Steensberg was the only one to consider population pressure as a possible determinant of cultural change, and in fact concluded—wrongly I believe—that it has not been a significant factor.

Clearly it was important to have the genetic aspects of crop plant evolution considered in such a symposium, and this was done by Barbara Pickersgill (University of Reading) and C. B. Heiser. They pointed out that domestication led to greater morphological variation than is seen in wild progenitors, but that unique processes were not involved. Moreover, the influence of man as a selection pressure may have changed through time—a fact to be revealed if at all by the archaeological investigation of earlier human communities and their implements. Although cytogenetic aspects of plant evolution have been explored to some extent in relation to wild plants and cultivars, relevant physiological adaptions are by no means worked out. L. T. Evans (CSIRO, Canberra) outlined some of the data and problems in this field—in so far as they might be relevant to studies on ancient cultivars. He indicated that there have been changes in life cycles, degree of environmental control and yield potential in some plants—related to factors of climate, basic anatomy, and hormone variation—but the physiological basis of such changes was not well understood. Human intervention does not seem to have produced an increase in relative growth rate in the various plant species.

In contrast to the previous papers which were mainly concerned with plants, the next two were a slightly limp attempt to restore balance and give more consideration to animal populations.

M. R. Jarman (University of Cambridge) gave general comment on the development of early animal husbandry. He attempted to show the differences between zoo-archaeology and archaeo-zoology, one being concerned with earlier human behaviour in relation to other animal species, while the zoological approach was perhaps more strictly to view the nature of microevolutionary change from wild to domestic forms within a particular species. He emphasised that we must settle into appreciating that animal husbandry in its most general sense occurred well before the Neolithic culture phase. Moreover, it is important to examine the possibility of close man–animal relationships in prehistory beyond the scale of domestic relationships seen today.

As yet biochemical taxonomy has had only modest recognition in helping to resolve questions of domestication. There is no doubt that it has far greater potential, and it was good that at least honourable mention was given to this field at the conference. E. M. Jope (Queens University of Belfast), in fact, concentrated on animal proteins, and spent much of his allotted time in introducing this complex field. He did however indicate clearly that various lines of investigation may eventually produce significant results, for instance, the haemoglobins and other biochemical variation in modern animal stocks, amino-acid differences in ancient bone, and perhaps even ancient and recent keratins.

Except for the concluding paper, the rest of the contributions were concerned with regions, and in some cases with particular crops. G. H. S. Bushnell (University of Cambridge) outlined the beginnings of agriculture in the Mexican area. Although he is the doyen of Americanists in this country, it was a little regrettable that those who had more direct acquaintance with the significant developments in the agricultural history of the New World were not in attendance at the meeting. Bushnell did make the pertinent point that the long, and probably ecologically complex, development of agriculture in the Americas could hardly be seen as a "revolution" in Childe's sense of the term.

Sir Joseph Hutchinson's task was to consider crop plant evolution in the Indian sub-continent. He indicated that the various plants of economic value to the earlier cultures had a diverse origin—being introduced at various times from Asia to the east and from both south-west Asia and Africa to the west. Tea was one of the few crop plants which was probably indigenous. Possible trans-Pacific movement of plants was considered in relation to maize. In contrast to these plant origins, Hutchinson believes that livestock may not have been so derived from elsewhere—wild forms of domestic varieties being still seen in India. Early agriculture in this area, Hutchinson suggests, is best seen as an extension of that evolved in Western Asia, and certainly there were links into Mesopotamia. Finally, Hutchinson pointed to the considerable variation which had occurred in some plants, such as the banana, cotton, wheats and rice—not so much an indication of the degree of domestication as the nature of the breeding system, length of time the crop was under cultivation, the cumulative nature of certain mutations, and even the response to disease. Further aspects of early Asian agriculture, and particularly rice cultivation, were explored by T. T. Chang (International Rice Research Institute, Philippines). As he said, rice now feeds more people than any other cereal and clearly has an intimate relationship with the development of some cultures, especially in eastern Asia. Varieties of rice appear at different dates. In some rice cultures, millet was in fact the oldest crop; an

Evidence for triggering

from Peter J. Smith

As long as the nature of deep earthquakes remains unclear, wisdom would seem to dictate emphasis on the study of single-source events. Yet multiple events, though more complex, give additional data which can be put to good use. Both Chandra (*Bull. seismol. Soc. Am.*, **60**, 639; 1970) and Fukao (*Phys. Earth planet. Interiors*, **5**, 61; 1972), for example, were able to use multiple-source earthquakes in the Peru–Brazil region to determine rupture velocities. In each case the secondary events apparently lay on a nodal plane of the primary shock, from which it was assumed that the fault plane was continuous between epicentres and that rupture was planar and unidirectional.

Clearly, however, such analyses beg the question of earthquake mechanism—a problem given point when Dziewonski and Gilbert (*Nature*, **247**, 185; 1974) showed that for some deep shocks the double-couple mechanism is not entirely adequate. Moreover, although Chandra and Fukao obtained relative locations and origin times of the secondary events, they did not determine the fault plane solutions. This omission could be critical, for Strelitz (*Geophys. Res. Lett.*, **2**, 124; 1975) now shows that it may have led to false assumptions.

Using both P and pP phases, Strelitz has analysed the 5 September 1970 Sea of Okhotsk earthquake which comprised a magnitude 4.5 event followed about 5 s later by a magnitude 5.7 event with roughly the same epicentre but a focal depth 23 km greater. He finds that the fault planes and principal stress axes of the two events were significantly different and that the second event lay on neither of the nodal planes of the first. A curved rupture zone seems to be ruled out on the grounds that such faulting would probably radiate continuously during rupture, whereas the two shocks observed were discrete events. That the proximity of the two events is purely coincidence also seems unlikely because other similar event pairs occur in the same region. Strelitz thus concludes that the first shock simply triggered the second.