raised in different animals against GAD extracted from the brains of different species (catfish, mouse, sheep, rat and cow). These GAD antisera differ in the degree of cross-reactivity with GAD from different species and probably recognize different isozymes of GAD. Although the overall labelling patterns in the retina are similar for each GAD antiserum, notable differences exist. An analogous situation is also found to exist for the many types of GABA antisera.

The GAD antiserum used by Nishimura *et al.* was raised in sheep against rat brain GAD[8]; it is one of the most widely used GAD antisera in the retina. However, this is the only report of photoreceptor labelling with this antisera and there are no other reports of labelling with any of the GAD or GABA antisera in use elsewhere. Is it possible that 'GABA' labelling of photoreceptors is so alien to current thinking about photoreceptor physiology that any staining of photoreceptors may have simply been ignored? I suspect that those of us who perform GAD and GABA immunocytochemistry will take a closer look at old slides as a result of the new work by Nishimura *et al.*

Despite the orderly arrangement of photoreceptors at the distal margin of the retina, the photoreceptor transmitter(s) has not been identified. A leading candidate is L-glutamate, but it is so ubiquitous in nervous tissue that its involvement in photoreceptor transmission has been difficult to prove. GABA is synthesized from glutamate via GAD and both glutamate and GAD are components of the GABA shunt, a metabolic reaction that serves to stabilize GABA and glutamate concentrations. It is conceivable that the immunocytochemical localization of GABA and GAD in photoreceptor terminals is more closely related to glutamate/GABA metabolism than to GABA neurotransmission. The speculation that GABAergic photoreceptors participate in the parallel ON–OFF pathways (responsive to increases or decreases in light intensity relative to the background) in the retina seems unlikely. There is excellent evidence, in various species, that the ON–OFF pathways are subserved by two classes of bipolar cell, second-order interneurones that make different types of synaptic contact with the same photoreceptor[9-11]. Thus, there is no need to invoke a presynaptic substrate for ON and OFF pathways.

The paper of Nishimura *et al.*[2] will certainly generate much debate. It addresses one of the most elusive issues of retinal cell biology — the identity of the photoreceptor transmitter(s). Although highly controversial, these findings appear to be based on solid technique and cannot be dismissed on technical grounds. Basic transmitter systems, such as GABA, appear well conserved in different species and it is unlikely that the macaque monkey would be the only species with GABAergic photoreceptors. Before it can be concluded that some photoreceptors are indeed GABAergic, or even contain GABA and GAD, corroborative studies using other techniques need to be performed not only on other mammals but also on cold-blooded vertebrates, from which supporting *in vitro* data can more easily be obtained. □

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1. Altman, J. *Nature News and Views* **313,** 264 (1986).
2. Nishimura, Y., Schwartz, M.L. & Rakic, P. *Nature* **320,** 753 (1986).
3. Dowling, J.E. & Ripps, H. *Nature* **242,** 101 (1973).
4. Ishida, A.T., Kaneko, A. & Tachibana, M. *J. Physiol., Lond.* **348,** 255 (1984).
5. Lasater, E.M. & Dowling, J.E. *Proc. natn. Acad. Sci. U.S.A.* **79,** 936 (1982).
6. Yazulla, S. in *Progress in Retinal Research* (eds Osborne, N.N. & Chader, G.J.) 1 (Pergamon, Oxford, 1986).
7. Miller, R.F. & Slaughter, M.M. in *Retinal Transmitters and Modulations: Models for the Brain* (ed. Morgan, W.W.) 123 (CRC, Boca Raton, Florida, 1985).
8. Oertel, W.H., Schmechel, D.E., Tappaz, M.A. & Kopin, I.J. *Neuroscience* **6,** 2689 (1981).
9. Stell, W.K., Ishida, A.T. & Lightfoot, D.O. *Science* **198,** 1269 (1977).
10. Lasansky, A. *J. Physiol., Lond.* **285,** 531 (1978).
11. Nelson, R. & Kolb, H. *Vision Res.* **13,** 1183 (1983).

Oceanography

Respiration down in the depths

from T. Roger S. Wilson

ALTHOUGH the deep ocean floor occupies a major part of the solid surface of our planet, it is still almost as alien to us as the surface of Mars. Recently, deep-ocean vehicles have been designed to sample and perform experiments on the sea floor. The results from the latest of these, reported in this issue by Clare Reimers *et al.* (*Nature* **320,** 741; 1986), show that the oxygen consumption of the deep ocean floor at the sites sampled is surprisingly heterogeneous, even on the centimetre scale.

Most of what we know about the environment of the deep ocean has been obtained from samples and cores returned from abyssal depths to the surface. This sudden decompression, and other related insults, are disastrous to most organisms. Some of the bacterial population are apparently destroyed, although gross microbiological activity is often enhanced by decompression (Jannasch, H.W. & Wirsen, C.O. *Appl. envir. Microbiol.* **43,** 1116; 1982). Even the inorganic chemistry of the sediment and pore-water is disturbed, so that from samples taken on shipboard it is impossible to determine accurately fundamental facts such as the oxygen demand of the sediments or the saturation of the pore-water with respect to carbonate minerals (Emerson, S. *et al. Earth planet. Sci. Lett.* **61,** 220; 1982). It has therefore been necessary to develop *in situ* sampling and incubation techniques. These techniques are at an early stage of development — indeed, it is only a relatively short time since the first measurements of dissolved oxygen in deep-ocean sediments were made (Wilson, T.R.S. *Nature* **274,** 354, 1978).

Reimers *et al.* now report a significant advance in the development of such devices, and give some very interesting preliminary results. Their seafloor lander has returned information on sub-seabed oxygen gradients measured directly within the upper layers of the sediment. Microelectrodes which can give a spatial resolution of better than ten microns have been used in sediment studies for some time (N.P. Revsbech *et al. Limnol. Oceanogr.* **25,** 403; 1980), but never before as part of a remote analytical module. Because of the problems of keeping a ship accurately on station several thousand metres above the landing site, the instrument is autonomous, discarding ballast to return to the sea surface automatically after sampling.

The vertical resolution of the new measurements (1 mm) and their precision in such a difficult environment is a technical *tour de force*. The results are fascinating, showing fine detail in the oxygen profiles, and significantly different profiles only a few centimetres apart. Horizontal heterogeneity is expected in near-shore sediments with their intense bioturbation, but few perhaps would have predicted it in the sediments of abyssal depths. We are probably not seeing the direct effect on pore-water oxygen of a recent stirring of the sediment because diffusion would quickly smooth out differences on this scale. What, then, is the mechanism?

Reimers *et al.* have a plausible suggestion for the heterogeneity that involves variability in microbial respiration caused by the redistribution of particulate organic carbon within the upper layers of the sediment. The high quality of these data confirms the value of the *in situ* technique. It is difficult to see how the insight given by these results could have been obtained in any other way.

Deep-ocean sediments hold a record of the climate of past ages and will, in the future, form the final sink for fossil fuel carbon dioxide. Even though the study of these sediments requires much ingenuity and patience, it would be unwise to ignore what the results can tell us. □

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