Lond., **B187**, 449; 1974).

Clearly there is no unique cell-to-cell matching, and Chung promulgates the view (*Cell*, **3**, 201; 1974)—credited to Lettvin—that what the retinal cells possess is information on their position relative to each other. If that is rigorously preserved, all that is needed at the tectum is an axial polarity to orient the entire array of retinal fibres.

It is the development of that axial polarity that Chung and Cooke have now subjected to experimental scrutiny. The aim of the experiments was to rotate the tectum at different stages of embryonic development and by later mapping the retinal projection, to determine the stage at which its axial polarity became fixed—in a manner precisely analogous to Jacobson's experiments on rotation of the retina.

The operation is technically very difficult because of the size and fragility of the embryonic brain and because at the stage at which most of the operations were performed (21-24) the different brain regions are determined but not yet anatomically distinct. The rotated tissue in each case was of the order of 15 cells square, and in the light of later anatomical analysis, seems sometimes to have included part or all of the adjoining diencephalon, and/or to have excluded part of the tectum.

Anatomically, therefore, the consequences of the operation varied considerably. Because the brain regions at this stage are determined but not yet differentiated, disturbed fragments of diencephalic or tectal tissue regulated to produce complete structures, giving rise to duplicated diencephalons or tecta, or both, at various positions depending on the exact origin and extent of the rotated tissue.

The effect of the anatomical chaos upon the retino-tectal map however turned out to be strictly independent of the orientation or morphology of the tectal tissue itself. The electrophysiological results clearly showed that the axial polarity of the map was determined by the diencephalon. Where the diencephalon was in its normal position anterior to the tectum, the map was normal; where it was transposed so that the tectum was anterior to the diencephalon, the map was reversed. In one case, where a tectum was wedged between two diencephalons, two maps with opposite polarities were recorded, one starting from each tectal edge. Morphological aberrations were ignored by incoming optic fibres. In an animal in which two tecta had developed side by side, the optic nerve entered aberrantly between the tecta but nonetheless spread out over the surfaces of both to give a normally ordered and polarised map.

Chung and Cooke infer from these results that the diencephalon acts as an organiser controlling the axial polarity of the retino-tectal projection, and preliminary results from grafts of diencephalic tissue alone seem to support this conclusion. They also raise the question of whether the tectal cells ever acquire permanent positional labels.

But the results raise numerous other important questions which are now accessible to experimental investigation. One of the most fundamental concerns the nature of the diencephalic signal. Morphogenetic signals have so far proved uniformly elusive, but until now, neurologists have not known even where to look for them. A further fascinating question is whether it is the same signal that first determines the morphological polarity of the embryo and later provides the axial polarity that guides the optic fibres. The morphological determination of the brain, the determination of axial polarity in the retina, and the determination of the axial polarity of the tectal map are all clearly separated in time.

But the two known organising regions in *Xenopus* are very close neighbours in space. The diencephalon develops from a region overlying the blastopore lip, which is the classical organiser for the early stages of development.

Could the classical and diencephalic organisers control all three developmental processes with the same signal, interpreted by the cells in different ways at different times? The only evidence so far for temporal changes in the interpretation of an organising signal comes from work on the cellular slime mould (Rubin *et al.*, *J. Embryol. exp. Morph.*, **33**, 227; 1975), which has the heuristic disadvantage that it is not an embryo but the unique advantage of possessing a chemically identified morphogenetic signal. The identification of an organising region in the amphibian brain offers the opportunity of investigating this question in the developing nervous system of a vertebrate. □

Glassy polyethylene

from P. D. Calvert

In principle any crystalline material can be converted into a disordered glassy solid if it is cooled sufficiently rapidly from the molten state. For some time it has seemed that polyethylene crystallised too quickly for the glassy state to be reached even by splat cooling techniques. But recently Hendra and co-workers have reported the preparation of glassy polyethylene simply by dropping a foil-covered film of polyethylene at its melting point into liquid nitrogen (*J. Polym. Sci. Lett.*, **13**, 365; 1975). The infrared spectrum shows it to be amorphous at 120 K and crystalline after warming to 180 K. Other workers must apparently have allowed their samples to warm up before studying them.

There is no shortage of polymers which can be produced in both crystalline and glassy states but polyethylene is by far the most studied synthetic polymer and there is already controversy over its glass transition temperature. As normally prepared polyethylene is semi-crystalline containing 30–70% amorphous material dispersed between small crystallites. The glass transition temperature of this amorphous phase has been claimed to be 125, 193 or 240 K. In a 1973 review (*Macromolecules*, **6**, 288) Boyer argued strongly for 193 K. Hendra's experiments show crystallisation occurring above ~ 170 K suggesting that the glass transition must be below this.

At the Shrivenham meeting of the polymer physics group of the Institute of Physics, Hendra discussed some even more controversial results. Using a peak in the Raman spectrum at about 20 cm^{-1}, which is a longitudinal acoustic mode of the whole chain segment within a crystal, they could measure crystal thicknesses. Crystals which form at 160–180 K have a thickness of about 170 Å parallel to the chain. This value is similar to that found in high temperature crystallisation. Since chain motion must be very restricted so close to the glass transition this result suggests that large scale untangling is not necessary for crystallisation and so throws doubt on the chain-folded crystallisation concept which is the basis of most present theories of crystallisation.

Worse still, they found that the crystal thickness formed on warming the glass depended on the temperature at which the polyethylene was melted before quenching to the glassy state. Melting at 400 °C gave rise to 155 Å crystals; at 160 °C the crystals formed were 180 Å thick. This is heretical in that liquids are not thought to contain any significant structures which could be quenched in and affect subsequent crystallisation. The results might be attributed to degradation of the polymer or destruction of nucleating particles but melting at 160 °C subsequent to melting at 400 °C again produces 180 Å crystals so the effect is reversible.