HeLa cells studied by Penman are derived from a tumor and have survived for many generations in an artificial environment. They are no longer highly differentiated constituents of an organized tissue, as are the colleterial cells. A second possible source of discrepancy is the comparison of the ability of an intact cell to synthesize a specific protein with the ability of a cell-free system to continue protein synthesis.

The objections noted above are theoretical and must be evaluated by further experiments. One crucial and demonstrable difference which the colleterial cells share with other specialized cells is the marked insensitivity of protein synthesis to actinomycin D inhibition. As stated in our report, actinomycin D, at concentrations twice that required to inhibit uridine incorporation by more than 95 percent, depresses protein synthesis very little if at all. Singer and Penman (*1*) have shown that actinomycin D inhibits protein synthesis directly in HeLa cells and thus causes underestimation of mRNA half-life. Craig (*2*) has shown that in mouse L cells, also a tumor-derived line, both actinomycin D and cordycepin appear to block initiation of protein synthesis. Craig further suggests that both inhibitors may have a common target "factor." If cordycepin inhibits protein synthesis directly in colleterial cells, it must act at a site which is insensitive to actinomycin.

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Quartz Cleavage and Quick Clays

I am critical of the report by Krinsley and Smalley (*1*) inasmuch as it pertains to the cleavage of quartz and to quick clays.

The characteristic conchoidal fracture of quartz and the flat plates produced are well known. Even though quartz (as well as flint and obsidian) can be "knapped" to produce flake and blade-shaped particles by the proper application of pressure, this is not called cleavage because the crystallographic orientation of the flake is controlled by the manner in which pressure is applied and not by atomic structural factors. There is no reason why the tendency to flake should not occur in the small as well as large particle and with no more reason to call it cleavage. Krinsley and Smalley's addition to the debate on the cleavability of quartz is not substantive.

With regard to quick clay applications, Krinsley and Smalley state that recent diffraction studies "have shown that quartz predominates in some postglacial clay soils; if these particles are flat plates (similar in shape to kaolinite) then it may be possible that open structures of the 'cardhouse' type do exist in 'sensitive' clay soils." Although quartz may predominate in some postglacial clay soils, the evidence is overwhelming that quartz does not generally predominate in natural inorganic clay-sized materials such as soils. An important reason for studying clay minerals in the size fraction less than 2 μm is that the amount of dilutant quartz almost universally drops to nil at that size. As a clay mineralogist I have observed this to be generally true in my own work as well as in the reports of work by other clay mineralogists (*2*). Thus, one would not expect quartz to predominate in postglacial clay soils in general, or at least enough so in "sensitive" varieties to explain their quick nature. Specifically, my work with Jørgensen (*3*) and Rosenqvist (*4*) on Norwegian quick clays shows minor quartz in the coarser fractions (2 to 64 μm) of quick clay slide materials, but essentially no quartz in the clay-sized (< 2 μm) fractions of the quick clay. Since the evidence is against the ubiquitary presence of quartz as a predominant constituent of clay-sized soils, speculations about quick clays and cardhouse structures are pointless.

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The report by Krinsley and Smalley (*1*) once again raises the question of the nature of the cleavage in quartz. Their observations on the shape of sedimentary quartz particles appear to confirm what one would expect from purely theoretical considerations.

In a crystalline substance such as quartz, a cleavage—that is, a statistical preference for rupture in a certain direction of the atomic structure—can occur only in a direction in which the bonding is relatively weak, that is, significantly weaker than in adjacent structural directions. The *quality* of a given cleavage, however, as expressed in the observed (macroscopically, or at least microscopically) degree of continuity of the rupture planes, should depend mainly on the spacing of the atomic planes normal to the potential cleavage direction. For crystallographic planes with small Miller indices the spacings are relatively large, and thus the degree of continuity of a rupture plane parallel to such a crystallographic plane should, on the average, be also large—provided, of course, that the bonding across the plane is relatively weak to begin with. Conversely, cleavages parallel to atomic planes with nonrational intercepts (or large Miller indices) would be expected to be of poor quality; that is, the degree of continuity of a given rupture plane would, on the average, be small (despite the weakness of the bonding across the plane), and cleavages would be observable, if at all, only on the microscopic scale.

The poor cleavage in quartz—observable (as a statistical preference) only under the microscope—has erroneously been interpreted as occurring parallel to $\{10\bar{1}1\}$ and $\{01\bar{1}1\}$ for no better reason than that the observed cleavage direction is at an angle of 51°46′ with the unique crystallographic axis (*2*). If this cleavage were indeed parallel to *r* and *z* it would have to be of a quality good enough to be observable even with the naked eye in a hand specimen. An analysis of the crystallographic structure of quartz in terms of bond directions (*3*) indicates that, by coincidence, this direction (51°46′ from the *c* axis) happens to be one of several directions of relative weakness in the bonding. However, the azimuthal position of this particular weakness in the bonding does not coincide with that of the normal to *r* or *z*, but is at 30° from either normal; that is, the weakness in the bonding is normal to atomic planes with nonrational intercepts and, therefore, not likely to give rise to a cleavage of good quality.

It should not prove too difficult to mechanically separate the quartz platelets from the postglacial clay soils mentioned by Krinsley and Smalley. The platelets could then be sprinkled on a glass plate so as to rest on their bases. If there are indeed cleavages parallel to any such form as $\{10\bar{1}1\}$ or $\{10\bar{1}0\}$ they would be confirmed by a simple powder x-ray diffraction pattern; a cleavage parallel to (0001), should it exist, would show up clearly under the polarizing microscope. In the absence of confirmation by x-ray diffraction or the polarizing microscope one would have to conclude without hesitation or reservation that the observed poor cleavage is not parallel to any of these crystallographic planes.

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Berry makes some interesting points about quick clays. We accept that in most situations soil particles less than 2 μm in diameter might reasonably be expected to be clay mineral particles, but the quick clay situation is something special. After all, most soils do not have the ability to change from solid to liquid when subjected to a modest shock loading. One of the most famous quick clays is the Leda Clay of eastern Canada, and Gillott (*1*) has shown that its "clay-size fractions contain a significant proportion of primary minerals." Examination of Gillott's results will show that appreciable amounts of quartz and other primary minerals are found in all size fractions greater than 0.2 μm.

The current quick clay debate is really about whether the theories based on clay minerals need to be supplemented by a new approach which takes account of the large quantities of primary minerals observed in quick clays. The Ullensaker slide material examined by Berry and Jørgensen (*2*) had a size distribution in which 57 percent of the material was between 64 and 2 μm in diameter; we would like to suggest that this part of the soil played some part in determining its properties. Attempts have been made to produce artificial quick clays by using illitic clays and centrifugal sedimentation (*3*); these did not produce high sensitivities, and neither did similar experiments involving more realistic sedimentation methods (*4*).

Some interparticle force gives quick clays their relatively high undisturbed strength. The argument is about the nature of this bond. Conlon (*5*) has elegantly and convincingly argued that short-range cementation bonds and an open space-frame type of structure can explain the behavior of the Toulnustouc River quick clay. If the primary minerals (with a large contribution from quartz particles) can contribute to this space-frame structure, their study is certainly not pointless. If they happen to be largely plate shaped, possibly by the operation of some cleavage mechanism, then the contribution to an open structure could be very effective.

Berry points to one significant point which may have to be resolved if the quick clay debate is to make sense; are the Canadian (and Alaskan) quick clays the same as the Scandinavian quick clays, or should they be considered as totally distinctive materials, possibly requiring separate mechanisms to explain their behavior? Or do we look for some common factor which applies to both? The latter suggestion seems more scientifically satisfying. A full discussion of the short-range bond model of quick clay behavior will be published shortly (*6*).

Berry believes that quartz breaks with conchoidal fracture (we concur with this), but does not cleave. However, many authors have reported quartz cleavage (*7*), although it is not common for large particles. In particular, see the photographs in a recent publication (*8*), particularly plates 30, 38, and 42, which we believe show cleavage in quartz.

Hoffer's discussion emphasizes the complexity of the problem of quartz deformation, and it also indicates that the simple approach adopted by Berry in his discussion does not do full justice to the problem. Another aspect of the complexity of quartz deformation has been pointed out recently by Moss *et al.* (*9*). Their observations introduce a new dimension into the quartz cleavage argument and appear to provide an elegant explanation for the reported plate particle observations.

Some of the earlier argument has been misdirected because it concerned perfect crystalline quartz but, as Moss *et al.* perceptively observe, it is not quartz of this type which provides the clastic particles we find in sediments. These are derived from the highly imperfect quartz in granitic rocks, which contains some characteristic defects. The defects are introduced because the quartz particles are under considerable stress during the rock-forming process and they tend to deform. They do not deform by "cleavage" however; the process is more akin to "slip," the type of plastic deformation occurring in metals.

This type of deformation occurs in metamorphic rocks (*10*). In quartz the atomic pattern parallel to (0001) is regular and repeats itself at intervals of one unit cell parallel to the *a* and *c* axes. The slip system proved experimentally [slip plane (0001), slip direction $\langle 11\bar{2}0\rangle$] has the shortest and simplest atomic movements. Thus, it is possible that the formation of small, plate-shaped sedimentary quartz particles with prominent (0001) faces may be controlled by igneous processes and the cleavage of quartz particles may not be an important factor at all.

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