MATTERS ARISING

Polygon patterns on Europa

PIERI[1], in his study of the lineament and polygon patterns on Europa, has discussed the problem of the development of crack networks in cooling basalt flows and similar planar, contracting systems. He observed that although an ideal hexagonal pattern maximizes the reduction of strain energy per unit surface area and is therefore the most efficient pattern given an isotropic stress field, nevertheless, even well developed columnar basalts tend to show many pentagons along with the expected hexagons.

The observations by Beard[2] on basalt flows the data used by Pieri) certainly show a large proportion of pentagons, ranging from 50% at Mount Rodeix to 35% at the Giants Causeway. Beard also recorded the mean number of sides of the basalt polygons and obtained values ranging from 5.23 at Mount Rodeix to 5.66 at the Giants Causeway. In an ideal system, with only 3-rayed crack vertices the mean side number should be six. A simple Monte Carlo model of a contracting cooling basalt can explain the significant occurrence of non-hexagons and also the reason why Beard's side counts were less than the ideal six.

In natural conditions, in an essentially unbounded basalt flow, one would expect the crack pattern to begin forming in a random manner; conditions can never be ideal enough for the entire pattern to develop in one cracking operation. Thus it can be expected to grow from randomly distributed stress centres[3]. Using a simple random number placement method and an effectively infinite field crack pattern, models can be produced which show many of the features observed by Beard. The model networks have a significant number of pentagons and heptagons and with just the random condition operating always have a mean polygon side number of six. However, a small number of extremely short sides are always produced and if these are eliminated the mean side value drops to ~5.6–5.7.

This suggests that Beard was unable to detect the very shortest sides and that this accounts for his mean side values in the 5.6 region. If this is so then a flow like the Giants Causeway is very close to an ideal flow; the ideal flow has a mean side number of six but as nature favours random processes we should expect a reasonable proportion of pentagons and heptagons. Pieri stated that the more isotropic and uniform the stress, the more likely it will be that hexagons form: this is looking to an ideal–ideal situation; in a real–ideal situation (a possible geological happening) it is likely that the mean side number will approach six but we should still expect non-hexagons to appear in the network.

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PIERI replies—It is clear from Smalley's comment above and from previous work[1] that polygonal fracture patterns forming in real materials in even the most ideal of isotropic stress regimes are subject to compositional anisotropies at many scales and a fracture network cannot form all at once. Thus, the ideal perfectly-formed pure-hexagonal pattern will never form in nature and will have particular difficulty in the context of global planetary fracture patterns[2]. I did not mean to imply that the end result of optimized real conditions would be pure hexagonal patterns, but rather that hexagons would be more numerous in situations where conditions were more uniform. Clearly, pentagons and heptagons will still be present, and I thank Smalley for clarifying this point.

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Low-angle X-ray scattering of chromatin

LANGMORE AND SCHUTT[1] have recently reported low-angle X-ray scattering experiments on chicken erythrocytes in which they find a peak at ~400 Å in the plot of s^2I against s. They conclude that this peak is related to the side-by-side packing of chromosome fibres. I wish to show here that this peak may have a different interpretation.

Langmore and Schutt[1] multiply the recorded intensity I by s^2 to correct for the random orientation of the fibres with respect to the X-ray beam, where $s = 2\sin(\theta/2)/\lambda$. However, as shown quite generally by Porod[2] and discussed in detail by Mittelbach[3], information on the cross-section properties of rod-like particles is best obtained from the study of the dependence of Is (instead of Is^2) on s.

At very low angles, a system of rod-like particles obeys the approximate Guinier equation:

$$(Is) = K \exp[-2(\pi s R_G)^2] \qquad (1)$$

where K is a constant and R_G the radius of gyration of the cross-section. For a cylindrical particle of uniform density and radius R, $R_G = 0.707\,R$. If Is^2 is plotted instead of Is, a maximum in the curve will appear when $d(Is^2)/ds = 0$. Introducing this condition in equation (1), it is easily shown that a maximum in the plot of Is^2 against s should appear for

$$s = 1/2\pi R_G \qquad (2)$$

Therefore, the maximum found by Langmore and Schutt[1] in the plots of Is^2 against s for $s = 1/400$ Å^{-1} is consistent with the presence of cylindrical objects with a cross-sectional radius of gyration $R_G = 64$ Å, as calculated from equation (2). This value would correspond to cylindrical particles of 180-Å diameter if their electron density were uniform. Thus I think that the peak observed by Langmore and Schutt[1] is due to intrinsic features of the chromatin fibres and does not demonstrate any regular side-by-side packing of chromosome fibres.

I suggest that the determination of the position of maxima and the use of equation (2) may be a useful alternative to Guinier plots in determining the value of R_G. A similar procedure could be used for globular particles, but in that case the maxima in the plot of Is against s would be used.

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LANGMORE AND SCHUTT REPLY—Although it is necessary that widely spaced homogeneous cylindrical fibres of diameter D give rise to a maximum in s^2I at $\sim s = (2.2\,D$ Å$)^{-1}$, a peak in the s^2I curve is not sufficient to prove the existence of widely spaced homogeneous cylindrical particles. The analysis that Subirana suggested is only relevant to the determination of the radius of gyration of particles provided that these particles are rod-like and widely separated. Proper Guinier analysis[1] tests the validity of these

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necessary conditions by comparing the shape of the experimental scattering curve with that predicted for widely separated rod-like particles. Such analysis of the scattering from our molecules shows that the Guinier conditions are not met, because the experimental plot of log sI against s^2 is not linear, due to the fact that the chromosome fibres we have studied are not widely separated (*vide infra*).

In our letter we outlined three basic arguments to prove that the 400 Å band is due to the packing of fibres and therefore not the result of scattering from widely separated rod-like molecules. These arguments were that: (1) the 400-Å peak is found in the I versus s plots of living chicken erythrocytes after subtraction of the non-nuclear background (represented by the scattering from living rabbit erythrocytes); (2) the 400-Å band in the s^2I vs s plots can be directly related to the 300-Å centre-to-centre spacing of fibres often seen in thin sections of a wide range of cells, including chicken erythrocytes[2]; and (3) the 400-Å peak in the s^2I curves of the scattering from intact nuclei is lost when the thick fibres are intentionally disaggregated into soluble thick fibres (as assayed by our electron microscopy) by elimination of divalent cations from the buffer. Each of these arguments disproves that scattering from isolated thick fibres has given rise to the 400-Å features in our patterns.

If, as we suggest, erythrocyte chromosome fibres are homogeneous cylinders of 370–400 Å diameter, a dilute dispersion of fibres would give rise to a maximum in s^2I at ~850 Å. To see a peak at these very small angles would require better first-order resolution in our X-ray camera.

Thus we feel that our experiments have demonstrated that the 400-Å feature is due exclusively to the side-by-side packing of chromosome fibres. The absence of a 400-Å feature in disaggregated fibres is inconsistent with the hypothesis put forth by Subirana. Furthermore the analytical technique he has proposed is not generally valid because it cannot test whether the necessary conditions for a Guinier analysis are met.

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Stability of loess in light of the inactive particle theory

A RECENT article by Smalley *et al.*[1] has suggested an interesting theory pertaining to loess stability. The inactive particle theory of soil sensitivity attempts to explain the sensitivity of quickclays on the basis that these soils possess only small amounts of clay minerals and as a result, develop no long-range bonds. Therefore once disturbed, they do not retain the adequate shear strength needed to remain stable[2,3].

In the classical Terzaghi sense, loess soils, which are composed predominantly of silt particles, would probably not be considered sensitive soils, in that the ratio of undisturbed to remolded strength, at constant moisture content, is usually around 3, depending on clay content, and therefore would generally fall into the category of medium sensitivity. If, however, sensitivity is taken as the ratio of undisturbed to saturated strength (in unconfined compression) as indirectly suggested by Feda[4], then some loess soils would have to be considered quick.

Loess soils, particularly bluff-line deposits, typically lose shear strength as a result of moisture saturation; in which case landslide potential becomes an extreme hazard. Loess in this state may be susceptible to "spontaneous liquifaction"[5] which could help explain the extent of landslides during the 1920 earthquake in the Kansu Province of China.

In recent investigations throughout the midwestern US, *in-situ* stability of loess has been related to liquidity index, that is when the *in-situ* moisture content reaches the liquid limit, usually on saturation, stability is all but lost and can be readily identified by isolated flow in boreholes. Because liquid limit is related to clay content, and saturation moisture is a function of density, this instability only occurs in special circumstances, typically low density and low clay content. As both calcareous and leached examples of this condition have been noted, the leaching theory of Rosenquist does not seem applicable in that carbonate leaching need not be complete, however, the cementation bond may be weakened. Scanning electron microscopy (SEM) qualitatively reveals that only a small portion of the clay fraction (<2 μm) of loess is composed of clay minerals, with the remainder being comprised of clay sized quartz particles. The amount of clay is related to closeness of the deposit to the source. An indirect measure of sensitivity is given by Skempton's[6] "activity" which for bluff-line loess averages ~0.46, clearly in the "inactive" range.

The inactive particle theory seems to have significance to loess stability, however, to investigate the theory fully thermogravimetry will be necessary to quantify the amount of active minerals present in each of the various particle-size fractions.

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SMALLEY REPLIES—LUTENEGGER makes quite a good case for considering loess a sensitive soil, and for applying the inactive-particle short-range-bond hypothesis to it, but if we are going to take this approach we shall have to acknowledge the pioneering opinions of Denisov[1]. He stated that a subsident loess (the sort described by Lutenegger) is extremely sensitive, particularly when saturated. The water content in such a state is above the liquid limit; the condition which occurs in the classic quickclays. Denisov pointed out that loess in this state preserves some strength and can form steep sides to canals and pits. When disturbed the shear strength drops to zero. The basic reason, according to Denisov, for the appearance of highly sensitive quickclays is their "preservation under natural conditions of the uncompressed state": an observation which could be very close to the truth.

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