that are not formally distinguishable from crossing over.

These problems of definition apart, what is it that constrains the obligatory crossover to occur in the pseudoautosomal segment of the X–Y bivalent? The constraint is certainly not the length of the pairing segment — in man, X–Y pairing, as defined by the formation of a synaptonemal complex, includes most of the Y short arm and can extend into the long arm, much longer than the pseudoautosomal segment. More probably it is the extent of X–Y homology in the pairing segment.

In other situations where stretches of synaptonemal complex are formed between non-homologous chromosome segments, crossing over is excluded from these non-homologous regions. Because there are degrees of homology, the pseudoautosomal segment may end in a zone where the likelihood of a crossover rapidly decreases in conjunction with a decrease in homology, rather than having a discrete boundary.

It is generally agreed that the heteromorphic mammalian X–Y pair must have evolved from a pair of homologous chromosomes. The pseudoautosomal segment might therefore represent a region of the original homologous pair that has been retained to ensure chiasma formation and orderly X–Y separation at meiotic metaphase I. Although gene-dosage considerations do not preclude the pseudoautosomal segment from exchanging material with the autosomes[7], it may nonetheless be a conserved linkage group with significant homology between mammalian species. If so, with relaxed hybridization conditions it should be possible to use human pseudoautosomal DNA probes to pull out related pseudoautososmal sequences from the mouse genome.

Combining the flexibility of mouse breeding with the exceptionally high recombination frequency in the pseudoautosomal segment should allow a rapid and detailed genetic analysis of this region. Now the molecular geneticists have stuck in their thumbs, I am sure that they will pull out some plums. 1986 promises to be a good year. □

1. Rouyer, F. *et al. Nature* **319**, 291 (1986).
2. Cooke, H.J. *et al. Nature* **317**, 687 (1985).
3. Simmler, M.-C. *et al. Nature* **317**, 692 (1985).
4. Keitges, E. *et al. Nature* **315**, 226 (1985).
5. Ashley, T. *Hum. Genet.* **67**, 372 (1984).
6. Ashley, T. *Genetica* **66**, 161 (1985).
7. Burgoyne, P.S. *Hum. Genet.* **61**, 856 (1982).
8. Polani, P.E. *Hum. Genet.* **60**, 207 (1982).
9. Page, D.C. *et al. Nature* **311**, 119 (1984).
10. Holm, P.B. & Rasmussen, S.W. *Carlsberg Res. Commun.* **48**, 415 (1983).
11. Burgoyne, P.S. & Baker, T.G. in *Controlling Events in Meiosis* (eds Evans, C.W. & Dickenson, H.G.) 349 (Company of Biologists, Cambridge, 1984).
12. De la Chapelle, A. *et al. Nature* **307**, 172 (1983).

Paul S. Burgoyne is in the Medical Research Council Mammalian Development Unit, University College London, 4 Stephenson Way, London NW1 2HE, UK.

Nuclear winter

Towards a scientific exercise

from K.A. Emanuel

The detonation of the first atomic bomb on 16 July 1945 changed the world in many ways, not least of which is the view some scientists have taken towards their profession. Many have come to feel that responsibility for the influence of their achievements rests partially with them and should not reside solely with politicians. While imparting certain benefits to political decision making, this attitude has at times tainted the objectivity that is crucial to scientific endeavour. Nowhere is this more apparent than in the recent literature on 'nuclear winter' research, which has become notorious for its lack of scientific integrity. Among the most serious criticisms levelled at this work has been the failure to quantify the large uncertainties associated with estimates of the war-initiated fires and their combustion products, the highly approximate nature of the global circulation models used in the calculations, and the appearance of the results in popular literature before being exposed to the rigours of peer review.

Although controversy continues to surround this work, serious research is beginning to qualify the earlier bold assertions. A good example appears on page 301 of this issue in the paper by B.W. Golding, O. Goldsmith, N.A. Machin and A. Slingo, which attempts to examine the physical factors affecting the disposition of a smoke plume shortly after its inception. These authors point out that previous estimates of the nuclear winter effect have relied either on one-dimensional models, which must assume a uniform distribution of smoke, or on three-dimensional models with grid intervals too large to resolve mesoscale (medium-scale) circulations very near recently initiated plumes. Yet it is likely that such circulations would result from the horizontal temperature gradients produced by radiative heating of the smoke.

Using a mesoscale numerical model, Golding *et al.* show that mesoscale ascent rates of 20 cm s- are possible 12 h after plume initiation; such rates will rapidly lead to condensation of water vapour even in circumstances where the air is initially quite dry. Subsequent scavenging of the smoke by precipitation could substantially alter the characteristics of the plume before it diffuses into the continental-scale pall which is the starting point of most nuclear winter calculations.

The calculations presented by Golding *et al.* are themselves dependent on assumptions concerning the concentration, geometry and radiative characteristics of the smoke plume and the large-scale meteorological conditions in which it is released, and the model does not explicitly deal with the interaction of condensed water and snoke particles. The results do strongly suggest, however, that more attention must be paid to the mesoscale characteristics of the plume in the first day or so of its lifetime. The paper is a welcome step in transforming nuclear winter research from a means of political advocacy to a scientific exercise. □

K.A. Emanuel is at the Center for Meterology and Physical Oceanology, Massachusetts Institute of Technology, Cambridge, Massachusetts 02139, USA.

Archaeology

Reconstructing ancient diets

from Richard Burleigh

A paper published recently (*Nature* **317**, 806; 1985) and one on page 321 of this issue make significant new contributions to the investigation of ancient human diets by the analysis of stable isotopes in skeletal remains. In the first, M.J. DeNiro is concerned with the detection of changes in the composition of sub-fossil bone (diagenesis) that might lead to false results. The other, by S.H. Ambrose and M.J. DeNiro, deals with reconstruction of patterns of diet in humans who are known from historic records or presumed from archaeological evidence to have followed particular modes of subsistence. Both papers help to provide a firmer basis for possible investigation of human diet in the much more remote past.

Measurement of the ratios of stable carbon isotopes ($^{13}C/^{12}C$) in collagen from skeletal remains has been used successfully in recent years to establish prehistoric human diets and hence, somewhat more inferentially, patterns of subsistence in the past, for example, in parts of North America, Africa and north-west Europe. The basis of this method is that the carbon-isotope composition of collagen in bone reflects the isotopic composition of the plants that form the base of the food web, isotope ratios being expressed as $\delta^{13}C$ values, or the difference in parts per