# Editorial

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It was with the greatest sadness that I heard of the death of Professor C O Carter, Assistant Editor of this Journal from 1970 to 1983. All readers will wish me to send our deepest sympathy to his wife and family. An obituary will appear in a later issue.

## Lateral reading 6

MITOCHONDRIAL DNA: NEW CLUES ABOUT EVOLUTION (Burton N, Jones JS. *Nature* 1983; **306**:317–8)

With the introduction of antigenic and electrophoretic comparisons of protein structure the genomes of related species can be looked at from an evolutionary point of view, and one of the surprises is that morphological evolution appears to have proceeded without much change at loci which code for proteins. Mitochondrial DNA may help to explain this paradox, and in the leader (which I found difficult) both plant and animal worlds are considered. "Mitochondrial DNA is able to penetrate the boundaries between species because it is not closely linked to genes responsible for maintaining reproductive isolation." Can someone please tell me how it does this? Hybridisation does not seem to explain it.

The leader states that mitochondrial DNA has already provided evidence about the evolutionary history of animals and plants and because it is maternally inherited this should mean that we are all more closely related to our mothers than to our fathers. But are we? In butterflies crosses between races or species usually give identical results as regards morphology whichever way round the cross is done. Furthermore, unlinked nuclear modifying genes which perfect mimetic patterns get into the general population whatever major genes for pattern an individual is carrying—and into naturally occurring hybrids.

Could someone please read the leader and write an explanatory review of the whole subject? If acceptable we pay up to £75.

REVERSAL OF DIABETIC NEPHROPATHY IN HUMAN CADAVERIC KIDNEYS AFTER TRANSPLANTATION INTO NON-DIABETIC RECIPIENTS (Abouna GM, *et al. Lancet* 1983; **ii**:1274–6)

How far this report is 'genetics' is arguable, but it certainly shows the importance of the environment. Two cadaver kidneys showing established diabetic nephropathy from a patient with a 17 year history of insulin dependent diabetes were transplanted into two non-diabetic patients. After 7 months renal biopsy specimens showed almost complete resolution of the nephropathy and both recipients remain free from proteinuria after a further 7 months. Long-standing type I diabetes, therefore, need not necessarily contraindicate kidney donation, and the observations made are also highly relevant to the pathogenesis of diabetic nephropathy. The donor's blood groups were B Rh+, HLA A3, A29, Bw35, Bw6, Cw6, DR1, and those of the two recipients were B Rh+, HLA A29, B5, B8, and B Rh+, HLA A29, B22, Bw35, Cw4 respectively.

A POLYMORPHIC DNA MARKER GENETICALLY LINKED TO HUNTINGTON'S DISEASE (Gusella JF, *et al. Nature* 1983;**306**:234–8)

The recent identification of a restriction fragment length DNA polymorphism showing close linkage to the locus for Huntington's chorea is a dramatic example of the power of recombinant DNA techniques in gene localisation. Before this study there was no positive evidence as to which chromosome was involved, and this is the first time that any disorder has been localised by DNA techniques without some previous knowledge of a chromosome assignment.

The fact that the relevant gene probe (G8) identifies two separate polymorphisms allows a haplotype to be constructed, analogous to that already familiar from HLA studies. The haplotype approach is likely to be of increasing value as adjacent DNA sequences are studied, and results in a much higher proportion of subjects showing heterozygosity and thus being informative.

The possible clinical applications of this development will need considerable further information; in particular we will need to know the precise recombination fraction ($\theta$) between marker and disease, the current confidence limits still being wide. Other autosomal linkage studies have shown that a considerable sex difference in $\theta$ may exist. It should soon be possible to exclude the existence of more than one locus for the disorder.

Discussion as to how a close linkage can best be used for prediction within families is already under way. There are likely to be as many relevant factors relating to the structure of families as relating to the



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gene probes and associated polymorphisms. The onus is now placed on medical geneticists to ensure that families with Huntington's chorea in their regions are fully studied and documented. This is perhaps the most essential prerequisite for the use of any predictive test and is a much more onerous and time-consuming task than is generally realised. (Summarised by Professor P S Harper.)

SYNCHRONISATION OF DEFAECATION IN THE AFRICAN ELEPHANT (Rees PS. *J Zoology* 1983;**201** part 4: 581–5)

In this paper evidence is produced which suggests that there is some degree of synchrony in the defaecation of African elephants in captivity, and this has been previously described in domestic cows where, if one cow defaecates, a large proportion of the milking herd follows suit—allomimetic behaviour.

As regards elephants, it seems that eliminatory behaviour could be influenced by selection and this may have an adaptive significance if it occurs in the wild. Thus, when an elephant defaecates it is forced to assume an extremely vulnerable position which, in the case of a young animal, might make it very liable to be attacked if it lagged behind the rest of the herd. Therefore, the argument goes, there would be heavy selection favouring any mechanism that would increase the probability of a young elephant defaecating at the same time as its mother or as some other elephant nearby and the optimum time for defaecation would be when the herd is not moving.

Is this an inherited behavioural trait or does it have to be learnt in each generation?

HYBRID DYSGENESIS IN DROSOPHILA: CORRELATION BETWEEN DYSGENIC TRAITS (Eggleston P, Kearsey MJ. *Heredity* 1980;**44**:237–49)

It is only recently that I heard about hybrid dysgenesis, but having given it a good airing among medical geneticists I find that I am not the only ignoramus. The phenomenon occurs in *Drosophila melanogaster* in certain crosses between laboratory stock females hybridised with wild males, and the aberrations manifest themselves in the F1 females by poor egg production (GD) and low hatchability (SF), and in the F1 males by increased mutation rates, a high incidence of meiotic abnormalities, and male recombination—but only one way round, not in the reciprocal cross. At the chromosomal level, genes are said to 'jump' in much the same way (I gather) as Barbara McClintock described in maize. The paper under consideration suggests that GD and SF have a common cause and are not independent phenomena.

So be it, but my problem is more fundamental. Is this remarkable situation peculiar to *Drosophila* and, if so, why? In my butterflies we frequently cross laboratory stock females with males from wild populations, and retrospectively have had no need to invoke HD, but we may have missed the abnormalities.

If HD is a true bill, could it be responsible for a trait appearing in the offspring of parents neither of which had the character in their ancestry? Answer obviously "yes", but could I please be brought up to date?

UNISEXUAL LIZARDS. PARTHENOGENESIS IN A LAND VERTEBRATE (Cole CJ. *Sci Am* 1984;**250**: 84–90)

Though this paper has no medical relevance, it is of extreme general interest and represents the culmination of many years' work. When parthenogenetic, the lizards are necessarily all females and the haploid eggs have borrowed from the plants the device of doubling up the chromosome number so that meiosis can take place, though crossing over is irrelevant. The matter is gone into in great detail in the paper, and I have a copy of the *Scientific American* if anyone wishes to borrow it.

What interested me was the hypothesis put forward to account for the parthenogenesis. It is postulated that at some time in the past the grassland species of whiptail lizard occasionally interbred with a sympatric desert species on the edge of its range. The hybrids were sterile but they may have competed successfully for food (hybrid vigour) with the non-hybrids in the mixed desert-grassland habitats. Once the hybrid females had learnt the trick of becoming polyploid their future was solved and a unisexual species reproducing by cloning had arisen. Bernard Shaw would have enjoyed this example of purposeful evolution!

Later: see Cole CJ. Evolution of parthenogenetic species of lizards. In: Reinboth R, ed. *Intersexuality*. Berlin: Springer-Verlag, 1975:340–55 and Mittwoch U. Parthenogenesis. *J Med Genet* 1978;**15**:165–81. Are the editor's ears red?

CLOSE LINKAGE OF FRAGILE X MENTAL RETARDATION SYNDROME TO HAEMOPHILIA B AND TRANSMISSION THROUGH A NORMAL MALE (Camerino G, *et al. Nature* 1983;**306**:701–4)

This paper describes the use of a cDNA probe for factor IX in analysing a very interesting family with

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mental retardation associated with the fragile X chromosome. Six affected males in generation III were related through their five mothers who were sisters; these five women had two further sisters, in one of whom the fragile X chromosome was detected, and three healthy brothers. The affected and carrier subjects from generation III and the seven women in generation II all possessed the same DNA polymorphism detected by the factor IX probe, and so did the mentally and cytogenetically normal grandfather of generation I, but not the grandmother. The authors concluded that the grandfather possessed the X linked gene for mental retardation, not only because of the inheritance of the DNA polymorphism but also because of the segregation in generation II, with certainly six and probably all seven of his daughters carrying the X linked gene, while none of his sons did.

There are several conclusions and unanswered questions to be drawn from the studies of this interesting family. Firstly, the linkage between the cDNA factor IX probe and X linked mental retardation is close, probably less than 12 cM. It is likely that such linked DNA polymorphisms will be more useful in following the transmission of the X linked gene through families than cytogenetic studies. Secondly, this family provides another example of a healthy male transmitting the gene for X linked mental retardation to his daughters, and should be a warning to students of families with X linked mental retardation that a grandmother can only be assumed to have transmitted the X linked gene if she has an affected son or brother. This is only the third example of a healthy transmitting male being available for cytogenetic studies; this man, like one of the other two, did not express the fragile X chromosome in his lymphocytes. Since about one-third of female heterozygotes, particularly those who are adults of normal intelligence, do not manifest the fragile site in their blood cells, it is perhaps not surprising that an occasional male (or more) may be cytogenetically normal. Indeed, there are many males with X linked mental retardation who have similar clinical features to those originally described by Turner *et al*, yet who are cytogenetically normal, and it will be interesting to discover whether or not their X linked gene is also situated at Xq27. What is more surprising is that a male, hemizygous for a disorder which produces mental retardation, can be mentally normal. Since the grandfather in this family was likely to have been hemizygous in all his X bearing sperm because of the segregation among his offspring, mosaicism (as would occur, for example, if he had acquired a half X chromatid mutation) is unlikely to account for his normal mental state and normal cytogenetics. Clearly there is much more to be learnt about the manifestation of this curious X linked gene. (Summarised by Dr Sarah Bundey.)

PUBLICATION OF UNREFEREED ABSTRACTS. PUBLISHING 'IN CONFERENCE' (Bray MA. *Nature* 1984;**307**:206)

Michael Bray draws attention to what he considers an alarming trend in the biological literature—the reporting of scientific data at conferences followed by publication of the abstracts in journals. He concludes: "While it is not possible to blame scientists (myself included) from taking this easy road to publication, the inclusion of unrefereed conference abstracts in subsequent articles, where they acquire the same pedigrees as fully detailed and refereed papers, should be viewed with some unease, as this practice is open to considerable abuse".

What do readers think?

C A CLARKE

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