tion experiments on the dehydrogenase-NAD system can now be assigned. There is an immeasurably fast initial change, which corresponds to the effect of the temperature rise on the NAD spectrum. Of the two second-order processes one (the slower) can now with certainty be attributed to the minor component and is absent in the purified protein. This leaves one other relaxation, the concentration-dependence of which shows it to be first order.

The NAD binding process therefore involves only one rapid second-order and one slow first-order reaction. The sequential binding scheme, with a set of consecutively different affinities, has no room in it for a first-order reaction. The concerted model on the other hand demands an intramolecular isomerization between states of high and low binding affinity; moreover there is also as expected a sharp decrease in this first-order rate constant as the NAD concentration increases. Other criteria for an all-or-none affinity change are likewise fulfilled, and it is also found that all four subunits must participate in the cooperative process, so that models based on dimeric cooperative units can be excluded.

Although these results make possible by far the least equivocal assignment so far of a mechanism for a cooperatively interacting system, one would not of course try to generalize from this one case, and indeed Kirschner *et al.* point out that the conditions in the cell are very likely such that the cooperativity is not called into play. If this is so, the teleological argument would run, the mechanism in an enzyme designed for, rather than adventitiously endowed with, cooperativity might well be quite different.

In a companion paper (*ibid.*, 51) Kirschner has used the stopped-flow method to study the properties of the NAD-complex of the enzyme in both its affinity (allosteric) states. This is made possible by the slow rate of conversion of the apoenzyme from the unreactive state, in which under the conditions of the experiment it predominantly finds itself, to the reactive state on addition of NAD. In effect a metastable complex of NAD with the low-affinity state is formed, which endures for more than 3 s. The spectroscopic change accompanying the binding of NAD to the low-affinity state was determined, and the activity of this form, with the NAD attached, towards the substrate glyceraldehyde-3-phosphate could also be studied. This proved to be negligible. Kirschner shows that with the aid of this interesting and important fact, earlier kinetic data on the enzyme, previously apparently hopelessly complicated, can be quantitatively explained.

SELENOLOGY

Why Lunar Erosion?

from our Geomagnetism Correspondent

THE presence of soils and fines on the Moon's surface indicates that the lunar rocks have undergone extensive erosion. But what is the predominant erosion mechanism? The lack of water on the lunar surface shows that one of the most important causes of terrestrial erosion cannot apply to the Moon; but the existence of so many lunar craters, on the other hand, has frequently led to the assumption that the principal source of erosion is meteorite impact, or at least the secondary ejecta thrown up by the impact of meteorites. From an examination of the morphology of microcraters in lunar rocks, however, Hörz *et al.* (*Earth Planet. Sci. Lett.*, **10**, 381; 1971) now conclude that although the occurrence of erosion by meteorite impact is not in doubt it is not a very significant process—the bulk of lunar erosion seems to take place not as a result of the impact of conventional meteorites but of primary cosmic ray particles.

To arrive at this conclusion Hörz and his colleagues carried out detailed stereoscopic microscope investigations of about 4,000 lunar microcraters larger

than 0.4 mm. The samples examined were two fine-grained and three coarse-grained crystalline rocks and one breccia from the collection of Apollo 12 samples.

Precise details of the morphologies of the microcraters are to some extent dependent on the particular type of rock; but most craters examined comprised a central pit (a glass-lined cavity in the centre of the overall crater), a surrounding halo (a concentric zone of shock-fractured minerals) with a high albedo and an average diameter 2.2 times greater than that of the pit, and a spall area (a concentric area around the pit which was removed by shock wave interaction with the free surface) which includes the halo and whose average diameter is 4.5 times greater than that of the pit. Completely fresh pits have diameter-depth ratios ranging from 2 to 5. Variations in relative diameters and depths are undoubtedly attributable to differences in impact velocities, the intensity of fracturing of the rock surface from previous impacts, the relative abundances of target minerals and errors of measurement. The point is, though, that almost all microcraters have the same morphological form.

The geometries of the microcraters are apparently similar to those of cra-

Induced Self-fertility

IN these days of technological precision it is comforting to find that results may still be obtained by accident. A case in point is the work reported by Brasier in next Wednesday's *Nature New Biology*. He has found that sex organs can be induced in heterothallic fungi when normally they will arise only when isolates of different compatibility groups are paired. Thus, sex organs were induced in an isolate of *Phytophthora palmivora* when contaminated with an isolate of another fungus, *Trichoderma viride*.

Screening of many different species of *Phytophthora* followed and the stimulus provided by *Trichoderma* was found to be effective in nearly all cases, although of the two sexually compatible groups, A¹ and A², only A² demonstrated this induction of the sex organs and the effect seems to be confined to isolates of this group.

The chemical nature of this sexual stimulus cannot yet be defined but the restriction of the stimulus to A² suggests that it may have affinities with substances produced by the A¹ compatibility type. Although Brasier could not induce sexuality with liquid culture filtrates or autoclaved agar cultures, he found that production of sex organs was stimulated by inverting a Petri dish of *Trichoderma* over a

Petri dish of *Phytophthora* so that there was no physical contact between the two cultures. It was observed that a far higher proportion of *Phytophthora* sex organs contained mature oospores when there was no physical contact, probably because the more rapidly growing *Trichoderma viride* killed the *Phytophthora* over which it grew. It seems likely, however, that the chemical responsible for the stimulus must be volatile.

Brasier points out that observations of a similar nature have been made in the past few years for bacteria and metabolic diffusates of soil micro-organisms have also been shown to cause the sexual stimulation of some species of *Phytophthora*. His own attempts with a number of other soil fungi have so far proved unsuccessful.

Brasier's finding has important genetic and ecological implications quite apart from its obvious use of making the identification of heterothallic *Phytophthora* easier. The possibility of inducing selfing in A² isolates would render genetic analysis much more simple and if the phenomenon occurs in nature, where *Trichoderma* and *Phytophthora* are potentially antagonistic, the survival of some *Phytophthora* species as oospores may be considerably enhanced.

ters produced on glasses and crystalline materials in the laboratory with projectiles the velocities of which exceed 10 km s^{-1} . Because of this, and because of the presence of a glass-lined pit in most lunar microcraters, Hörz *et al.* conclude that the velocity of 10 km s^{-1} must also be regarded as the minimum velocity for the production of these microcraters. On the face of it then such microcraters could be produced by the ejecta of a large scale impact event, for Gault *et al.* (in *Shock Metamorphism of Natural Materials*, Mono Book Corporation, 1968) recently showed that the velocities of such ejecta may well reach 20 km s^{-1} . The velocities of fine-grained particles within any given impact "jet" are unlikely, however, to exceed 10 km s^{-1} . The only other particles impinging on the Moon with greater velocities are primary cosmic rays; and so Hörz and his colleagues are led to the conclusion that at least 95 per cent of the lunar microcraters are produced by cosmic ray impact.

But velocities are not the only consideration. Three of the crystalline rocks examined by Hörz *et al.* have sharp boundaries between heavily cratered and completely uncratered surfaces. In theory, of course, this phenomenon is quite easy to explain in terms of the effect of secondary ejecta—uncratered areas could be taken to be newly broken surfaces not yet exposed to the bombardment of secondary projectiles. If this were so, however, boundary lines would be associated with the sharp edges and corners characteristic of breaks.

But in the lunar rocks examined there is no such correlation. On the contrary, in all three samples the boundaries between heavily cratered and uncratered areas run across relatively flat surfaces and bear no relation at all to rock geometry. Moreover, the boundaries are planes rather than lines in that they can be traced around the whole circumference of the rocks. As a result of these features, Hörz and his colleagues suggest that the uncratered surfaces were simply buried in the lunar regolith and thus remained unexposed to cosmic rays.

Finally, as Hörz *et al.* point out, the presence of a spall zone around the crater pits shows that the predominant erosion process on the scale observed is the "catastrophic rupture" of rock surfaces by discrete microfracturing events. Overlapping spall zones are frequent, the complete or partial destruction of craters by individual events is commonly observed and discrete pieces of pit walls are often seen to be broken off. All these features indicate not the gradual removal of material but catastrophic removal by high velocity, high energy impacts.

INSECTS

Cricket Songs

from a Correspondent

ACOUSTIC biology was, as expected, the chief (but not sole) concern of a semi-ACOUSTIC biology was, as expected, the insect body and between insects held in the Animal Acoustics Unit of the Sir John Cass School of Science and Technology (City of London Polytechnic) on June 9 and 10.

After a short film of bush-cricket stridulation (Mr A. Currie, Sir John Cass), some physical aspects were discussed by Dr H. C. Bennet-Clark (University of Edinburgh) and Mr H. Nocke (University of Cologne). Comparing *Drosophila* (an acoustic doublet in a free field) with the mole cricket in its burrow (a piston-driven exponential horn) highlights their differing communication problems—*Drosophila* loud,

but short range and private, *Gryllo-talpa* long range and broadcast. In *Gryllus*, Nocke's elegant series of experiments shows unequivocally that the "harp" of the tegmen (not the "mirror") is the radiator—a linear-type resonator uniformly tuned; and that it can also resonate to the emission of another cricket, as far away as 150 cm.

The discussion of hearing by Professor F. Huber (University of Cologne) and Mr D. B. Lewis (Sir John Cass) started with song recognition and its neuronal basis, and the control on responsiveness. Regen's classic work was always misinterpreted—he never claimed to have found clear frequency discrimination. Nevertheless, physiological evidence of some possible degree continues to accumulate, even to the level of processing in the brain. In intensity/time studies, an important new finding is that at the cellular level,

Greater Complexity in the Mantle

ONCE upon a time the upper mantle seemed to be a remarkably homogeneous region within the Earth. But as seismic techniques have gradually become more refined the upper mantle has gradually come to appear less and less homogeneous. So many discontinuities and partial discontinuities have now been observed, often quite close together, in the upper 1,000 km of the mantle that it is no longer possible to be sure that the boundary one seismologist observes below one part of the Earth's crust is the same as that observed by another worker at a roughly similar depth below a different part of the crust. Nor is it clear that any given discontinuity actually exists around the whole Earth whether at a constant or varying depth. In short, the picture gradually becomes more confused; and in next Monday's *Nature Physical Science* observations reported by Simpson *et al.* apparently add to the confusion. This is not, of course, an adverse criticism of these authors' work but merely a reflexion on the complexities of nature.

Simpson and his colleagues have taken advantage of the greater resolution available from combining seismic array data (P wave travel-time gradients) with conventional travel-time data to discover a previously unknown segment of travel-time curve. The array in question was WRA (Australia); and 330 earthquakes within the Indonesian-Philippines region (epicentral angles 12° to 30°) were analysed. The uncorrected P wave travel-time gradients clearly indicate the presence of upper mantle discontinuities at 360 km and 650 km—discontinuities which have been observed previously by several

other seismologists. But between epicentral angles of 19.4° and 20.6° Simpson *et al.* have detected, for the first time, several low amplitude arrivals a few seconds earlier than the times predicted from a simple model involving only the two discontinuities at 360 km and 650 km. In the sense that these newly discovered arrivals arise from epicentres within a very limited region (the Molucca Passage), they may just be a reflexion of crustal structure, even though Simpson and his colleagues have attempted to correct for such structure. Simpson *et al.*, however, adduce other evidence which suggests that this problem does not arise here and, as a result, conclude that the 360 km discontinuity should be replaced by a doublet at 280 km and 440 km.

How then does the new Simpson model (discontinuities at 280 km, 440 km and 650 km) compare with others? The closest perhaps is that deduced recently by Whitcomb and Anderson (*J. Geophys. Res.*, **75**, 5713; 1970) who, using a completely different method, observed discontinuities at 280 km, 520 km and 630 km. This is a pretty good correspondence for the upper and lower boundaries but not for the middle one. Although Whitcomb and Anderson observed no obvious boundary at 400–450 km, however, there is ample evidence for such a discontinuity which has even been interpreted in terms of a phase transformation from olivine to a spinel structure. The point is that the existence of Simpson's 440 km discontinuity is well supported by other workers. On the other hand, the 520 km boundary does not appear in the Simpson model. The discrepancies between the two models thus remain.