation, especially because even small strain obliterates previous fabrics (e.g., Benn, K., 1994. Overprinting of magnetic fabrics in granites by small strains: numerical modelling. *Tectonophysics*, 233(3-4), pp.153-162).

Amended to ‘indicative of strain.’

Line 132: Experiments on magmatic systems show that AMS can arise with no deformation (Huang, F., Lundstrom, C.C., Glessner, J., Ianno, A., Boudreau, A., Li, J., Ferré, E.C., Marshak, S. and DeFrates, J., 2009. Chemical and isotopic fractionation of wet andesite in a temperature gradient: experiments and models suggesting a new mechanism of magma differentiation. *Geochimica et Cosmochimica Acta*, 73(3), pp.729-749), therefore it best to state that in general strain relates to AMS but not always.

Whilst this is true in experimental conditions, it is not directly relevant here because in the crust there is always strain resulting from either magma pressure, magma buoyancy, or tectonic stresses.

The chosen plutons constitute excellent choices for the proposed study.

Thank you

Line 178-180: In many plutons, the magnetic assemblage and the carriers of AMS vary across the intrusion and therefore the AMS cannot be considered a uniform marker of magmatic strain due to differences in the intrinsic anisotropy of AMS carriers ($P_{\text{biotite}} \sim 1.20$ to 1.29 , $P_{\text{amphibole}} \sim 1.665$ – Tarling, D. and Hrouda, F. eds., 1993. *Magnetic anisotropy of rocks*. Springer Science & Business Media).

Thanks for pointing out this potential ambiguity. We do not intend to suggest

that it is uniform. To clarify this point we added the following sentence. 'In this study we do not suggest that the mineralogical assemblage is a uniform indicator of magmatic strain, but that the orientation data can be used to provide a generalised view of the strain field'.

Table 1: The authors need to specify the radiometric method and material used for determining the age of the pluton (e.g., Ar-Ar whole rock). Ideally only high-temperature crystallization ages should be considered and cooling ages should be excluded.

Age data source and isotopic method columns added to Table 1.

Line 199: The actual name is Saint-Christophe d'Allier with a T.

Spelling error corrected, thank you.

Line 203-205: The syntax of this sentence needs to be revised.

Sentence revised.

Line 254: Were any of these intrusions emplaced through multiple batches of magma as it has been reported in many granitic plutons elsewhere?

- Vigneresse, J.L. and Bouchez, J.L., 1997. Successive granitic magma batches during pluton emplacement: the case of Cabeza de Araya (Spain). *Journal of Petrology*, 38(12), pp.1767-1776.
- Leuthold, Julien, Othmar Müntener, Lukas P. Baumgartner, Benita Putlitz, Maria Ovtcharova, and Urs Schaltegger. "Time resolved construction of a bimodal laccolith (Torres del Paine, Patagonia)." *Earth and Planetary Science Letters* 325 (2012): 85-92.
- Paterson, S., Memeti, V., Mundil, R. and Žák, J., 2016. Repeated, multiscale, magmatic erosion and recycling in an upper-crustal pluton: Implications for magma chamber dynamics and magma volume estimates. *American Mineralogist*, 101(10), pp.2176-2198.

Thank you for the helpful references. Whilst some of our intrusions were emplaced through multiple batches of magma, as this study focuses on comparing the AMS fabric to the regional tectonic strain field, we found that the uncertainty on the dating of the granites and the timing of tectonic changes meant no meaningful statistically significant comparisons could be made at this scale. Therefore, we focused on demonstrating that the significant tectonic changes occurring during emplacement can be seen in pluton scale AMS fabric despite these smaller scale variations.

Line 349: The accent in Lozère is incorrect.

Corrected, thank you.

Line 353: The Kamb contour diagram for the Two-mica granite is based on 9 samples only, which is probably insufficient.

Contour has been plotted to a lower significance level to account for the low sample number.

The relationship between magnetic susceptibility and degree of anisotropy needs to be added in this otherwise excellent review. The nature and existence of dependency between the two parameters K_m and P' informs on possible causes of P' variations within a pluton and between plutons.

Thank you for this suggestion but unfortunately not all papers used for this review reported these parameters. Therefore, to prevent having an incomplete set of graphs we have instead captured this information in the P vs T plots (Figures 3, 4, and 6), by colouring the data points by K_m values, in agreement with suggestions made in other reviews. The implications of this dependency is therefore expanded on in the discussion section now.

Also histograms showing the distribution of all 3 AMS parameters (K_m , P' and T) would be very useful to evaluate if a pluton is homogeneous or not.

This is a useful suggestion but as our study uses only already published data and this information can be found in the original studies, we did feel this would expand this study unnecessarily. This information has instead been captured in a more summary form by colouring data points on the P_j vs T graphs by K_m value.

Figure 7: Perhaps add latitude along the horizontal axis.

Thank you for this suggestion; however, whilst the plutons are arranged in roughly geographical order in Figure 7, the relative spacing between the associated stereonet does not reflect the plutons' true distance to avoid overlapping of the stereoplots (due to some plutons being very close to each other). As such Figure 7 is not a map but a timeline figure, where the plutons are listed in geographical order horizontally.

Line 571: What does “nicely matches” mean? Perhaps nicely is not necessary here.

Superfluous nicely removed.

Line 584: Should be tilted not titled ...

Corrected.

Perhaps a short discussion about the estimated depth of pluton emplacement is warranted. Plutons emplaced at a high structural level in the continental crust (e.g., Ferré, E.C., Wilson, J. and Gleizes, G., 1999. Magnetic susceptibility and AMS of the Bushveld alkaline granites, South Africa. *Tectonophysics*, 307(1-2), pp.113-133; Wilson, J., Ferré, E.C. and Lespinasse, P., 2000. Repeated tabular injection of high-level alkaline granites in the eastern Bushveld, South Africa. *Journal of the Geological Society*, 157(5), pp.1077-1088) tend to have less of an internal fabric than plutons emplaced deeper in the lower crust (Nédélec, A., Ralison, B., Bouchez, J.L. and Grégoire, V., 2000. Structure and metamorphism of the granitic basement around Antananarivo: A key to the Pan-African history of central Madagascar and its Gondwana connections. *Tectonics*, 19(5), pp.997-

1020).

We agree that this could be a relevant next step, but for now we feel this goes beyond the scope of this contribution. Also, this information is not available for some/most of the reported plutons, and so any attempt at this discussion would be incomplete.

Figure 17: I wonder if bootstrapping this data with ellipses of confidence would help making a more convincing visual case ...

Figure 17 removed based on other reviewer comments. Overall, we agree the visual case is not that convincing and as such more work is needed to fully make this point, which is beyond the scope of what can be done within this particular paper.

Comments by Associate Editor with Authors' Response

Reviewer Four-Editorial Review (Andy Parsons)

General

The language and information used to describe how AMS can be used as a proxy for strain needs to be addressed. There is a general running assumption throughout the paper that AMS fabrics record strain. This is not a true representation of AMS and how AMS studies can be used in structural geology. It must first be demonstrated that AMS fabrics in a given sample record the orientation of deformation fabrics, as other factors relating to mineralogy and magnetic carrier phase also control AMS. This is not acknowledged or discussed in the paper but it is a fundamental part of AMS analysis. AMS is the combined contribution of both the mineralogy of a sample and the shape, alignment and distribution of those minerals in a sample.

We totally agree with this, and we have added an extra sentence to the method to clarify this point; however, while this is critical for studies comparing different lithologies where the mineralogical assemblage can be extremely variable, in this study we focused on granites only, which contain a relatively simple and homogeneous mineralogy. So, we think any potential AMS "bias" induced by a variable mineralogy is minimum or negligible in the studied rocks.

The authors should also include a description of how mineral properties (ie. the type of magnetic carrier(s)) control AMS. For example, there is no discussion of how different carriers have different K_m and different P' values that inevitably contribute to the bulk rock AMS signal. This is important because it means that differences in AMS fabrics between samples could be a result of differences in strain, or differences in magnetic carriers, or differences in both strain and magnetic carries.

This is certainly a factor that always needs to be considered in magnetic studies (magnetic fabric analysis or palaeomagnetism). However, as explained above, in this study we selected rocks with an overall similar and simple mineralogy (i.e. granites). Although we cannot exclude some variability in the mineralogical assemblages of the selected rocks, which may have affected to some extent the magnetic fabric, we consider this to be negligible. So, the only significant variation across our different rocks may be related to the concentration of the ferromagnetic minerals, but this will mainly affect the size of the AMS ellipsoid rather than the orientation, which is the real target of our study.

Additionally the authors need to be careful with their use of terms stress and strain, which lead to statements which are misrepresenting what I think the authors are actually trying to say.

Errors made here have been correct, thank you for highlighting.

Lastly in its current form I am not convinced by section 4.3.1. “Effect of magma flow fabrics”. Firstly the definition of these end-member fabrics is unclear. Both must involve magmatic flow if they are syn-magmatic. There are many explanations for how and why the fabrics are slightly different. The authors need to provide better clarification of the definitions of the end-member and provide more convincing evidence to support their proposition of end-member fabrics or remove this section entirely. BUT I think your description of this in the conclusions is actually a more precise representation of what you are trying to say. I think you are referring to the amount of magmatic strain in each pluton and arguing that even when strain is low, presumably due to lower differential stress from the regional tectonic stress field, that you can still get meaningful and useful AMS fabrics that relate pluton emplacement and magmatic deformation to regional tectonics. I still wouldn’t call these end-members but they do show that AMS from low strain plutons can be still be used as a proxy for deformation and stress regimes.

Section 4.3.1 has been removed. We agree with this (and the other) reviews which overall illustrate that more work is needed to convincingly make the point aimed for in this section, especially defining ‘end-members’. We don’t think that work can effectively be achieved within this particular paper so have removed that section but have kept the discussion points highlighting how even in lower strain plutons the regional strain field can still be determined. Thank you very much for the helpful comments on this.

Specific

Line 38: This is not completely accurate. For example, some AMS fabric records a “distribution anisotropy” reflecting the spatial distribution of grains, rather than their preferred alignment. Similarly, the AMS of Magnetostatic Single Domain magnetite reflects the crystallography of the grain rather than its shape, and uniaxial single domain magnetite can have an AMS that is inverse to its grain shape.

We have edited this sentence and have addresses the distribution of magnetic and grain axis in the new section added on mineralogy, and specifically address single domain magnetite.

Line 59: I would argue that this comment is overreaching. In some case, the AMS tensor may provide a proxy for for strain kinematics and strain geometry,

but this is not always the case. It is important to clarify that the first step to such studies is to validate the link between AMS fabrics and deformation fabrics on a case-by-case basis.

Yes, it is worth pointing out that this is an important step. We explain our selection criteria later on and have added in further explanation here of how we assured the AMS fabric linked to the mineral alignment fabric.

Additionally, where the use of AMS as a proxy for strain is valid, AMS can be reliably used as a proxy for strain kinematics and geometry, but there are many factors that typically prevent AMS from being a marker of strain magnitude, because the P' of the AMS ellipsoid is not simply a function of grain shape alignment. At best, if it can be demonstrated that the magnetic carriers of two samples are the same, then the AMS of those samples may be used as a proxy for *relative* strain magnitude.

We are only relying on fabric orientation here – ‘relative strain magnitude in 3 dimensions’. Because of the equivocality mentioned we do not go further than fabrics.

Line 67: I understand what you are trying to say here but it needs rewording. At present this reads as though ALL mineral alignment fabrics in plutons are syn-magmatic, whereas in reality post-magmatic deformation of a pluton can also lead to mineral alignment.

Reworded to make it clear we are only talking about syn-magmatic fabrics here, thank you.

Line 74: A bit confusing – if you are making a distinction here between “regional tectonic stress” vs “regional tectonic stress and magma flow” then is the former in reference to post-magmatic solid-state deformation? Its not clear to me. This problem returns in the discussion with section 4.3.1. If all the fabrics analysed by this study are syn-magmatic then the process of forming those fabrics is always going to involve magmatic flow. What may differ is the driving force behind that magmatic flow. I am guessing here, but perhaps your definition of “magmatic flow” may mean something very specific that is not actually clear to me (or other readers) as all syn-magmatic deformation will involve magmatic flow. If its syn-magmatic deformation driven by a regional tectonic stress field, that deformation and the resulting fabric will still be formed via magmatic flow.

This line was purely intended to demonstrate that in previous studies individual plutons have been interpreted as having fabric dominated by tectonic stress, magma flow or a combination of the two. As such there is not a difference between ‘tectonic stress’ and ‘tectonic stress and magma flow’

just that in some cases there is not a clear dominance of one of these in the final fabric, whilst in some cases clear indicators of magma flow fabrics can be identified. This has been reworded to make it clearer.

Line 95: I like this section as a broad introduction but please spend some time reorganising and trimming the text to avoid instances of repetition. In some places it is a bit confusing.

Thank you for the suggestion, this section has now been significantly revised and polished.

Line 97: This section is helpful but it is incomplete as it does not discuss how mineral properties also control AMS. You discuss how AMS can relate to mineral fabrics, but the type of magnetic carries present in each sample also impart a control the AMS. This is a crucial point to get across because it means that interpretation of an AMS fabric needs to consider both mineral fabrics and mineralogy.

We addressed this issue in an earlier comment above as well as in the text by adding a short discussion on the types of magnetic behaviour and its link to AMS fabrics.

Line 98: Some repetition in the opening few paragraphs. Please consider my initial comments regarding the carriers of AMS and the relation of AMS to mineral fabrics

See above.

Line 99: You need to define magnetic susceptibility – this is not something that is widely known

Definition/equation added.

Line 104: Which parameters?

Specific parameters were outlined later on, but we have now also specified these here for clarity.

Line 116: Ellipsoid anisotropy

Corrected

Line 132: See my initial comments to the intro. This is overreaching because AMS is also controlled by mineral properties that are independent of deformation fabrics.

Again, see our reply to your general comments above. Specifically, here we have changed to 'usually a reliable indicator' to reflect this. A paragraph has also been added in the introduction about mineralogy (Section 2.2) also added as per earlier comments in this review.

It needs to be demonstrated that the AMS of a sample is controlled by the mineral fabric before interpretations of AMS as a proxy for strain can be made.

In our cases the original authors do this, we didn't want to overly repeat already published results. We have added an extra paragraph in our explanation of the selection of dataset (Section 3.1) to further clarify that we checked the AMS fabrics alignment with the mineral fabric.

Line 156: But this assumption needs to be shown to be valid.

Expanded explanation of relationship between AMS fabrics and mineral fabrics added to Section 3.1

Line 178: I think I understand what you are trying to say, but you need to clarify it more. As it stands, I do not see how you can relate AMS to applied stress based on the magnetic carrier. I think it's a matter of rewording this to more accurately reflect what you mean.

This section has been expanded based on this and other comments. Thank you for your suggestion.

Line 181: Mineral alignment of what? This is important as it demonstrates why it is better to refer to AMS as a "proxy" for strain fabrics because I would guess that here your "mineral alignment" probably refers to macroscopically observed plagioclase, or quartz, or biotite/amphibole/pyroxene, whereas the AMS is probably controlled by a ferromagnetic phase. So demonstrating how the ferromagnetic phase (or whatever you determine the magnetic carrier to be) relates to the bulk rock magmatic mineral fabric is important. For example, are ferromagnetic grains located along the crystallographic planes of plagioclase and therefore mimicking the plagioclase SPO or are they interstitial minerals that have their own SPO? As this is a synthesis of published work I do not expect you to be able to give this answer for all data used but I would expect you to discuss this problem openly and report any evidence/observations available that support your assumed correlation of AMS fabric and magmatic strain fabrics.

Very useful point – proxy will be used more often in this study. We have expanded this section to further support out correlation between AMS fabric and mineral alignment fabric.

Line 244: You need to add a bit more information:

- 1. In your stereonet, are your K1 and K3 datapoints the averaged K1 values of each site/sample or are they the collection of K1 values from all specimens from all sites/samples.**

Datapoint on the stereonet represent the average value for a sample site (i.e. the average of all samples collected at that site) as is outlined in the methods section (line 299). This explanation has now also been added to this line for extra clarity.

2. Can you explain and justify your choice (e.g., sample K1 or specimen K1) and explain the difference between the two alternatives above. It's important that the readers know that you are determining averages or averages.

An explanation of our choice is already outlined in the methods section (section 3.1) but a line has been added justifying why this choice was made.

Line 245: Plots of K_m vs P' or plots of T vs P with colour coded data points that show K_m by colour would be helpful. This would at least give an indication of whether we are dealing with purely ferromagnetic fabrics.

Yes, we include T vs P_j plots in the results section. Where the required data is available in the original publications, we now have scaled these plots by K_m values too. The implications of this is now discussed in the relevant discussion section for each region.

Line 248: This is a partial representation of what I was asking for in the introduction and it would help to have this information in the intro section instead of here. More is needed though to fully explain how one robustly interprets AMS with respect to deformation (see previous comments).

The earlier section (Section 3.1) has been expanded. See response to previous comments.

Line 271: I think you mean stress rather than strain.

Corrected

Line 273: this is an oversimplification of the problem because you are mixing up concepts of stretching and shortening directions (i.e. the strain ellipsoid) with directions of stress. For example in Tibet the regional extensional field is best represented by an oblate ellipsoid showing horizontal stretching (flattening). Similarly in a compressive regime, a plane strain fabric (e.g. triaxial) would produce a cluster of K1. I think the problem here is that shortening and stretching directions are present in both compressional and extensional regimes. So on their own, the clustering or girdling of AMS axes doesn't give a definitive distinction of extension vs compression. This either needs rewriting or removing.

Yes, we agree that on their own clusters or girdles of AMS data do not distinguish extension and compression, which is why in this section we explain our assignment is based on the agreement between the three distinct ways we are comparing the AMS fabric and the regional tectonic field. We have now added a sentence indicating that the clustering of K1 aligns with the maximum strain axis not purely extension or compression to make this clearer.

Line 281: This is fine but T is also controlled by mineralogy of the magnetic carrier so this needs to be considered when looking at samples with different T

values. Additionally, when P' is very low (e.g. <1.1) then differences between prolate and oblate fabrics become less meaningful because the degree of anisotropy is so low that the AMS ellipsoid is almost a sphere. T is most useful when P' is high and when P' can be shown to relate to the deformation fabric rather than mineralogy – those are the most robust situations to use T as a proxy for strain geometry.

Thank you for this useful suggestion. We have now added a sentence to this section explaining the limitations of using T as proxy for the shape of the strain ellipsoid.

Line 354: Fig 3d(i) and 3e(i) are duplicates of each other – looks like your missing the correct K1 dataset for one of these plots (probably 3d(i)).

Thank you very much for noticing this error, the correct dataset for Figure 3di has now been added to Figure 3.

Line 364: Looks like an even spread of prolate and oblate fabrics to me.

Reworded.

Line 414: The P' value of this pluton is extremely high. Can you explain why? I would be suspicious that this high P' may be a mineralogical affect possibly due to later alteration.

Thank you for bringing this to our attention- this certainly seems like a possibility as the P value is very high, however it doesn't significantly affect our interpretation. Mention of the high values has been added.

Line 426: See my comments previously about the use of T. In these two samples the degree of anisotropy is very low (<1.1) so the even distribution of prolate and oblate fabrics is basically a consequence of the very low amount of anisotropy. It takes very little variation in K2 or K3 to push these ellipsoids either side of T=0 but really they aren't showing a strong anisotropy and so the T value is not very meaningful.

In response to earlier comment, we have added a sentence explaining the limitations of using T and also noted this particular caveat again here.

Line 537: But they may not have recorded any strain if the mechanical anisotropy between the magma body and country rock was too high. Perhaps more likely is that progressive deformation during cooling would mean that deformation would move from the magma into the country rock as the magma cooled, crystallized and strengthened.

We believe this point is very similar to what we have written which aims to indicate the metamorphic aureole is recording deformation during the period where the magma body was cooling. We have reworded this to make this clearer.

Line 545: Perhaps consider the T parameter here. If you have strong P' and you have a prolate T = -1, the K2 = K3 meaning that sigma 3 may be parallel to K2 or K3. It may not apply here if you don't have strongly prolate fabrics but you need to consider this

Thank you for suggesting this point to consider, however we do not think we have strongly prolate fabrics here looking at the Pj vs T plots- the majority of the FMC plutons show dominantly oblate fabrics (4/7 plutons) and the others show a fairly even spread of oblate and prolate fabrics. It could be argued the Pont-de-Montvert-Borne pluton shows a slight dominance of prolate fabrics, but as this sentence is *talking about overall trends this seems to not be the case.*

Line 547: I think you need to take a step back here before getting to this point. Your data show predominately horizontal stretching yes, but to jump straight to this being extensional collapse is too much of an assumption. For example, take a look at AMS fabrics from granitic and migmatitic rocks in the Himalaya (Parsons et al 2016; Guillot et al 1993; Rochette et al 1994). These show a dominance of subvertical K3 and subhorizontal K1, and typically show a cluster of K3 but variability in clustering to girdling of K1. By your own words you would call these fabrics extensional deformation but our age constraints from these rocks tell us that this is not orogenic collapse as the orogen was and still is in a phase of convergence and crustal thickening.

There are at least two of your datasets which show that K1 is clustered whereas K3 is girdled. This means that you have a prolate deformation – this could be an indication of transpression or transtension – something to consider. You need to be more careful and accurate with the language used and the jumps from data to interpretations.

Thank you for this useful observation and we have added these possible alternatives and used a more “moderate” tone when drawing our conclusions here, instead stating our data fits with the expected extensional collapse rather than alone being indicative of that.

Line 551: Can you explain this a bit more? Are the arrows the extension directions? What were the compression directions?

Extra detail added to the figure caption to clarify confusion specifying that the arrows indicate the slightly differing extension direction in different regions of the FMC.

Line 580: This is more convincing than the French Central Massif dataset. Its clear there is a relationship between field structures and AMS – but can you go a step further and provide supporting information to back up the interpretation of extensional collapse?

This is beyond our scope, as we are not reviewing the tectonic interpretations, but just using broadly accepted models. This is a useful way to test our idea though.

Line 621: Diking is a brittle deformation so the dike orientation is important but flow within the dike is going to be affected by the shear stresses from the dyke wall. Therefore I would argue that you cant use dyke AMS to infer regional tectonics. Better to just rely on the orientation of the dyke (i.e. dykes form parallel to sigma 1 and perpendicular to sigma 3)

The intended aim of this paragraph was to make this very point that you cannot use the fabric in the nearby dykes to infer regional tectonics, as also outlined in the original paper. However, as this was unclear, and as we don't show the data from the dykes, we have removed this paragraph to avoid confusion.

Line 676: See previous comments on ellipsoid shape

Please, see previous answer to this point.

Line 691: No I would argue against this. You need both plots because the P vs T plot contains information about the magnetic carriers and the compositional controls on AMS (that both need to be considered) and that information is completely lost in the woodcock analysis

We have reworded this section to make our intentions clearer- we aim to show the importance of both as you have outlined. Our point on woodcock analysis was only to say that this analysis seems to capture the entire pluton scale fabric in place in a clearer manner

Line 746: There is no K1 alignment here so I would say that it's not appropriate to make interpretation of deformation from this. Whilst K3 shows some alignment the randomness of K1 makes it difficult to make any deformation related interpretations with certainty

Agreed, the Rathfriland fabric is much weaker, as had been noted, but we accept your point that attempting assignment based on this fabric is fairly speculative. We have reworded this bit to instead say we cannot confidently assign this particular granite based on its fabric alone.

Line 831: A discussion of the impact of mineralogy on AMS fabrics would explain how this is possible

Yes. We have referred back to the added paragraph on this (Section 2.2, see earlier replies to comments) and better highlighted how to study takes a simplified starting point and approach to allow larger regional databases to be compiled.

Line 836: I was very very confused by this section and would have recommended deletion if it weren't for your explanation in the conclusions. Please see my comment in the conclusions and rewrite this section accordingly.

Section removed, see later replies but all comments between here and Line 864 refer to the now deleted section, which is better addressed in the 'general comments' section of this report. Thank you for your time and assistance with this section.

Line 837: stress

Section deleted (see above).

Line 839: kruckenburg et al 2006 show an excellent example of this from the naxos migmatite dome

Section deleted (see above).

Line 840: this ignores the control of mineralogy

Section deleted (see above).

Line 841: this is a bit confusing – I think what you are getting at is that the stress field responsible for the AMS fabric is the sum of stresses from the regional tectonic stress field plus the localized stress heterogeneity produced during magmatic flow within the pluton

Section deleted (see above).

Line 842: no need for the word “forces” here

Section deleted (see above).

Line 844: I struggle to understand what the difference is. If deformation is syn-magmatic then the fabrics will be produced by magmatic flow. If you are making the argument that these plutons are syn-tectonic plutons then magmatic flow directions will be driven by the tectonic stress regime. The only way I can find this section to me meaningful is if you are actually comparing magmatic flow under static conditions (i.e. no tectonic stress) versus magmatic flow during deformation (but I don't think this is what you mean either). Otherwise I see no difference between the two as both of your end-member fabrics must still be formed via magmatic flow

Section deleted (see above).

Line 847: I see no difference I the fabrics shown in fig 17.

Section deleted (see above).

Line 851: There is a myriad of ways you can explain the differences between your end member fabrics. Please take a look at Kruckenburg et al 2006 where you will see just how variable the AMS fabric of a single pluton may be.

Section deleted (see above).

Line 864: I really can't see how these fabrics are distinguishable.

This section has been removed. See the response in the 'General comments' section of this review- overall we agree that we cannot clearly make the intended point of this section in this contribution so have removed it.

Line 873: Please expand this statement – I think you are implicitly talking about shearing during the syn-magmatic phase but I'm not sure. Additionally shear zone fabrics need not be parallel to the shear zone.

An example has been provided to clarify our meaning. The mention of contact parallel fabrics has been removed as the direction of the fabric relative to the contact does not affect our point that such small-scale fabrics do not prevent the overall tectonic fabric being seen on the pluton scale. Thank you for helping us clarify this point.

Line 892: This isn't quite true and contradicts what you've written above. You state that Geblin et al 2006 reports fault zone parallel fabrics adjacent to the fault zone. So I think what you are trying to say here is that small scale structures do not prevent using AMS fabrics in plutons to study regional stress fields as long as localized features such as shear zones are omitted from the sampling.

The point intended by this sentence is that in our plots we have included all datapoints from the original study including those shown to be linked to smaller scale structures, such as fault zones. We intend to show that outliers like these do not significantly affect the pluton-scale AMS. This has been reworded to better clarify our approach.

Line 904: This is a far better explanation of section 4.3.1. You are referring to the amount of magmatic strain in each pluton and arguing that even when strain is low, presumably due to lower differential stress from the regional tectonic stress field, that you can still get meaningful and useful AMS fabrics that relate pluton emplacement and magmatic deformation to regional tectonics.

Thank you. Mention of 'end-members' has been removed, to reflect the removal of section 4.3.1. but this conclusion kept.

2nd Round of Revisions

Decision Letter

Dear Ms Knight and co-authors

Thank you for resubmitting your manuscript “Testing the sensitivity of anisotropy of magnetic susceptibility (AMS) to the regional tectonic strain field in granite plutons; insights from two orogen-scale studies” to Tektonika. We have read through and reviewed all of the changes made by the authors and sought a subsequent review of your revised paper. We are of the opinion that you have done a good job addressing the previous points raised by the original reviewers and editors, particularly with respect to the introductory sections, the interpretations, and the layout. However, after careful consideration, we have decided that further revision is required.

In their response to the reviewers, the authors indicated that they disagreed with a number of revisions requested by Reviewer 2, Rob Strachan (Rob is happy to be identified). Consequently we asked Rob to conduct a second review of your paper, focusing on these responses. Rob Strachan has raised a number of important issues within the latest revision that relate to the authors’ use and description of geochronology and published literature. We agree with the reviewer’s position and request that the authors carefully consider these points, particularly with respect to how geochronology is used. Additionally, we have identified a small number of issues that need to be addressed, listed as comments in the manuscript commented on by associate editor Andy Parsons.

The comments provided by the reviewer include clear instructions on what is needed and so it should not be too much work for the authors to address these points. Rob has also kindly indicated that he is happy to be contacted directly, should the authors wish to discuss any of the points he has raised.

We ask that you carefully address and respond to each of the points raised by the reviewer and the associate editor in a response letter, and we ask you to submit a copy of your revised manuscript, with all changes clearly marked, and a clean version, during your resubmission.

We hope that you will be able to address the reviewers' concerns and submit a revised manuscript within 2 months (please note that Tektonika does not impose a time limit on submitting revised manuscripts, this suggested timeline is provided as a guide only). If you require additional time for your resubmission, please don't hesitate to get in touch with the editorial team to discuss a revised timeline. Feel free to reach out at

any stage if anything is unclear.

We look forward to receiving a revised copy of the manuscript by 30th July.

Thank you for giving us the opportunity to consider your work.

Yours sincerely,

Andy Parsons, PhD, Associate Editor - Tektonika

Janine Kavanagh, PhD, Executive Editor - Tektonika

Reviewer 1

(see attached pdf)

Associate editors reviewer

(see attached Word doc)

Comments by Reviewer 2 with Authors' Response

Dear Editor and Reviewers,

We greatly appreciate the time you have spent helping us to improve this manuscript and thank you all for your comments. Below are our replies to your individual comments, with the comments as red bullet points and our replies in black underneath. We have addressed all the points you raised as requested and believe this has left our manuscript much improved.

Again, we thank you for your time on this.

Hazel Knight and co-authors.

Reviewer Two- Rob Strachan, Round Two

1) (Relating to previous comments on grammar)

- Good – but “Acadian” is misspelt as “Arcadian” in some places.

Thank you for highlighting this mistake, it has been amended. The Arcadian is a shopping center here in Birmingham, so I must have mistyped that!

- Line 1242 in track changes version.....poor grammar..... ”you move”??

Amended

- Line 1381 in the track changes version..... the Acadian orogeny led to a short period of transtension? Surely you mean transpression?

Thank you for highlighting this error; this has now been corrected.

2) (Relating to Table 1)

- This is still incomplete – by isotopic method used I meant that if U-Pb zircon it should be made additionally clear whether the method is TIMS, LA-ICPMS or bulk fraction. This is important for evaluating the reliability of results.

Thank you for clarifying your meaning here, and apologies for not fully understanding this previously- the column in Table 1 has been updated to include the method where possible. Several of the dates are reported as from ‘personal communications’ (e.g. for the Pont-de-Montvert-Borne Pluton) or unpublished master’s thesis (e.g. for the Margeride Granitic Complex) so unfortunately, we cannot provide further information

on these.

- There is a question mark opposite a ?400 Ma age for the Rosses pluton, this needs to be addressed. Also I am sure that there is a K-Ar age in the literature for the Ross of Mull granite (Beckinsale & Obradovich 1973?).

This was a rough age determined from contact relationships with other plutons in the Donegal batholith that we used as the best available estimate before you brought the work of Arichibald et al 2021 to our attention (I originally completed this work for my MSci project in 2020 so pre-dating this). This date has now been removed since we have reliable dates. Those K-Ar dates for the Ross of Mull have also now been included in Table 1 for completeness, thank you for highlighting.

3) (Relating to Table 1)

- Unfortunately, the location of the Loch Loyal Complex is incorrect, it is way too close to the Moine Thrust

This has been corrected.

4) (Relating to Figure 6)

I am not sure that I indicated that there were any *significant* uncertainties in the ages of any plutons! Normal practice would be to take any relatively recently obtained U-Pb zircon ages (anything done over the last 30 years using TIMS or LA-ICPMS) as representing the age of emplacement and in almost all cases this seems very straightforward. Obviously, there are some competing zircon ages such as those for the Ross of Mull, but these have overlapping errors.

I am not sure what purpose is served by showing the age range of available isotopic data for each pluton in these figures. It inadvertently could be taken to imply that these ages represent the *duration* of pluton emplacement and crystallization. Furthermore, in the case of the Caledonian plutons it suggests that you are giving equal weight to modern U-Pb zircon studies and some very old K-Ar and multigrain bulk fraction studies. Depending on the depth of emplacement of each pluton, the K-Ar ages could simply represent cooling through a blocking temperature, potentially many myr after emplacement.

Multigrain bulk fraction zircon studies (Pidgeon & Aftalion 1978) have been proven to be unreliable time and time again. Different ages mean

different things, some of which may be meaningful, others not.

The approach of quoting the age range of isotopic data in figures 3, 4 and 6 seems at odds with the fact that by the time we get to Figure 12 for the Caledonian plutons (e.g. Ardara and Rosses) you now are clearly going with the U-Pb zircon ages as being the best indicator of the emplacement age (yet do not adopt this approach for Ratagain of course). Go with the zircon ages from the start and put your preferred emplacement ages in Figures 3, 4 and 6. You need to be consistent.

Thank you for this helpful comment, we agree with your position and have changed these figures accordingly. In the originally submitted manuscript there was one age per pluton on Figures 3, 4 and 6 and 12, but we changed this in an attempt to address reviewers' comments, especially focusing on Ratagain, so that issues/debates about which dates to use could be brought up later in the discussion rather than here where we are just presenting the data. However, I agree with your point that this could be taken as implying a duration, and as you say brings in ages which we later discount as less reliable during interpretation. We have reverted to specifying a single date here, using modern U-Pb dates where possible as before. Where there are multiple U-Pb dates (e.g. Ross of Mull, Rocles Pluton) we have taken the most recent study, if the errors are overlapping. This is not the case for the Ratagain granite, so both are specified here. The position of Ratagain in Figure 12 has been better explained in the discussion (see response to later comments).

5) (Relating to Figure 12/13)

As stated above, you are giving equal weight to isotopic ages of varying quality and significance.

As explained above we have made it clearer that we are favoring modern U-Pb dates as you have suggested. We address the specific issues with Ratagain in reply to your final comment

6) (Relating to the position of the Ratagain granite on Figure 12/13)

- See comments below at base of review.

See reply at base of review

7) (Relating to the position of Ross of Mull granite on Figure 12/13)

- I have re-read Soper & Woodcock (2003) and cannot find any reference to

this pluton. The only detailed emplacement study is that of Zaniewski et al. (2006) in the *Scottish Journal of Geology* which demonstrates that the Ross of Mull pluton was emplaced during extension – NOT transpression. I guess you could get away with putting the pluton into the post-420 Ma transtensional field and suggest that it was emplaced into an extensional zone within an overall transtensional regime.

Correct- Soper and Woodcock do not mention this pluton, but they are talking about the tectonics at the time period the Ross of Mull granite was emplaced. I apologize if this reply to your previous comment was unclear, I simply meant to set out that on this figure we are comparing the geochronological ages for the plutons with the regional tectonic history set out for this period by Soper and Woodcock. We compare all plutons here in this way.

More specifically in this case Zaniewski et al., (2006) invoke normal displacement on the Iona fault to explain floor down drop as a possible space making mechanism for the Ross of Mull granite. However, such floor down drop does not necessarily need extension. On this end Petronis et al. (2012) subsequently discovered AMS fabrics more consistent with E-W compression and developed Zaniewski et al's model to include this aspect.

8) (Relating to Lines 763-769 (Ratagain))

- See comments below at base of review

See reply at base of review

9) (Relating to the Ratagain granite)

- The assignment of Ratagain to the Acadian transpressional period still seems poorly justified. You acknowledge that the AMS data indicate emplacement during transpression, so this could be either the Silurian (late Scandian) or Devonian (Acadian) transpressional events. The U-Pb zircon age of 425 Ma supports the former, and your preferred intrusion age of 395 Ma in the middle of the latter does not correspond to any published isotopic data. So the question arises what extra evidence can be summoned in support of an Acadian age? In lines 1394-1395 in the tracked changes version you state that Lawrence et al. 2022 proposed an Acadian age on the basis of petrographic and magnetic evidence. It is clear that the magnetic evidence is ambiguous (see above) so that only leaves petrographic evidence. What is the petrographic evidence that would enable Acadian plutons to be confidently distinguished from older

Scandian plutons? Your treatment of Ratagain seems so anomalous that you need to devote a paragraph or so justifying the proposed Acadian age. This means not just referring readers to “the work of Lawrence et al. 2022” but taking a paragraph to explain in more detail the lines of evidence that those workers used to support an Acadian age.

We appreciate your comments on this section. We have added in that detailed paragraph setting out our reasoning as a response to your previous comments (Lines 993 to 1028 in the track changes document).

An important point to make here is that, as you outline above, we all agree that Ratagain AMS data indicate emplacement during transpression. This is really the only point that is directly important for our conclusions because, as you also state, the geochronological data suggests it could be either the Scandian or Acadian transpression event. Assignment of Ratagain to either of these periods has no significant effect on our conclusions; both the geochronology and AMS fabric are compatible with the regional tectonic framework, as both indicate emplacement during a period of transpression, suggesting that the Ratagain granite too is recording the ambient tectonic strain field at a pluton scale. However, it is more for completeness we must assign Ratagain to one tectonic period. So I think it is important to state that if we instead assign Ratagain to the Scandian transpression it does not change our further conclusions, though of course this doesn't mean it isn't important to get this bit correct!

The specific geochemical/petrological evidence which we are using to assign Ratagain to the Acadian has been summarized in the manuscript as follows; *“Lawrence et al., 2023 analysed >100 samples from the complex, overall finding an enrichment in large-ion lithophile elements (LILE's) (e.g. Rb, Ba, K and Sr) and a depletion in high-field strength elements (e.g. U, Th, Nb, Zr, P, Ti) resulting in high Sr/Y and La/Yb ratios. This geochemical signature is characteristic of melts generated during slab-break off. Furthermore, Lawrence et al 2023 also plotted tectonomagmatic discrimination diagrams (Rb vs. Nb + Y, Rb vs. Ta + Yb, Nb/Sr vs. Sr/Y and La/Yb vs. Sr/Y) (see Figures 11 to 12 in Lawrence et al., 2023) all of which showed Ratagain plotted within the slab failure field. Slab failure has frequently been suggested as a method of melt generation for the late Caledonian granites (e.g. Atherton and Ghani, 2002; Miles et al, 2016; Archibald et al, 2022), occurring following final convergence. On this basis we therefore assign the Ratagain granite to the younger Acadian transpression- allowing us to fit both the transpressive tectonic regime recorded by the AMS fabric and the geochemical slab break off signal within the accepted regional tectonic framework”*

We believe this geochemical evidence of slab break off is strong enough to assign Ratagain to the Acadian event as slab-break off is a well-documented part of the tectonic development in the Late Devonian following final ocean closure.

Combined with the AMS fabric evidence (Lawrence et al, 2022 and the plots here) that show emplacement in a period of transpression this better fits with an Acadian emplacement than the alternative. The full geochemical data is reported in Lawrence et al., 2023 which we now cite here. We have also made it clearer in the text that this is a more tentative assignment than for other plutons where this controversy in ages doesn't exist and highlight that overall the whole debate highlights the need for more geochronological work on the complex, which can hopefully resolve this in the future!

We believe this addresses your comments as we have, as requested, added a paragraph of explanation of our evidence here, as well as referring readers to more detailed studies so as not to make the explanation too long! We have also been clearer as you have highlighted that this is a slightly different assignment than for the other plutons where the age data is less varied.

Comments by Associate Editor with Authors' Response

Editor-Andy Parsons- Round two

- Line 43: Incorrect - syn-magmatic deformation can and regularly does create macroscopic foliations visible and measurable in the field

Thank you for highlighting this, it was not our intention with this sentence which we have now removed to avoid confusion.

- Line 77: Authors have not carried out my request to use the term "magmatic body forces"

We have aimed to carry out your request to the best of our ability - the term magmatic body forces is used in this sentence as we believed was required. We have now also changed 'magma flow forces' to just 'magma flow' in the second sentence to further disambiguate this. I apologies for any previous errors in attempting to carry out this request!

- Line 86: The former is also true for post-magmatic deformation. Also I reiterate here - magma flow is not an appropriate term for what you are describing as syn-tectonic and post-tectonic magmatic bodies will both display fabrics produced by magmatic flow. The driving forces may be different but the fabric is still produced by magmatic flow in each case. If the fabric formed at sub-solidus conditions then is formed via magmatic flow. If it formed at solidus (i.e. post-magmatic) conditions then the fabric is a solid-state fabric and not produced flow of magma.

Thank you very much for highlighting this important point, and we fully agree with you. We have reworded this to say 'fabrics formed by magma emplacement show no relationship with host rock fabrics' which we believe better explains our intended point here and avoids any confusion on the causes of the magma flow. As such we have removed any direct mention to 'magma flow' here.

- Line 197: The AMS axis orientations can tell us about strain kinematics but the directions on their own don't tell us about the strain magnitude

We agree, thank you for highlighting we have changed 'orientation' to 'magnitude'.

- Line 251: a girdle fabric from a compressional regime would indicate a flattening strain, but compression and flattening are not synonymous. As an example, Parsons et al 2016 (Tectonics) display both girdle and point maximum K1 fabrics from the metamorphic core of the himalaya, all of which formed under a compressional regime. It is inaccurate to suggest that one should expect an extensional regime to produce a point maximum K1 fabric and a compressional regime to produce a girdle K1 fabric. Your FMC data

demonstrate the confusion and inaccuracy here as you have fabrics with point maxima of K1 and K3. By your explanation here, that would indicate that the fabric cannot be distinguished as either extensional or compressional

Thank you for helping disambiguate this paragraph. We have re-written this section to make it clearer that we are setting up a very simplified starting point with reference to end members in a homogeneous ambient tectonic strain field, and that complexity exists- including triaxial settings with clusters (point maxima) of both K1 and K3 as you highlight. We also thank you for the helpful reference included here to help us make this point, highlighting that this is not always the case, especially in heterogeneous settings (e.g. mid crustal shear zones).

- Line 543: You still need to explain why you are happy to continue using this AMS data to make tectonic interpretations despite the possibility of alteration products controlling the AMS fabric

Thank you for encouraging us to look again at this graph- we now see that these Pj values have been reported as a percentage anisotropy rather than as direct values as for the other plutons (i.e. an anisotropy percentage of 6% was plotted as a value of 6 rather than the value of 1.06 as it should have been). This has now been corrected so we are plotting all the plutons the same- thank you very much for catching this error on our part! With these plotted correctly the Pj values are not significantly high so there is no indication of alteration and we have removed that sentence, addressing this issue.

- Line 673: Previously you have suggested that a cluster of K3 is indicative of a compressional regime. I suggest that you move away from making interpretations of the principle stress component directions as I am not convinced that the AMS data allow you to do this.

We have removed any mention to principle stress component directions in this sentence.

- Line 955: Why is this expected? AMS work from the Himalaya typically shows subvertical K3 directions from a compressional regime

We agree with your point that expected may not be the best word to use here to convey our point. We have changed this sentence to 'as may occur during homogenous orthogonal compression' to better convey our message.

- Line 966: Based on what? Again, I refer to AMS from the Himalaya showing K1 clusters produced in a compressional regime. I think you should move away from assuming that ams axis distributions (clusters or girdles) can distinguish extensional from compressional regimes

We have reworded this part to remove the link between AMS axis distribution and tectonic extension here and instead talk about the orientation of the stretching component of the fabric. This removed the ambiguity you have highlighted.

- **Line 991: If its highly uncertain (as we both agree) then such interpretations above are meaningless. Please remove the above interpretation and instead state that no interpretation can be made due to the absence of a well-defined K1 fabric**

The section has been removed as we both agree it is ambiguous. A sentence as you helpfully suggested stating that the absence of a clear k1 fabric means we cannot make a tectonic interpretation has been added to the above paragraph (lines 960 beginning 'During the following period of transtension..').

- **Line 1007: Reviewer 1 has provided some clear instructions for this section which need to be carefully considered. In addition to their comments, I would argue against the way this statement is presented. A correlation in AMS fabrics does not immediately indicate a causative relationship. The authors need to expand their interpretation here, stating why/how published data support this interpretation.**

We thank both you and Reviewer 1 for your help with this section, which we believe is significantly improved following your discourse on this. A paragraph has now been added explaining our reasoning for this assignment- we agree the AMS fabric alone does not indicate a causal relationship, so have done a more thorough job at expanding on the geochemical and petrological work (Lawrence et al, 2022 and Lawrence et al, 2023) which backed up our assignment. We have also made it clearer that all that matters for our conclusions is that Ratagain was emplaced during a period of transpression, which as Reviewer 1 highlights from the dates alone could either be the Acadian or Scandian transpressive periods. Whilst we do believe the evidence provided by Lawrence et al (expanded upon in the inserted paragraph) is sufficiently strong to assign it to the Acadian period we also make it clearer in the text that either age possibility is a potential option and compatible with our data and conclusions. Overall, this debate highlights the need for further dating of the pluton!

Acceptance Letter

Dear Ms Knight and co-authors,

We have now reached a decision regarding your submission to TEKTONIKA “Testing the sensitivity of anisotropy of magnetic susceptibility (AMS) to the regional tectonic strain field in granite plutons; insights from two orogen-scale studies”.

The Associate Editor, Andy Parsons, and I are now satisfied that the comments raised by the reviewers and through the editorial process have now been addressed. We are therefore happy to inform you that the paper has been now ACCEPTED to be published.

Your paper will now move to the Typesetting phase and our production team will be in touch with you to discuss this soon.

We look forward to publishing your manuscript and hope you will consider TEKTONIKA again in the future.

Congratulations, and thank you again for submitting your work to Tektonika!

Yours sincerely,

Janine Kavanagh, PhD, Executive Editor – Tektonika

Andy Parsons, PhD, Associate Editor - Tektonika