where S is the astrophysical S function (for $E_{c.m.} \lesssim 10$ keV, $S \approx S_0 = 54$ and 1.15×10^4 keV barn for $d+d \rightarrow {}^3He+n$ and $d+t \rightarrow {}^4He+n$, respectively), $\mu = m_a m_b/(m_a+m_b)$ is the reduced mass of nuclei a and b ($m_d = 2$ and $m_t = 3$ AMU) and $E_{c.m.}$ is the relative energy (in keV) having some thermal distribution. We thus have $\sigma_{d\leftrightarrow t}/\sigma_{d\leftrightarrow d} = 213\exp(-2.99/\sqrt{E_{c.m.}})$, which is 11 at 1 keV and 83 at 10 keV.

In case 2 it is reasonable to expect only one of the reacting pair of nuclei to be accelerated and for the other to be essentially stationary on the other side of the crack. The electric potential is independent of the mass, so E_{lab} will be the same for accelerated d or t. But the d-t cross-section will then be different depending on whether d or t is accelerated, as can be seen by rewriting equation (1) as

$$\sigma_{a\rightarrow b} = \frac{m_a}{\mu}\frac{S}{E_{lab}}\exp(-31.3\sqrt{m_a/E_{lab}}) \quad (2)$$

where a is the accelerated particle and b is the stationary particle. Note that this formula is independent of m_b except for the weak dependence in μ. Equation (2) has the interesting consequence that $\sigma_{d\rightarrow t}/\sigma_{d\rightarrow d}$ is a constant equal to 178, independent of E_{lab}. (This ratio is in fact not constant if electron shielding is taken into account, but shielding is important only at $E_{lab} < 1$ keV, where the cross-section is too small to account for the observed neutron bursts; likewise at energies greater than 10 keV the S function varies, but such energies are unlikely to be produced by cracks.) Furthermore, $\sigma_{t\rightarrow d}/\sigma_{d\rightarrow t} \approx 1.5\exp(-9.95/\sqrt{E_{lab}}) \ll 1$ at $E_{lab} \approx 1$ keV, so only the lighter particle is usefully accelerated, and hence the ratio of neutron yields is in effect still approximately constant.

Production of 14-MeV neutrons at a rate greater than that of 2.5-MeV neutrons in a 50:50 d-t mixture would prove that the fusion is really hot, but can hot fusion possibilities 1 and 2 be distinguished? In case 2 only accelerated deuterons react, so the total rate is reduced by a factor of two, but this factor is probably not helpful since we do not know the absolute rate to begin with. Fortunately, there is another possibility for discrimination, which comes from the ratio of 14- to 2.5-MeV neutrons. For case 1 this ratio depends on $E_{c.m.}$, and would reach and exceed the constant value applicable to case 2 only at energies higher than expected in a plasma. Also, in case 1 it is reasonable to expect $E_{c.m.}$ and hence the neutron ratio to be different in different materials (such as Ti and Pd), while in case 2 the ratio should be sensibly independent of the material although the absolute rates might differ.

For either type of microscopically hot fusion, the probability of producing fusion conditions depends very much on the type of alloy, how it is treated, and the attention to detail of the experimentalist. This may account for the many null results, often at laboratories well versed in neutron detection[8], and the frequent rumours of a statistically significant result failing to be repeatable. Identification of the fusion mechanism should enable choices yielding more reproducible results.

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1. Jones, S. E. *et al. Nature* **338**, 737–740 (1989).
2. Cohen, J. S. & Davies, J. D. *Nature* **338**, 705–707 (1989).
3. Klyuev, V. A. *et al. Sov. tech. Phys. Lett.* **12**, 551–552 (1986).
4. De Ninno, A. *et al. Europhys. Lett.* **9**, 221–224 (1989).
5. Menlove, H. O. *et al.* Los Alamos Nat. Lab. preprint LA-UR 89-1974.
6. Derjaguin, B. V., Lipson, A. G., Kluev, V. A., Sakov, D. M. & Toporov, Yu. P. *Nature* **341**, 492 (1989).
7. Takeda, T & Takizuka, T. Jap. Atomic Energy Res. Inst. preprint JAERI-M 89-093.
8. Gai, M. *et al. Nature* **340**, 29–34 (1989).

Antidote to taxonomic instability

SIR—The current instability of biological nomenclature is increasingly irksome[1-3], and the antics of the lunatic fringe of taxonomy bring the whole discipline into disrepute[4]. Legitimate name changes include the exposure of past errors in the recognition of species, not the exhumation of overlooked homonyms[5].

But revisions of classification may also trigger changes in nomenclature. The aim of most taxonomists is to arrange species into monophyletic taxa — natural groups whose members are supposed to share a single common ancestry. Paraphyletic taxa (that is, incomplete monophyletic taxa) are avoided on principle, but in practice one is always confronted by 'residual paraphyletic groups'[6]. These may be created when the rank of a subset of species within a genus is raised to the generic level: the remainder, named under the original genus, then constitutes a paraphyletic group which cannot be defined by a unique set of characters. It has been common practice in the past to conceal the prevalence of residual paraphyletic groups.

Many name changes result from raising the ranks of taxa in order to avoid paraphyly. The alternative option of demoting the excluded taxa to the next subordinate rank, to avoid the paraphyly resulting from their exclusion from the related monophyletic taxon of the same rank, is rarely considered. For example, new evidence suggested the insect family Empididae was paraphyletic, because of the exclusion of the Dolichopodidae. One solution was to raise the subfamilies in the Empididae to familial rank, the rank of the Dolichopodidae[7]. This had the effect of creating several new family names and restricting the definition of the Empididae. But if the rank of the Dolichopodidae had been reduced to that of a subfamily, the Dolichopodinae, the only consequential nomenclatural change would have been this one letter ('d' to 'n').

In considering the change in nomenclature, we need to distinguish between those subordinate taxa that are raised to generic rank without any reference to scientific arguments and those that are raised to avoid paraphyletic taxa. The former are done *ex cathedra* or else for the trivial reason that more detailed study has revealed subordinate groupings within a taxon.

The problem with the nomenclature of paraphyletic genera, in particular, derives from the requirement that the generic name is used as part of the formal species binomen — despite the knowledge that we are likely to be stuck with residual paraphyletic groups indefinitely. I propose that we should identify residual paraphyletic genera by calling them 'paragenera'. Furthermore, we should allow the first word of a species binomial to be either the name of the genus or the paragenus to which the species belongs. The rule of priority should apply, regardless of any subsequent resolution of the paraphyly of a paragenus by recognition of constituent monophyletic 'genera'. Likewise if the paraphyly of a paragenus is resolved by adding to it the excluded genera, thus demoting the latter to the subgeneric rank, the priority of their 'generic' names should continue to be recognized. In this case a subgeneric name would form the first word of the binomen.

This proposal would mean that the generic name would normally be the first word of the binomen, as is the current rule, but would in a few cases be a paragenus or a subgenus. The increase in stability of nomenclature that would result from this radical modification of the accepted rule would far outweigh the disadvantages. It should not be rejected out of hand by those unprepared to offer an alternative to the current rampant instability.

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1. Erzinclioglu, Y.Z. & Unwin, D.M. *Nature* **320**, 687 (1986).
2. Mayr, E. *Nature* **339**, 654 (1989).
3. Forey, P.L. *Linnean* **2**, 15–16 (1986).
4. Thulborn, T. *Nature* **321**, 13–14 (1986).
5. Erzinclioglu, Y.Z. *Antenna* **7**, 156 (1983).
6. Disney, R.H.L. *Field Studies* **5**, 841–865 (1983).
7. Chvala, M. *Fauna Ent. Scand.* **12**, 1–281 (1983).