Chapter 1

Unity in Diversity

Two Skeletons

Pierre Belon du Mans, doctor, naturalist, and traveler, is one of the most significant figures in the zoology of the sixteenth century. His was an adventurous life, brought to a tragic end by an assassin in the Bois de Boulogne, Paris, in April of 1564 or, perhaps, 1565. His most famous works are a natural history of fish and a natural history of birds. They are written in the colloquial tongue (French) and not in Latin, the sign of a spirit that wanted to be free of the shackles of tradition in order to suggest an original, innovative reading of natural events.

Even today it is worthwhile to leaf through Belon's ornithological work, if for no other reason than to peruse pages 40 and 41, almost entirely taken up with the figures of two skeletons: on the left that of a man, and on the right of a bird. Even this simple juxtaposition may seem unusual and perhaps irreverent. Man and bird are shown by Belon on a plane of perfect equivalence, postmortem, which at the time was probably a component of the collective imagination reminiscent of the *danses macabres* of penitential iconography—an equivalence reinforced by the ring attached to the skull, which ideally allows one to suspend each skeleton from a hook, so as to aid examination (fig. 1).

This explicit and almost brutal presentation of a human skeleton as an object worthy of study for natural history is itself a historical choice: in particular because this is not a medical textbook. And Belon's drawings assume a further, extraordinary value precisely because they encourage the reader to perform a careful work of

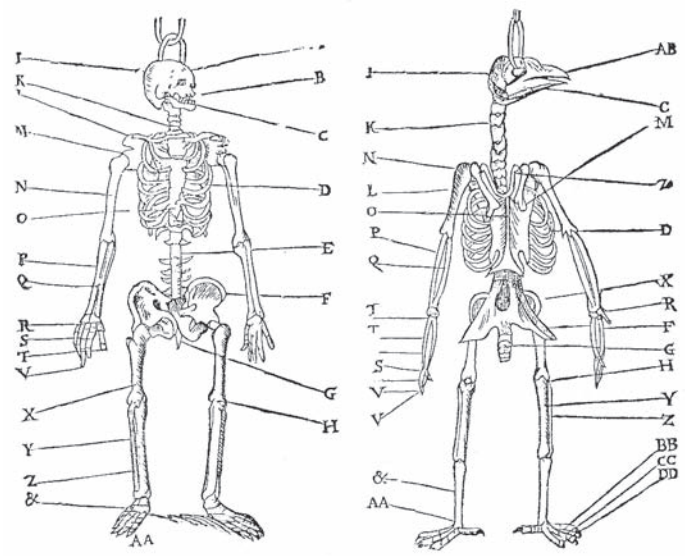

Figure 1. Comparison between the skeletons of a human being and a bird. From Pierre Belon du Mans, *L'histoire de la nature des oyseaux*, Paris 1555.

comparison. In fact the two skeletons' bones are marked by a letter or a couple of letters each, and the same letters are utilized each time to indicate which bones, in Belon's opinion, are the same in the human being and the bird.

With this illustrated guide, the examination could be undertaken, arbitrarily, in either direction. If one was more familiar with the human skeleton, one would use it as a guide to help one understand the bird's skeleton. If instead one was more familiar with the latter, one would utilize it as a model for the human skeleton. There is moreover no doubt that in the comparison offered by the *Histoire de la nature des oyseaux* the author took for granted the reader's greater familiarity with the human skeleton, as contrasted to that of the bird. Belon's attitude was perhaps influenced by the medical studies he had undertaken, even though he obtained his doctoral degree at a fairly advanced age. Moreover the theme of the book was the natural history of birds, and therefore every notion borrowed from other living organisms, including human beings, could only have an instrumental value. But one should not interpret the fact that a human

skeleton was used as a model for the study of the bird's skeleton as a survival of the old saying according to which man is the measure of all things. Man and bird, at the skeletal level at least, were considered equivalent by Belon.

The reader will begin to wonder what these two woodprints of 1555 have to do with the problems that today concern a newly formed discipline like evolutionary developmental biology. Please bear with me and accompany me in the following pages on a path that includes important milestones that have marked the history of the comparative method in biology. We shall soon arrive at the heart of those problems that most interest us.

Clashes at the Muséum

Belon having died, we can skip two centuries and a half, and remain in France, where, among the smoking ruins of the ancien régime, and Bonaparte's rising star, a center of scientific research has been born that will soon offer the international community some of the most innovative ideas in the realm of the natural sciences.

It had been Louis Daubenton, previously a trusted advisor to Georges-Louis Leclerq de Buffon, who had suggested to deputy Lakanal the guiding criteria the Convention would adopt when instituting the Muséum d'Histoire Naturelle on June 10, 1793. From its founding the Muséum had been conceived as a research center, a repository of precious research materials, and a place dedicated to public education. A group of *professeurs* was in fact called to supervise the activities of the different departments. The chair in vertebrate zoology was assigned to Etienne Geoffroy Saint-Hilaire, whom we shall meet several times in this book, while the position of *professeur* of the invertebrates was assigned, following orders from above, to Jean-Baptiste Monet de Lamarck, who as a consequence had to abandon his favorite botanical studies (in 1778 he had already published a *Flore françoise*). At this juncture, however, we are not interested in talking about Lamarck, but rather about Geoffroy Saint-Hilaire and Cuvier, three years Geoffroy's senior, whom Geoffroy requested be

appointed to the professorship in comparative anatomy. Over the course of the following years, the two zoologists experienced moments of sincere friendship and fruitful professional collaboration, but also moments of intense rivalry and animosity.

Cuvier, in his work *Le règne animal*, proposed a subdivision of animal species into four large groups or *embranchements*: the vertebrates, the articulates, the mollusks, and the radiates. According to Cuvier these large groups corresponded to four organizational designs, so different from one another that any comparison between them was an arbitrary exercise. The situation within each *embranchement*, however, is very different: because its species are constructed according to a largely shared structural design, it is legitimate, in any of these species, to search for anatomical parts corresponding to those of other species, precisely as in the comparison Belon had proposed between the skeleton of the human being and of the bird.

But Geoffroy could not believe that nature would follow entirely different designs to generate those that, ultimately, are none other than different species belonging to the one and only animal kingdom. In other words he was convinced that all animal species share a substantially common structural design. Certainly this unity of design is more easily discernible when we compare two similar animal species: for instance two species that have a spinal column, or a trunk that is articulated into segmented units and supported by various pairs of appendages, which in their turn are also made up of interconnected articulated segments. According to Geoffroy, however, even the barriers that Cuvier established between the different *embranchements* are not absolute. It is simply necessary to be more ingenious in overcoming them, finding criteria of comparison that are applicable to animals as different as a crayfish and a fish.

The problem in this latter case is not only due to the fact that the crayfish has an exterior skeleton, with all its muscles on the inside, while the fish has an internal skeleton, with the muscles around it. A no less serious problem is, in comparative terms, the position of the main axis of the nervous system. In fish, and vertebrates in general, there is a spinal cord that runs along the back of the animal, protected by the vertebrae. In the crayfish, and in arthropods in general, as well

as in many other animals, we instead find a ventral gangliar chain below the intestine. Not to worry, Geoffroy observed. In both cases there is a longitudinal nervous axis and we should not be concerned if in the vertebrates it is dorsal, while in arthropods it is ventral. In the end, it is still reasonable to think that the nervous system axis is always basically the same even if it occupies (or seems to occupy) different positions. Instead, what is the basis for our affirming that what we call "back" in a fish is "the same thing" as that which we call "back" in a crayfish? If only we could hypothesize that what we call the *ventral* aspect of a crayfish is equivalent to the *dorsal* aspect of a fish, the presumed contrast between the two anatomical arrangements would be reduced to fairly minor differences.

Nevertheless, it would still have been easy to refute Geoffroy's reasoning by maintaining that his supposed "solution" could be reduced to semantics, unless the opposite was true, and in reality it was the traditional anatomical descriptions that were hostage to arbitrary lexical choices, such as those on which Cuvier's position depended. From this point of view, the problem will not be reopened until the last years of the twentieth century. We shall discuss the issue again shortly.

In the meantime, however, in the cultural atmosphere of a Paris where Cuvier's authority had been progressively increasing, due to his undisputed scientific accomplishments and his ties to political power, Geoffroy's position, which suggested ways to compare vertebrates and Cuvier's articulates, did not have much hope of establishing itself. And the situation dramatically worsened when two of Geoffroy's students proposed a new and much less daring comparison, its two terms now being represented by a vertebrate and a mollusk.

Squid and Vertebrate

Meyranx and Laurencet examined what we know as cephalopods—the cuttlefish, the squid, and the octopus—as representative of the *embranchement* of the mollusks. And there are good reasons to attempt a comparison between vertebrates and cephalopods: more

specifically the presence in both of particularly complex and efficient eyes, and of a brain capable of performances attained only with difficulty by most invertebrates. It is important to note, though, that the resemblances between fish and squid end here. The squid for instance has neither a skull nor a spinal column, but only a very thin internal shell (the "pen," even lighter and more fragile than the better known "bone" of the cuttlefish). And the general arrangement of the internal organs in the two animals is also different, because in the squid, as in other cephalopods, the digestive tube, which is relatively short, is folded into a *U* shape, the anal opening relatively close to the mouth. But it is precisely this arrangement of the organs that particularly concerned Geoffroy's two students, by showing that the "simple" bending of the main body axis of a vertebrate, folding the animal in on itself, greatly reduced the difference between the structural design of cephalopods and that of vertebrates.

This new formal application of the comparative method, suggested by the conviction that all animals share one common basic anatomical design, was too much for Cuvier. He was probably mostly fearful that the extension of comparative exercises between representatives of his different *embranchements* would favor the diffusion of "transformistic" ideas, such as those that Lamarck had defended in his works and above all in the *Philosophie Zoologique* of 1808.

In any case the debate between Cuvier and Geoffroy, which flared up in 1830 at the Académie des Sciences, was heated and continued for several sessions, between February and May of that year. A comment by Goethe, who was in Paris at the time, and who, writing to a correspondent, hinted at a volatile situation that was about to explode in the French capital, remains famous. At that time, Paris confronted one of its many revolutionary episodes, more precisely the one that would lead to the abdication of Charles X. But it was not to the barricades on the streets that Goethe alluded in his letter, but rather to the confrontation between the two great zoologists in the rooms of the Académie des Sciences.

Time would in large measure vindicate Geoffroy's positions, in regard both to the general principle of the unity of a structural design

common to all animals, and to more specific questions, such as the equivalence of the dorsal aspect of vertebrates and the ventral aspect of arthropods and other animals. In fact, in the context of evolutionary developmental biology, Geoffroy Saint-Hilaire has, in recent years, come to impersonate the role of precursor to this new discipline on several occasions.

Topsy-turvy

Geoffroy, as we have suggested, "resolved" the problem by drawing the crayfish with its belly in the air next to a vertebrate viewed from the back and seen from above. Perhaps, he suggested, the distinction between belly and back is not really as certain or as profoundly rooted in animal organization as one would think. Ultimately, this distinction is above all dictated by the manner in which the animal relates to the external environment. The belly is the aspect facing the substrate, the sole on which the snake and the snail glide, the side from which the four legs of a gazelle or a crocodile reach toward the earth. And the back is, quite simply, the opposite aspect, the furthest from the substrate, that which an observer can see if looking down from above unless a shell or a protective carapace is interposed. The situation is less clear in the case of an animal that walks on two legs, like a human being, but even here the comparison with more conventional quadrupeds is not without merit. The distinction between belly and back becomes more arbitrary in the case of the earthworm, which, in its existence as a digger, is always completely surrounded by the substrate, almost as if it were an "all-belly" animal. There is moreover no zoology book that expresses any doubts as to the dorsoventral polarity of the earthworm, or that draws a heterodox transversal section. An established point of reference is the central nervous cord, the double ventral gangliar chain. But why ventral?

One must note that in many cases the distinction between belly and back cannot be inferred from an obvious structural polarity determined by the manner in which the animal moves around the world. The snail for instance glides on a flat sole that we have no

difficulty in designating as ventral, but in cases like the earthworm the distinction derives instead from our comparative work with other animals in which the polarity is more obvious. The fact remains, however, that in the fruit fly and in the crayfish the gangliar nervous chain is on the side closest to the substrate, while the spinal cord of the cat and the snake are instead on the opposite side.

But is there not any more objective fashion to recognize an animal's belly and its back? Perhaps it is possible that in animals as different as arthropods and vertebrates the distinction between belly and back is controlled by the same genes, just as the different positions along the anteroposterior axis of very diverse sets of animals respond to a common molecular code that can be traced back to the expression of the *Hox* genes, a family of genes that we shall discuss later.

Fifteen years ago, a brief note published in the prestigious British journal *Nature* announced that it was once more time to give Geoffroy Saint-Hilaire his due for the daring comparisons he had undertaken. In fact in vertebrates and insects, the first stages in the differentiation of the longitudinal nervous cords are controlled by the same pair of genes, those which in the fruit fly have been designated *short gastrulation* and *decapentaplegic* and which in vertebrates have their precise equivalent in, respectively, the *chordin* and *Bone Morphogenetic Protein-4* genes. These genes are responsible for distinguishing, inside the most external layer of the embryo (the ectoderm), between the cells that will remain to form the animal's integument and those that will form its nerves and brain. There are therefore good reasons to affirm the equivalence between the ventral gangliar chain of the fruit fly and the dorsal nervous cord of vertebrates.