Table 2. Homing success.

Time	The number of birds which homed on:					
	Sun		Overcast			
			Inexperienced		Experienced	
	Sup	Nup	Sup	Nup	Sup	Nup
Day of release	9	13	3	1	7	7
1 day after	3	1	3	2	3	2
2 days after	0	0	6	7	0	0
3 days after	0	1	6	2	0	0
After 3 days	0	0	0	2	0	0
Never homed	1	0	5	10	4	4

sults with the orientation of European robins (*Erithacus rubecula*) described by Wiltschko (*6*). He reports that his birds use the inclination of the magnetic field of the earth and interpret the direction in which the magnetic field vector and gravity vector make the smallest angle, as north. The earth's magnetic field in the Long Island area has an intensity of about 0.6 gauss and an inclination of 70°. This field would combine with the field from our coils to produce a total field around the pigeon's head. The intensity and inclination of the resulting field would depend on the orientation of the pigeon's head with respect to the earth. This in turn would vary with the direction that the pigeon was flying and with the angle at which it held its head. We have estimated the pigeon's head angle from photographs of flying pigeons; the coils should produce a field oriented from about 10° to 45° above the horizon. We then plotted the net vector resulting from the combination of the earth's field and the field of the coils carried by the pigeon. The resultant obviously depends on the direction in which the bird flies, but the interesting point is that a bird with a Sup coil finds the total magnetic field vector most steeply inclined (the smallest angle between the magnetic field vector and gravity) when flying north, whereas a bird with a Nup coil finds the steepest vector when flying south. If pigeons were using the direction of the steepest inclination of the field to indicate north, then perhaps this would explain why there was a tendency for pigeons with Nup coils to fly 180° away from home when the sun was not visible.

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Inhibition of Cell Metabolism

The use of metabolic inhibitors has been of great value in eukaryotic cell research. However, the interpretation of results is fraught with peril and requires a careful measure of unanticipated side effects. A serious error in interpretation is possible in the type of experiment reported by Grayson and Berry (*1*). They show inhibition of protein synthesis after the administration of cordycepin and conclude that this inhibition is a measure of messenger RNA (mRNA) half-life. However, much the same results are obtained if the drug is administered to HeLa cells, that is, synthesis decays with a half-life of approximately 2 hours; however, the mRNA lifetime in HeLa cells has been determined to be considerably longer than that (*2*). Cordycepin actually appears to have a direct effect on translation rate. In fact, in HeLa cells, after the administration of cordycepin for several hours. normal-looking polyribosomes are obtained, but the ribosomes no longer are functioning. We do not know the nature of the lesion, but it could conceivably involve adenosine triphosphate metabolism or the substitution of cordycepin for the terminal adenosine in cytosine-cytosine-adenosine turnover on transfer RNA.

The use of cordycepin as a differential inhibitor of mRNA metabolism is restricted to comparatively short times (approximately 2 hours or less). At longer times, the drug has other effects on the cells which are difficult to interpret. Certainly the assumption that protein synthesis decline reflects the decrease in available mRNA is unwarranted. While the authors may consider that HeLa cells are not analogous to the insect material they have studied, before they conclude that they have indeed measured a messenger half-life, a direct measurement of mRNA or polyribosomes is necessary.

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We appreciate Penman's comments about the pitfalls of interpretation of inhibitor experiments. Our report clearly demonstrates that two inhibitors which ought to produce similar estimates of mRNA half-life in intact cells produce very different data. Our suggestion that the cordycepin kinetics for the decay of protein synthesis may be more realistic than those derived from the use of actinomycin D requires more direct confirmation. We are attempting to isolate and characterize the mRNA produced by colleterial secretory cells and hope to gain further insight into the problem of determining messenger half-life.

It is difficult to compare our results with data that are not generally available at present, but it seems important to emphasize the rather considerable differences in the experimental systems being compared. Most obviously, the

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 non-