The stability of zoological nomenclature

SIR—With the exponential growth of scientific publication, and the enormous broadening of the subject of biology, the majority of biological publications that use the names of animals have been written in the past 50 years, and the majority of authors are now not taxonomists. Taxonomy, however, remains one of the cornerstones of biology with the main practical function of providing animals with unequivocal labels. Instability in taxonomic nomenclature is highly detrimental to the study of biology, and the declared aim of the International Commission for Zoological Nomenclature is to promote stability for the names of animal species. We suggest that this organization has failed in its aim, and indeed that within the Code of Zoological Nomenclature, as presently constructed, lie sections that cannot be regarded as anything but mechanisms for promoting instability.

To take one example, the code states that a specific name should be of the same gender as the generic name, and if the species is subsequently removed to a different genus, the ending of the specific name must be changed to agree with the gender of the new generic name. Since many, perhaps most, newly described species eventually end up in a different genus, a great many such names will have to be changed. Indeed, since the chance of the new genus being of different gender is probably about the same as its being of the same gender, this provision of the code ensures that about half of the names of species will have to be changed if they move to a different genus.

Another practice that guarantees instability is that of resurrecting long-dead homonyms. If a newly published name turns out to be identical to an existing name (that is, both generic and specific names are the same) then it is clearly a homonym and must be changed to avoid confusion. This is entirely reasonable. But if the problem goes unnoticed at first and, meanwhile, one of the two species is moved to a new genus, we might reasonably suppose that the problem has solved itself, since the two species now have different generic names. Regrettably, this is not the case, since according to the code, and for no better reason than that both species used to have the same names, the name must still be changed in spite of the confusion that will be caused. History must be kept tidy.

It might be useful to give an actual example of this practice. There lives in Africa a fly whose larva, called the Congo floor maggot, sucks the blood of man: it is thus of medical importance. It was named *Musca luteola* by Fabricius in 1805. Nobody noticed at the time that in 1763, Scopoli had already given the same name to a very different insect. In 1907, *Musca luteola* was moved to the genus *Auchmeromyia luteola* and there can be no confusion. Nevertheless, in 1980 the name was changed to *Auchmeromyia senegalensis* in the *Catalogue of the Diptera of the Afrotropical Region* because of the primary homonym and despite the fact that in the definitive works on this fly, including the medical literature, the name *Auchmeromyia luteola* is used throughout, and medical men have been informed that this is the only species of the genus of medical importance.

There are other devices in the code to ensure unnecessary name changes, for example the discovery of some ancient type-specimen, but nothing will be gained from an endless recital. Suffice to say that if the International Commission for Zoological Nomenclature expects to be taken seriously by the scientific community, it would do well to produce something better than the present document.

The response of taxonomists to this situation seems to be of two kinds. There are those (a minority, we think) who, having a legalistic turn of mind, do not question the code and fully approve of its rules. But far too many taxonomists wring their hands over the deplorable state of affairs, but are kept in place by the tyranny of the Commission: the Code must be obeyed. There is only one course open to us if sanity is to be restored and that is that some sections of the code must be consistently ignored.

The problems generated by strict adherence to the code are compounded by the current craze for cladistics (or phylogenetic systematics). Many taxonomists believe, almost as an act of faith, that a natural classification is there waiting to be discovered. We believe that classification is imposed by man, and furthermore it is usually subsequently 'discovered' to be wrong.

The changing of the classification with each and every publication on the phylogeny of the group is highly irresponsible. A particularly common and objectionable practice is the elevation of subfamilies to family rank. According to the code, the name of the original family has to be retained for the now restricted family, so that once again, the confusion is made compulsory under the code.

A better rule, if we really must split up families (a practice that seems to have little scientific merit), would be to forbid the original name to be used for the restricted family, in order that the otherwise inevitable confusion can be avoided.

To allow the present situation to continue simply devalues the family concept. However, there is no reason why the situation need arise at all. Phylogeny and classification are different activities, and we see no reason why the current (and usually ephemeral) ideas on the one have to cause confusion to the other.

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Interleukin terminology in disarray

SIR—Your comment on the unfortunate duplication of the term interleukin-4 (IL-4) for two distinct lymphokines is a very fair summary of the situation[1]. We wish to explain how this confusion came about, and to point out that no effective framework exists for the naming of lymphokines.

When we first became aware of this duplication we informed Professor Honjo (21 December 1985), and established that we had priority of publication, as he had not received galley proofs. He initially agreed to change the name of BSF-1 to IL-5, but subsequently opted to duplicate our usage of IL-4. Our paper appeared on 17 January[2], and their's on 20 February 1986[3]. The situation was further confused on 7 March by the designation of a largely uncharacterized activity involved in amplifying the T-cell response as IL-4A[4].

Unlike its biochemical counterpart, the nomenclature committee of the International Union of Immunological Societies (IUIS) has not been very active in this field (in fact the lymphokine subcommittee has never met). The term 'interleukin' was coined by an *ad hoc* group which defined IL-1 and IL-2[5]. Subsequent terminology has been determined by individual authors on the basis of priority of publication. Thus for example, the term IL-3 was proposed[6], and has been widely adopted for the factor produced by WEHI-3 cells. It replaces about a dozen synonyms. Another *ad hoc* group attempted to introduce a unified nomenclature for factors active on B cells[7], but this has not been universally accepted. These groups have usually arisen at scientific meetings, and have not necessarily represented all interested parties. It must also be pointed out that all these names have been based on biological activities, well before the protein sequences were established.

Our use of the term IL-4 was an attempt to clarify a complicated field. We had identified a lymphokine controlling eosinophil differentiation, and subsequently found it also had B-cell growth factor activity. We needed a name for this factor that was not linked to either biological activity, and we hoped to avoid the proliferation of names and consequent confusion that had occurred with IL-3. Because of our priority of publication we