The meaning of homology

SIR — The recent correspondence about the word homology has been concerned more with its use (or misuse) than with its meaning. It has been generally assumed that homologous DNA sequences and protein and chromosome structures reflect either a common ancestry or convergent evolution. The lack of demonstrable similarity is held to imply either distinct ancestry or adaptive radiation[1]. Yet some proteins which lack similarities may have had them and lost them through drift. More positively, one may say that the mere absence of homologies suggests nothing. Moreover, as the concurrent phylogenetic controls are not available, some accepted explanations, accounting for the existence of similarities, may ultimately require revision.

Alternatively, we could interpret the variety of homologous relations by considering the genes as determining both the evolutionary *propensities* (that is, Popper's measures of probabilities to evolve along preferred pathways under certain selection pressures[2]) and the critical structural features of the proteins encoded. Popper ascribes a concrete physical existence to propensities, which significantly limit the randomness of those homologies and which could not be accounted for by the above mechanisms.

Given the propensities to evolve similarly under particular evolutionary constraints, we must expect that structurally different proteins (having distinct origin) may yet display homologous amino acid sequences, whereas functionally related proteins (either of common origin or by convergence) may happen to lack them. Likewise, the propensity interpretation predicts molecules with unrelated function (and distinct origin) bearing conformational homologies, while structurally dissimilar proteins may share similar functions and have a common origin[3]. These predictions are supported by the factual evidence[3-5], qualify a gene as a hereditary carrier of evolutionary propensity and give an unexpected twist to the old question of structure-to-function relationship. The propensity viewpoint could be tested against blind chance once the rates of evolution are known.

Moreover, the propensity interpretation of probability in evolutionary relations challenges convergence as the putative explanation of certain similarities[3]. This is illustrated by analogy with the karyotype evolution in chronic myeloblastic leukaemia. The terminal clinical phase of the disease is associated with the development of increasingly similar karyotypic changes of the malignant clone in more than 90 per cent of patients[6]. As the similar chromosome aberrations accumulate, an impression of "convergent" karyotype evolution is simulated. The individual patients are unrelated yet a fictitious idea of convergence arises.

But this artefactual convergence towards a similar chromosome pattern is merely a manifestation of common but previously latent karyotype propensities to evolve in this particular blood disorder[6]. Extensive similarity of karyotype patterns does not imply genealogical (homologous) relations among the patients.

Since our extrapolations of homologies from the observed similarities often neglect the principle of maximum parsimony, we succumb unawares to a subtle prejudice that close resemblance inevitably derives from evolutionary relatedness (or chance). Unless the propensity interpretation is appreciated, such inference will continue to rationalize our sense of *déja vu*, camouflaging it with the often vague metaphor of "convergence".

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Wigner's phase-space density function

SIR — I and my colleagues were surprised by the article of J. Maddox (*Nature* **308**, 601; 1984) on Wigner's phase-space density function in quantum mechanics (which, as well known, may take negative values) and by the importance attached there to a recent paper by K. Wodkiewicz. This author has in fact merely discovered in 1984, and in a particular case, a well known result: if one considers both a (one-particle) state and a measurement apparatus, represented in general by a density matrix ζ and an efficiency matrix[1] F respectively, then the phase-space integral of the product of the two Wigner functions $W_\zeta(p,q)$ and $W_F(p,q)$ is always positive, being in fact just equal to the detection probability $P = Tr\, F\zeta$ $(=|\langle\psi|\phi\rangle|^2$ if ζ and F correspond to pure states ϕ, ψ, the only case considered by Wodkiewicz). His "operational probability distribution" is just the convolution product $P(p,q) = W_\zeta * W_F(p,q) = Tr\, F_{p,q}\zeta$ obtained for a class of apparatus $F_{p,q}$ translated in phase-space in the standard way $[W_{F,p,q}(p',q') = W_F(p-p',q-q')]$. The above results can be found, for example, in ref.2, or in the form just described (involving explicitly the measurement apparatus) in ref.1. Many recent papers have rediscovered this and other simple properties of the Wigner function, not found in his 1932 work, but already obtained long ago, for example the following result, given in that form in ref.1 and derived there from the previous one: the Wigner function always has positive mean values in any "gaussian" box in phase-space of dimension $\geq \hbar/2$, or in other words is positive after a "gaussian $\geq \hbar/2$ smoothing", a result discovered in several papers since 1975 (including one by Connell and Wigner himself in 1981).

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Electrical neutrality of atoms

SIR — In his excellent survey[1] on the experimental and theoretical aspects concerning the electrical neutrality of atoms, Close mentions that an unpublished result by King, based on the application of the Picard–Kessler method to H_2 and He gases, is $Q < 6 \times 10^{-21}$, where Q stands for the fractional charge difference between an electron and a proton. It should be added that a later result, also unpublished and obtained by King, applying the same technique to SF_6, puts Q at $(0 \pm 3.0) \times 10^{-23}$. Subsequently King and Dylla have reported[2] their determination, with an acoustic method, of the upper bound for Q as 1×10^{-21}, which apparently remains the best experimental figure we have at present.

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The rotation of birds' eggs

SIR — With reference to the interesting note by Jon Darius on the rotation of eggs (*Nature* **308**, 691; 1984), may I suggest that the blunt end of an avian egg might well have a function parallel to that of the mammalian fetal skull in stimulating the uterine muscles into contraction? Could the engaging of the blunt end be more effective in initiating the avian equivalent of "labour" than the tapered end, thus expediting the process of egg laying?

An alternative explanation of which I am less sure could relate to the development of the air sac inside the blunt end of the egg. At what stage in egg development does this air collect? Is it before laying, or is it while the egg is passing towards the cloaca? If the latter is the case, there would be an obvious advantage in presenting the egg blunt end first.

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