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# Reply to Jurdy & Stefanick comment

Ted Chang[1], Joann Stock[2] and Peter Molnar[3]
[1]*Department of Mathematics, University of Virginia, Charlottesville, Virginia* 22903, *USA*
[2]*Harvard University, Department of Earth and Planetary Sciences, Cambridge, Massachusetts* 02138, *USA*
[3]*Department of Earth, Atmospheric and Planetary Sciences, Massachusetts Institute of Technology, Cambridge, Massachusetts* 02139, *USA*

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We disagree with virtually all of what Jurdy & Stefanick have written. Part of our disagreement stems from personal opinions about what is 'simple', 'arbitrary', 'artificial', 'undesirable', etc., but other disagreements are more profound and reveal a very different understanding of finite rotations. Jurdy & Stefanick raise two basic objections. One concerns statistical questions that were not meant to be part of Chang *et al.* (1990). The other addresses the main issue of our paper, the parametrization of uncertainties of rotations. They suggest that both our approach is flawed and that theirs, outlined in Jurdy & Stefanick (1987), is better. Except possibly for their opinion of what constitutes a covariance matrix, we try not to indulge the reader with long discussions of questions of personal preference, but instead to confine our response to these basic questions.

Jurdy & Stefanick are clearly annoyed that we did not acknowledge that Jurdy & Stefanick (1987) used a 'covariance matrix' to describe uncertainties in reconstructions. Because the purpose of our paper was to discuss how to parametrize the uncertainty, not how to calculate covariance matrices, there seemed no need to review their discussion of covariance matrices (Jurdy & Stefanick 1987), or Chang's (1987, 1988). In fact, the use of covariance matrices has been standard statistical practice for decades. We apologize to Jurdy & Stefanick for somehow giving them the impression that we introduced the concept of a covariance matrix to this problem. At the same time, the question of covariance matrices is rendered additionally complicated by our disagreement with Jurdy & Stefanick that their covariance matrix is, in fact, a statistically rigorously defined and meaningful covariance matrix. Indeed, as discussed below, their covariance matrices exhibit certain anomalous behaviour which would indicate that a statistical justification for it does not exist. This disagreement was communicated in a letter from Chang to Jurdy in 1987, but their comment forces us to make the disagreement public.

A statistically meaningful uncertainty of an inferred quantity obviously depends upon the uncertainties in measured quantities, which are usually estimated from misfits or deviations from predicted values. To use such uncertainties to estimate an uncertainty in an inferred quantity, requires the deduction, or more likely the assumption, of a probability distribution for the errors in the measurements. In the context of plate reconstructions, a statistical analysis must include both (i) a discussion of the assumed probability distribution for the positions of magnetic anomalies and fracture zones, or whatever data are used to infer the rotation, and (ii) a reasonably plausible derivation of how errors in the data are transmitted to errors in the rotation.

The discussion of the probability model (i) is not simply an arcane point for the amusement of the mathematicians. Most statistical techniques rely on an assumption of independence of the errors of the data points (differences from their true values). Otherwise step (ii) becomes much more difficult. When the data points consist of selected points on the plate boundaries, the assumption of their independence is quite possibly false. Consider, for example, the use of interpreted intersections between magnetic lineations and fracture zones. Since the neighbouring intersections are interpreted from partially shared data, the assumption of their independence is clearly untenable (see Fig. 1). It follows that statistical analyses, such as those in Chang (1987), based upon an assumption of independence of interpreted intersections of magnetic anomalies and fracture zones can only be considered crude first approximations. On the other hand, assumed independence of the original identifications is much more reasonable. For this reason, we are now using the analysis given in Chang (1988).

The uncertainty in a multi-dimensional quantity is fully specified by its covariance matrix. For a random vector $\boldsymbol{t}$, the covariance matrix is defined to be;

$$\operatorname{cov}(\boldsymbol{t}) = E(\boldsymbol{t}\boldsymbol{t}^{\mathrm{T}}) - E(\boldsymbol{t})E(\boldsymbol{t})^{\mathrm{T}}, \tag{1}$$

where, $E(\boldsymbol{t})$ and $E(\boldsymbol{t}\boldsymbol{t}^{\mathrm{T}})$, the expected values of $\boldsymbol{t}$ and $\boldsymbol{t}\boldsymbol{t}^{\mathrm{T}}$, are the weighted averages of $\boldsymbol{t}$ and $\boldsymbol{t}\boldsymbol{t}^{\mathrm{T}}$ with the weights determined using the probability distribution for $\boldsymbol{t}$. For a representation of a rotation using a pseudo-vector $\boldsymbol{t}$, the probability distribution for $\boldsymbol{t}$ clearly depends, intimately and directly, on the probability distribution of the data. Consequently, if the rotation is parametrized by the pseudo-vector $\boldsymbol{t}$, the probability distribution for $\boldsymbol{t}$ cannot be asserted, but must be derived from an assumed probability distribution of the data.

Jurdy & Stefanick's equation (4) is simply a rewrite of (1) above, but neither in their present comment, nor in Jurdy & Stefanick (1987), do they discuss the probability model (i) for the data. Jurdy & Stefanick (1987) simply took uncertainties in rotations that Stock & Molnar (1983) used to illustrate geometric aspects of the uncertainties, and then fed them to their 'covariance matrix'. As a result their, Molar & Stock (1985), uncertainties in pole positions and rotation angles have the peculiar property that the addition of data would not decrease the eigenvalues of the covariance matrix (i.e. the sizes of their confidence region would not change). This behaviour is contrary to all statistical

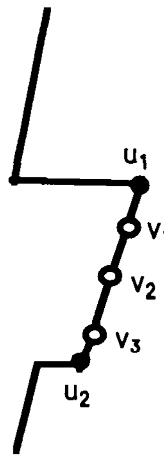


**Figure 1.** $v_1$, $v_2$, and $v_3$ (open circles) represent identified crossings of a magnetic anomaly lineation segment. $u_1$ and $u_2$ (closed circles) are the 'interpreted' endpoints of the segment. $u_1$ and $u_2$ are both determined in part through the use of $v_1$, $v_2$, and $v_3$ and hence cannot be independent. Independence of the errors in $v_1$, $v_2$, and $v_3$ is a more reasonable assumption.

experience except in pathological cases. Thus we suspect that their failure to consider (i) and (ii) is not just an unimportant omission of some mathematical formalism, but has resulted in their producing an answer which is incapable of being justified with any statistical reasoning. Because their approach is not statistical, they should not use the term covariance matrix.

The purpose of Chang *et al.* (1990) was to demonstrate that the uncertainty in plate reconstructions is best parametrized by small perturbations to the estimated rotation, not as uncertainties in the parameters usually used to describe the rotation: the pole position and the rotation angle. For Chang, the main justification for this approach is that it allowed him to prove theorems that relate assumed probability distributions of the observable quantities to uncertainties in reconstructions (Chang 1986, 1988). Accordingly, the parametrization in terms of perturbing rotations was simpler for him, for he could not prove such theorems with other parametrizations such as that used by Jurdy & Stefanick. One of the reasons that Stock & Molnar prefer a parametrization in terms of perturbing rotations is that it eliminates asymmetries in the calculated uncertainties of reconstructed points on plates. These asymmetries result from the non-commutativity of matrix multiplication and are inherent to the problem. We find it ironic that the word 'asymmetry', modified by 'arbitrary' and 'artificial', figures so prominently in Jurdy & Stefanick's criticism. Below we discuss the significance of the asymmetry that seems to trouble Jurdy & Stefanick.

Consider a plate $L$ reconstructed to a fixed plate $R$ by a rotation $\mathbf{A}$. One first forms an estimate $\hat{\mathbf{A}}$, or best fit, by some criterion. Then one uses misfits of data to estimate the uncertainty in the rotation. We used the misfit to estimate the family of rotations that combined with $\hat{\mathbf{A}}$ to yield an uncertainty in $\mathbf{A}$, while Jurdy & Stefanick advocate using the misfits to determine uncertainties in the pole position and angle. (They call this process 'jiggling' when it applies to our approach, but not to theirs, and point out that our procedure involves two steps. They fail to note that their approach also requires two steps, as do virtually all estimates of uncertainties based on misfits of data from those expected from the estimated parameters.) Because matrix multiplication is not commutative, the perturbing rotations that can be combined with $\hat{\mathbf{A}}$ without degrading the fit significantly are different from those that can be combined with the inverse rotation $\hat{\mathbf{A}}^T$ to cause the same degradation. Thus, there is an asymmetry in the way we represent the uncertainties, as Jurdy & Stefanick state. Let us consider the origin of this asymmetry, and how profoundly it affects the uncertainties in reconstructions.

Because the perturbing rotations are small, the pseudo-vectors that parametrize them do obey vector algebra without significant error, a quality denied finite rotations of a few degrees or more. Let $\boldsymbol{h}_\mathbf{A}$ represent the pseudo-vector describing a small perturbing rotation and $\boldsymbol{\Phi}(\boldsymbol{h}_\mathbf{A})$ represent that rotation, so $\mathbf{A} = \hat{\mathbf{A}}\boldsymbol{\Phi}(\boldsymbol{h}_\mathbf{A})$. We relate uncertainties and misfits of data to $\boldsymbol{h}_\mathbf{A}$ by its covariance matrix: $\operatorname{cov} \boldsymbol{h}_\mathbf{A}$. Obviously if we held plate $L$ fixed and estimated the rotation $\mathbf{A}^T = \mathbf{A}^{-1}$ that brought plate $R$ to it, the perturbing rotation $\boldsymbol{h}_{\mathbf{A}^T}$ and $\operatorname{cov} \boldsymbol{h}_{\mathbf{A}^T}$ would be different from $\boldsymbol{h}_\mathbf{A}$ and $\operatorname{cov} \boldsymbol{h}_\mathbf{A}$. For each pseudo-vector $\boldsymbol{h}_\mathbf{A}$, there would be a corresponding pseudo-vector $\boldsymbol{h}_{\mathbf{A}^T}$ such that:

$$\boldsymbol{\Phi}(\boldsymbol{h}_{\mathbf{A}^T}) = \mathbf{A} \cdot \boldsymbol{\Phi}(\boldsymbol{h}_\mathbf{A}) \cdot \mathbf{A}^{-1},$$

which merely states that $\boldsymbol{h}_{\mathbf{A}^T}$ is the same pseudo-vector as $\boldsymbol{h}_\mathbf{A}$ rotated (by $\mathbf{A}$) from plate $L$ to plate $R$. Similarly, the covariance matrices for $\boldsymbol{h}_\mathbf{A}$ and $\boldsymbol{h}_{\mathbf{A}^T}$ are related by

$$\operatorname{cov} \boldsymbol{h}_{\mathbf{A}^T} = \mathbf{A} \operatorname{cov} \boldsymbol{h}_\mathbf{A} \mathbf{A}^{-1}. \quad (2)$$

The preceding algebra and discussion are meant to show that the asymmetry called 'arbitrary' and 'artificial' by Jurdy & Stefanick is nothing more than the arbitrary, but not artificial, decision of which plate is held fixed and which is moved in the reconstruction. In one sense, this asymmetry is very large: if plate $L$ is held fixed and plate $R$ is rotated, the positions of all but at most two points on plates $L$ and $R$ will lie in different positions from those for the case where plate $R$ is fixed and $L$ rotated. We are not aware of any scientific question, however, whose solution is dependent on the choice of which plate is held fixed. Thus, we see nothing profound in Jurdy & Stefanick's concern about this asymmetry.

The same asymmetry perceived by Jurdy & Stefanick applies to three plates. Suppose plate $L$ is rotated by $\mathbf{B}$ to plate $M$, which is rotated to plate $R$ by $\mathbf{A}$, so that plate $L$ is rotated to $R$ by $\mathbf{C} = \mathbf{A} \cdot \mathbf{B}$. Chang *et al.* (1990) and Chang (1988) write

$$\operatorname{cov} \boldsymbol{h}_\mathbf{C} = \mathbf{B}^{-1} \operatorname{cov} \boldsymbol{h}_\mathbf{A}\mathbf{B} + \operatorname{cov} \boldsymbol{h}_\mathbf{B}. \quad (3)$$

This merely states that $\operatorname{cov} \boldsymbol{h}_\mathbf{A}$, which is measured in the frame fixed to plate $M$, must be rotated to plate $L$ before it is added to $\operatorname{cov} \boldsymbol{h}_\mathbf{B}$. Alternatively, we could rotate plate $R$ to $M$ by $\mathbf{A}^{-1}$ and then to $L$ by $\mathbf{B}^{-1}$, corresponding to the rotation of $R$ to $L$ by $\mathbf{C}^{-1} = \mathbf{B}^{-1}\mathbf{A}^{-1}$. In order to account for the changes in reference frame, the proper condition for symmetry is

$$\operatorname{cov} \boldsymbol{h}_{\mathbf{C}^T} = \operatorname{cov} \boldsymbol{h}_{\mathbf{A}^T} + \mathbf{A} \operatorname{cov} \boldsymbol{h}_{\mathbf{B}^T} \mathbf{A}^{-1}. \quad (4)$$

Indeed it is easily shown that in light of equation (2), equations (4) and (3) are equivalent. The apparent asymmetry for the combined reconstruction of three plates, which again troubles Jurdy & Stefanick, is nothing more than the asymmetry associated with choosing one plate as fixed.

We also deny Jurdy & Stefanick's claim that the 'moving exponential parametrization' which we use limits the geometries that can be analysed. In the 'moving exponential parametrization' independent estimates of the same rotation can be combined to form an optimal estimate using a formula which has a formal resemblance to the corresponding formula in Jurdy & Stefanick (1987). Triple junctions can be analysed and in fact better analyses than that proposed by Jurdy & Stefanick are available.

The cleanest approach to the triple junction problem is to estimate simultaneously the rotations for all three limbs. This yields information not only about the errors in each of the constituent reconstructions, but also about how the errors in the three reconstructions are related. It is possible for the reconstructions of any two limbs to be individually (statistically) feasible, but for the pair to be jointly impossible. The simultaneous estimation of the triple junction recovers information about the joint distribution of the two reconstructions.

An example of both of these techniques, the combination of independent estimates of the same rotation and the simultaneous estimation of the triple junction, appears in Royer & Chang (1991).

Jurdy & Stefanick further imply that an estimate $\hat{\mathbf{A}}$ and its associated estimated 'moving exponential parametrization' covariance matrix $\Sigma$ is insufficient information to express the errors of the estimate $\hat{\mathbf{A}}$ when $\hat{\mathbf{A}}$ is constructed using a chain of rotation links. Instead they state that the interpretation of $\hat{\mathbf{A}}$ and $\Sigma$ requires knowledge of the data and the rotation links used. This is simply not true. The matrix $\Sigma$ can be converted to a confidence region of permissible small rotations which is then left multiplied by $\hat{\mathbf{A}}$. Graphical techniques for visualizing this confidence region are available and have been used in Chang (1987) and Royer & Chang (1991).

Jurdy & Stefanick propose that when their error ellipsoids are used, different ellipsoids constructed by alternate chains and/or by possibly different authors can be compared by visually checking to see if they intersect. Since our confidence regions can be graphically presented, the same tool is available when 'moving exponential' confidence regions are constructed.

We feel bound to emphasize, however, that this approach, although simple and heuristic, is statistically incorrect for checking the consistency of two estimates. It is true that if two 95 per cent regions do not intersect, the estimates are inconsistent (*although at only an approximate 90 per cent level*). The converse assertion that intersection implies consistency is false. This can easily be seen for even the simplest statistical procedure. The proper approach to reconcile two estimates $\hat{\mathbf{A}}_1$ and $\hat{\mathbf{A}}_2$ involves calculating the covariance matrix of $\hat{\mathbf{A}}_1^{-1}\hat{\mathbf{A}}_2$ and then checking to see if the null rotation is consistent with it. If the estimates $\hat{\mathbf{A}}_1$ and $\hat{\mathbf{A}}_2$ are independent, meaning that they are derived from entirely separate data, this is quite routine. Otherwise a detailed knowledge of the data and chain of rotation links is required.

Jurdy & Stefanick question if it is not desirable to be able to compare two different rotation estimates without needing to know the data or chains of rotation links used in the estimates. We maintain that, unlike the interpretation of a single estimate, the comparison of two estimates inevitably requires this knowledge. If the same data and the same chains are used, the two estimates should be very close. On the other hand if different chains are used, somewhat larger differences are reasonable. Thus we cannot tell whether a difference between two estimates is reasonable without knowing the extent to which similar data have been used in their construction. Furthermore, if a statistically significant difference in the estimates is found, its interpretation clearly depends upon the approaches used to derive the estimates. For example, when different chains are used, such a difference might be due to a mistake in the assumed underlying plate geometry. This possibility is precluded if the same data and chains are used.

Jurdy & Stefanick explicitly question the desirability of tying error estimation to the data. This attitude is a complete repudiation of the philosophy underlying statistics. The Jurdy & Stefanick approach is simply not statistics. They should not have used terms such as 'covariance' which have established and widely used precise statistical definitions.

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