halide crystals, the characteristics of cascade and thermal spike mechanisms have also been seen[13]. But there is an additional mechanism at work which is related to electron excitation, because sputtering can be produced by low energy electrons and photons[22]. This has satisfactorily been explained[23,24] in terms of an exciton model in which a Cl_2^- ion is produced with anti-bonding orbitals which cause the sputtering, in common with the model of Pollitt *et al.*[1]. Unlike their idea it has been worked out in some detail and I am surprised they do not refer to it.

In the sputtering of adsorpates, molecular solids and polymers, experimental data are scattered. The fact that large molecular fragments are sputtered and the expectation that electronic relaxation times are relatively longer than in metals gives the model by Pollitt *et al.*[1] a better chance. An obvious test would be to search for correlation between sputtering yield and the electronic stopping power for the incident ion, perhaps by varying the ion energy over several decades straddling the energy where electronic stopping power is a maximum (say 10 keV–1 MeV H^+).

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Tectonic emplacement of ophiolitic rocks in the Precambrian Mona Complex of Anglesey

THORPE[1] believes that the Anglesey serpentinite–gabbro suite should be regarded as part of an ophiolite—and therefore tectonically emplaced—because of alleged similarities with Tethyan ophiolites. The latter embrace a large and diverse collection of phenomena, so that it is hardly surprising that some parallels can be drawn. However, other features seem to have been misrepresented by Thorpe and critical differences neglected.

Apart from occurring on a scale quite minute in comparison with Tethyan and other ophiolites, the Anglesey suite (exposed not at Newborough, but near Rhoscolyn, grid ref. 277770) does not display the diagnostic properties of an ophiolite. For example, the serpentinite and gabbro show no semblance of a layered or sequential arrangement, there is no sheeted dyke complex, and although cherty sediments and pillowed basalts do appear on Anglesey they are distant, both geographically and stratigraphically, from the intrusive suite.

Thorpe asserts that aspects of the Anglesey suite such as 'finely preserved igneous textures' near margins, and 'absence of evidence for regional thrusting' are of 'widespread occurrence' in many tectonically-emplaced ophiolite complexes'. It seems to me that many ophiolites (see ref. 2) are characterised by a peripheral tectonite fabric, at least at the basal margin, and that geological maps of ophiolite terrains are replete with thrusts.

I disagree that the suite 'provides evidence of local rapid uplift, erosion and transport'. The country rocks are, on all sides, the same, distinctive, deep-water lithology which constitutes much of West Anglesey. Debris from the suite is not found (unlike Tethyan instances). The melange (olistostrome) occurring elsewhere on Anglesey evinces such processes arising later, but it also seems to contain no fragments of the serpentinite–gabbro suite, which may have still been at depth.

I have agreed[3] that the overall Monian association suggests an ultimate oceanic or upper mantle origin for the suite, but unfortunately the geochemistry cited by Thorpe reveals nothing about how the material was emplaced. A comparison with other, such as Tethyan, ophiolites would imply emplacement by obduction, although Thorpe seems to envisage some (unspecified) mechanism associated with plate subduction. The field evidence is most compatible with the suite being a deformed magmatic peridotite–gabbro complex.

Applying the fashionable soubriquet 'ophiolite' to the Anglesey suite requires a worrying stretching of the definition, and introduces serious implications for the stratigraphy and tectonics of the Mona Complex as a whole.

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Extending the range of radiocarbon dating

THE letter from Hedges and Moore[1] on laser enrichment of ^{14}C and its application to radiocarbon dating describes a most interesting and valuable line of research deserving the acclaim of all concerned with late Quaternary studies. It may be worth pointing out, however, that their statement of their expectation that the laser enrichment and new mass spectrometric methods will allow radiocarbon dating to be applied "well into the last interglacial period" begs a chronometric question for the dating of the last interglacial is not yet known with certainty. A more cautious and less pre-emptive statement might be to the effect that the new methods, by extending the far limit of radiocarbon dating to 100,000 years, may allow at least a part of the last interglacial to be absolutely dated. This is a different matter and in view of the importance of Hedges' and Moore's work the distinction is worth making clear at the outset.

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HEDGES AND MOORE REPLY—The distinction made by Burleigh is well taken. In this context it should be pointed out that the absolute dating to which he refers can only be achieved after reliable calibration of the radiocarbon time scale for this period. Fortunately there are grounds for believing that corrections to the radiocarbon time scale will not be very large[1].

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Failure to produce hepatic hyperplastic nodules in rats by portacaval anastomosis and testosterone

WEINBREN AND WASHINGTON[1] recently reported the spontaneous development of hyperplastic nodules in the livers of rats previously subjected to portacaval anastomosis (PCA). The nodules were found in all rats by 6 months post-operation. Others have failed to note such nodules in PCA rats and the authors suggested that this may be related to the difficulty in detecting the lesions microscopically or to differences in rat strains, housing or diet. We have examined 24 PCA rats 10–15½ months post-operation and have not found liver nodules. Because

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