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Interactive comment on "Mechanical models to estimate the paleostress state from igneous intrusions" *by* Tara L. Stephens et al.

Anonymous Referee #1

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This paper is interesting and well written, providing a mechanical model for using sill intrusion attitudes and opening vectors to constrain magma pressures and stress states during emplacement events. Admittedly, the approach and model is not completely new, as it builds off original work from authors such as Jolly and Sanderson (1997). However the study is timely and provides an advance in how we view sill intrusion systems generally.

There is currently a predominant view in the scientific literature that sill intrusions result primarily as a result of bedding and layer heterogeneities. Although this appears to be the case for some examples, it is equally possible that many sill systems form in compressional settings, where regional least compressive stress is vertical. Furthermore, dike intrusions are frequently used as paleostress indicators. The same should ap-

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Discussion paper

ply to sills also, particularly when accompanied by careful and detailed field kinematic inspections, as has been applied in this study.

Recent work from this group has already applied sills as paleostress indicators, and this paper focuses largely on detailing the methodology for such an approach, whilst providing a nice field dataset for proof of concept. It is a timely piece of science, which will benefit future structural, tectonic and volcanological studies investigating tectonic-magmatic interactions.

Below I provide some general and specific comments. The paper is in an excellent state and appears to have gone through a number of reviews already, so to me the manuscript requires only minor corrections. I look forward to seeing a final published version of this paper, which I think will provide a useful resource for future intrusion studies.

GENERAL COMMENTS

1) As discussed below, there may be potential for this method to be misapplied in the absence of detailed investigations of the timing and history of intrusive events, and the authors should explicitly discuss the critical field observations needed before applying the approach. The authors have previously done extensive and careful field work in this study area (e.g., Walker et al., 2017), so I believe the approach is valid for the San Rafael field. However, the method assumes that the stress field remains constant during emplacement of the measured intrusions, and thus the magnitude of fluid pressure (i.e., magma) is the critical parameter that determines the spread of the intrusion attitude data. With this in mind, the method also requires the emplacement of the measured intrusions, which exhibit a variety of attitudes, to be closely spaced in time (i.e., L137: "created during the same dilational event"). If one were to take this approach to a different field setting, it therefore may be misapplied if the timing of events is unknown.

So with this in mind; in contrast to the model presented in this study, what if the magnitude of the fluid pressure remained relatively low (thus restricting the range of intrusion attitudes at any given time and place), but the orientation of the stress field varied both temporally and spatially? Such a scenario would seem really likely under a classic cone sheet model for example.

The authors somewhat address this point on L 267-285, but I think a little more discussion is needed so that readers can explicitly see the potential pitfalls with the approach, and be shown how to deal with these with detail field observations and measurements. For example, what is unique about this study, compared to Jolly and Sanderson (1997), is that the authors also present the attitudes of fractures exhibiting compressional shear failure, thereby tightly bounding the ellipse in the stereonets that provide constraints on fluid pressures. The interconnectivity between intrusions of different attitudes is also illustrated, suggesting they formed near-contemporaneously. I feel that careful measurements like these robustly support the model, and future studies could also consider this approach.

2) It would be useful for the reader to also have some quick background on why sills have been omitted from these types of paleostress analyses in the introduction. This could be included as a few sentences immediately after the comments on lines 46-48. To me, this issue stems from the problem that (1) sills in many cases must be fed by dikes, and are often observed in regions which are thought to be extensional. Thus if we consider stress as the primary control on sill orientation, then the least compressive stress must be both vertical and horizontal in the region where sills are fed, which is often in an extensional setting. The effects of mechanical layering essentially act as a work-around for this paradox. (2) Sills are often observed intruding sedimentary layers, although this is no surprise as sedimentary layering is often horizontal. I know this group has brought up points such as these in recent papers, but I feel that it is important to bring these points to the forefront to provide broader context for the paper.

Specific comments

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L 115: Please consider the terms “extensional” and “compressional” as the subject matter is intrusions rather than faults.

L 141-143: The other possibility is that it is multiple events where the orientation of the principle stresses has varied. I don’t think this is the case in the paper, but it should not be overlooked. See Comment #1.

L 162-163: No direct feeder relationships are observed, which is very rare in nature anyhow, but they have been inferred. For example, Figure 3 of Richardson et al. (2015) points to potential dike feeder based on sill thicknesses. Additionally, the thickness distributions of these sills do suggest potential NNE-SSE-trending feeders, which is parallel to the dikes (i.e., Walker et al., 2017). Finally, the Richardson et al. paper, which details the broad distribution of the sills in question, seems to be omitted from this study. It should be included and cited throughout accordingly.

L 325: As suggested here and other locations in the text, the sills are often assumed to be intruding pre-existing structures when their attitudes are oblique to the principle stresses. What about faults and fractures generated at the propagation front of sills related to uplift of the overburden (e.g., Fig 7 of Thomson 2007)? I don’t think these would count as pre-existing, but would be oblique to the least compressive stress, and thus fit with the model presented. Note that they don’t have to be as high-angle as the ones in the figure referred to.

Figure 1: Change normal fault regime, to “extensional regime”. Change thrust fault regime to “compressional regime”. We are focusing on intrusions in this paper, and not faults.

Figure 3: Same as figure 1. Change to “extensional regime”, “strike-slip regime”, and “compressional regime”.

Also, I think in this figure there needs to be a little more explanation in the caption. From my understanding, if the pole to the fracture plane is situated in a certain color portion of the stereonet, then the "color" indicates the opening angle (μ). So if the pole of the fracture plane is in a black region, it will have an opening angle between 80-90 degrees (and subsequently a large true thickness). If the pole of the fracture plane is in a yellow regime, it will have an opening angle between 0-10 degrees, and have a smaller true thickness.

I doubt this is intuitive to most readers looking only at this figure, and I think the paper would benefit from taking the time to explain it in greater detail, perhaps using an example like I did above. [Note, I noticed later that the text discusses poles to planes, but I still think it should be explained first thing when the reader first views the caption.]

Recommend references

Richardson, J. A., Connor, C. B., Wetmore, P. H., Connor, L. J., & Gallant, E. A. (2015). Role of sills in the development of volcanic fields: Insights from lidar mapping surveys of the San Rafael Swell, Utah. Geology, 43(11), 1023-1026.

Thomson, K. (2007). Determining magma flow in sills, dykes and laccoliths and their implications for sill emplacement mechanisms. Bulletin of Volcanology, 70(2), 183-201.

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