In July, 1986, I directed a NATO «Advance Study Institute on the evolutionary biology of intelligence». The published proceedings of the Institute (Jerison & Jerison, 1988) consist of 24 chapters, including a final chapter of «Afterthoughts» in which I review the meeting and, with the cooperation of William Hodos and L.M. Herman, debate some of the more difficult issues that were raised. (See Appendix A for the complete list of chapters). There was enough interest in our proceedings to undertake the present publication on revised and updated statements by several of the contributors in this special issue of Human Evolution. In addition to introducing the contributors to this Special Issue, I review the articles by Pickford and by Harvey and Pagel in more detail, because of my special interest in the topic of these papers.

My plan in arranging the course of lectures for the Institute was to emphasize, in order, evolutionary biology, neurobiology, analytic issues (philosophy and artificial intelligence) and, finally, behavioural data from ethology and psychology. The order reflects my judgment of what people interested in intelligence know and ought to know about its biological foundations, with the least widely known discipline given the highest priority. This emphasis resulted in a somewhat unusual balance of contributions, which is also present in this Special Issue.

The order of presentation of the papers here is the natural one dictated by the logic of the topic. It is appropriate to begin with a philosophical analysis of the place of «intelligence» in evolutionary biology, an issue addressed here by Michael Ruse. The serious questions of definition must also be addressed, since one clear conclusion is that there is no consensus. Nobody (myself included) was really satisfied with the definition that I have come to support: that intelligence in biological context refers to the set of behaviors controlled by «excess neural processing capacity» as estimated by measures of encephalization. That is, in fact, an acceptable minimal definition, but its vagueness about the possible behaviors is a serious fault. The approach suggested in Henry Plotkin’s contribution to this Special Issue may provide a clearer and more suitable framework for thinking about intelligence. Problems of definition are raised in several other contributions at the Institute and in the papers in this Special Issue. Although they remain unsettled, I am persuaded that our analysis is important not only for insight into the evolution of this inadequately defined phenomenon but also for classification of its nature, i.e., its proper definition.

The papers by Pickford and by Harvey and Pagel, which follow Ruses’s and
Plotkin's, are expositions of evolutionary data. Martin Pickford accepted our invitation to present a tutorial lecture on the fossil record with emphasis on primate evolution. An updated version of that lecture was published in the proceedings of the Institute cited earlier. For this Special Issue, Pickford has prepared a completely new statement, with many original ideas about the significant events in primate history. Since I am including this revised statement as part of our conclusions about intelligence and evolutionary biology, and since I have discussed only a few of these with Pickford, it may be appropriate to offer some comments on his ideas, essentially as "referee’s comments".

The most exciting of Pickford’s ideas was his suggestion that the advantage from a prosimian to simian grade in the Oligocene or Miocene included the release of the several eye-muscles from interaction with the temporal muscle by the development of a complete enclosure of the eyeball by bone. This rather simple morphological change would have had major effects on the ability of an animal to handle visual information, which would, in turn, have had major effects on cognitive processing. The effect would occur because jaw movements would not interfere with physiological nystagmus necessary for proper registration of a visual image.

I am most skeptical about Pickford’s ideas on the role of isometric scaling of skeleton on body size in primates. If Potter’s analysis cited by Pickford stands up, and isometric scaling is indeed appropriate, there remains the issue of whether the shift of resources between splanchnocranium and neurocranium was significant enough to affect gross skeletal: body scaling measures. There is also the question of whether the scaling reflects a fundamental process that constrains growth, and that departures from the scaling are impossible.

As a former acoustician, I am especially sensitive to ideas involving the processing of auditory information, and enjoyed Pickford’s introduction of ideas about the role of interaural distance as a selective pressure. Since I was not able to communicate with Pickford on this, I could not ask him to provide more documentation for the reader, to enable readers get more information than is available in Morgan’s book, which Pickford cited. I would have added citations to Masterton & Diamond (1973), Masterton, Heffner & Ravizza (1969), and perhaps most unusually, Konishi (1987) on sound localization in barn owls. This might have suggested a less significant role for inter-aural distance on capacities for localization of sound in space. Enough is known about sound localization to make it possible to prepare a detailed quantitative statement of Pickford’s hypothesis, which would make it easier to evaluate.

I was fascinated by the way Pickford pulled together the different evolving systems that cooperate in a particular way in effecting “intelligent” behavior. I like his approach to why human speech has its particular fundamental frequency and overtones — why the formants are at the frequency bands at which they lie. Pickford proposes that the frequencies optimize the effectiveness of vocalization for communication over great distances. I am not completely persuaded by his argument, because alternative methods for distance communication are known, in which these base frequencies are less critical. Dolphin calls and echo-ranging are several octaves above those of humans (Schusterman et al., 1986), for example, although their communication in water may be under different constraints from that in air. And I suspect that there would have been many arguments with his view by others at the Institute, if it had been presented there, since other species communicate quite effectively with signals at various frequency bands.

It may be an unnecessary comment, but for those unfamiliar with the matter, I should note that although the fossil record for early hominids is incomplete in many important ways, if 100 crania are known, the record is one of the best for any vertebrate