

OBITUARY

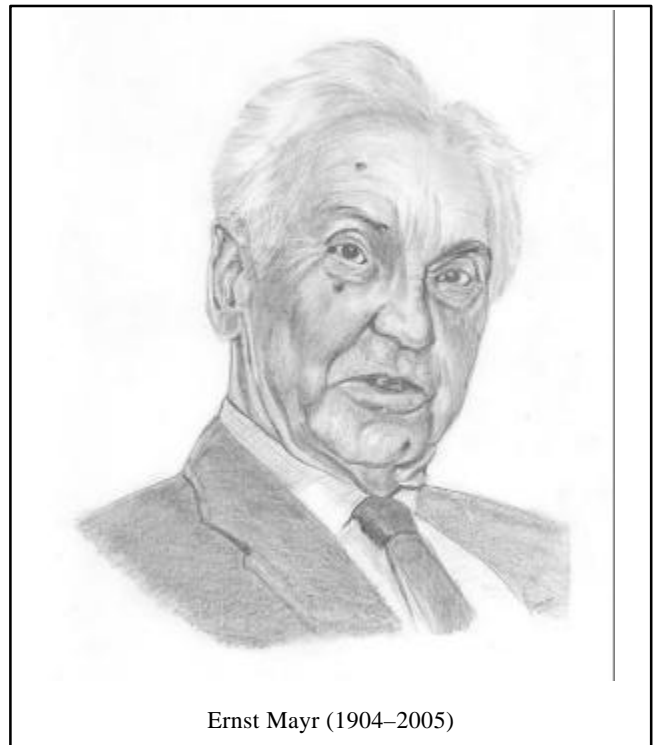
Ernst Mayr (1904–2005)

The story goes that, with the intention of celebrating Ernst Mayr's 100th birthday by commissioning an essay on his life and work, *Science* asked him who would be the best person to write such an essay. Ernst Mayr is supposed to have replied, "Ernst Mayr of course"! The resulting essay entitled "80 Years of Watching the Evolutionary Scenery" (Mayr 2004) – as sharp and precise as it is rich in historical detail – is a special gift to be savoured as it gives us a unique glimpse of the evolution of evolutionary thought in the 20th century. Even more important, it gives us a glimpse of the evolution of Ernst Mayr's thoughts.

And there is very good reason to be interested in the evolution of Ernst Mayr's thoughts, not only because it spanned a record 80 years, but also because he was one of the principal architects of 20th century evolutionary biology. Ernst Mayr remains best known for his early work in the 1930's and 1940's, culminating in his *Systematics and the Origin of Species* (1942). This, along with Theodosius Dobzhansky's *Genetics and the Origin of Species* (1937) and George Gaylord Simpson's *Tempo and Mode in Evolution* (1944), constituted the "Modern Synthesis" of evolution. The modern synthesis brought together genetics, systematics and paleontology to explain the mechanism of the origin of species with a degree of success that, notwithstanding the title of his book, was impossible for Darwin. Mayr's principal contribution was the elaboration of the idea of allopatric speciation, the idea that geographic isolation leads to reproductive isolation leading ultimately to new species. This stemmed from his biological species concept which led him to famously define species as "a group of actually or potentially interbreeding natural populations reproductively isolated from other such populations". Mayr's early ideas evolved in the backdrop of his field work on the birds of New Guinea and the tropical southwest Pacific islands. Less well known to non specialists are Mayr's mammoth contributions as an ornithologist – he is said to have "described more species

and subspecies of living birds than anyone else of his or subsequent generations" (Diamond 2005).

All this came about by a curious set of circumstances. Born in Kempton, Germany, on 5 July 1904, Mayr was all set to faithfully follow the family tradition of a career in medicine when he sighted and carefully observed a pair of rare ducks called red-crested pochards, a species that had not been sighted in Germany for 77 years. This of course was only possible because his father had inculcated in him an abiding interest in nature by taking him out on Sunday natural history expeditions. That's something to think about and emulate for those of us in India who keep complaining about our young people being blindly driven to medical and engineering colleges! The citing of the pochards brought Mayr in contact with Erwin Stresemann, a famous German ornithologist of the time. Interestingly, it is the same Erwin Stresemann that India's foremost ornithologist Salim Ali went to train with and about whom he wrote "I considered him my guru to the



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end". Stresemann lured Mayr to abandon his medical career and do his Ph D with him in ornithology, by offering him a position at the Berlin museum and an ornithological expedition to the tropics. The catch was that the Ph D was to be completed in 16 months. Mayr got his Ph D in time, got the promised job at the Berlin museum, which he described was like being "given the keys to heaven" and went on a 2 year expedition to the southwest Pacific which he said fulfilled "the greatest ambition" of his youth. The expedition led to a job at the American Museum of Natural History where he worked for about two decades before moving over to Harvard University as Agassiz Professor of Zoology.

In addition to ornithology and evolutionary synthesis, Ernst Mayr has played a major role in the development of the history and philosophy of biology; it would not be an exaggeration to call him the father of the modern versions of these fields. Mayr's persistent theme has been that biology is different from other sciences and that the physics-centered history and philosophy of science is of little relevance to biology. There is much truth in his assertions, given that history and chance play such important roles in biology. Mayr celebrated his 100th birthday by publishing a book entitled *What Makes Biology Unique? Considerations on the Autonomy of a Scientific Discipline* (2004).

Even after accounting for his longevity, Ernst Mayr will rank among the most prolific of biologists – he published over 700 papers and 25 books, 14 of them after he turned 65, and 5 of them after he turned 90! Not surprisingly, Mayr was much decorated with awards and honours the most notable being the so called triple crown, consisting of the Balzan Prize (1983), The International Prize for Biology (1994) and The Crafoord Prize (1999). Jared Diamond, who came under Mayr's spell at the age of 16 and went on to develop a parallel successful career in ecology and evolutionary biology, in addition to being a membrane physiologist, attributes Mayr's extraordinary success to a multitude of factors including outstanding memory, a quick learning ability, almost super-human industriousness and perseverance and the ability to forge friendships and collaborations with ordinary people and younger colleagues (Diamond 2005). It is hard for me to believe that Mayr's legendary self-confidence was not also an important factor. Diamond says politely that Mayr "was self-confident without being overconfident". I was looking for a similarly polite literary technique when I came across the inimitable Salim Ali who in *The Fall of a Sparrow* (1985), recalls "In the bird room of the museum I also had my first meeting with Ernst Mayr, another brilliant and upcoming young protégé of Stresemann who had just returned from a major expedition in the New Guinea region, and whom I met again in the USA..." and goes on to say, "Ernst Mayr is currently Emeritus Professor of Zoology at Harvard University and undisputably among

the topmost biologists of the world today. Unfortunately – in marked contrast to his mentor Stresemann's unassuming modesty – Mayr makes you feel he is not unaware of the fact".

There is at least one area where Mayr's self-confident, radical, uncompromising stand will provide rich material for future historians. Mayr held a rather dim view of the role of mathematics in the study of evolution. There are many who would consider the contributions of Ronald Fisher, J. B. S. Haldane and Sewall Wright at least as important, if not more, than the contributions of Mayr, Dobzhansky and Simpson, to the modern synthesis and the bringing together of genetics and evolution. It is true that the work of Fisher, Haldane and Wright was more concerned with evolutionary change within populations while that of Mayr, Dobzhansky and Simpson was more concerned with evolution between populations, but that does not necessarily mean that one was more or less important than the other. Evolution is about change both within and between populations. But Mayr had no such notions of equality and compromise. In his celebrated *Animal Species and Evolution* (1963), Mayr wrote "The Mendelian was apt to compare the genetic contents of a population to a bag full of colored beans. Mutation was the exchange of one kind of bean for another. This conceptualization has been referred to as 'beanbag genetics'. Work in population and developmental genetics has shown, however, that the thinking of beanbag genetics is in many ways quite misleading. To consider genes as independent units is meaningless from the physiological as well as the evolutionary viewpoint". And in a Proceedings of the Cold Spring Harbor Symposium volume on Quantitative Biology 1959, Mayr wrote, "Fisher, Wright and Haldane have worked out an impressive mathematical theory of genetical variation and evolutionary change. But, what precisely, has been the contribution of this mathematical school to evolutionary theory, if I may be permitted to ask such a provocative question?" It would have taken much less to provoke Haldane who in his characteristic style replied, from the Genetics and Biometry Laboratory, Government of Orissa, India, in an article entitled *A Defense of Beanbag Genetics*: "While Mayr may certainly ask this question, I may not answer it at Cold Spring Harbor, as I have been officially informed that I am ineligible for a visa for entering the United States. Fisher is dead, and when alive preferred attack to defense. Wright is one of the gentlest men I have ever met, and if he defends himself, will not counterattack. This leaves me to hold the fort, and that by writing rather than speech". Haldane then went on to demonstrate the utility of beanbag genetics, concluding that "One of the important functions of beanbag genetics is to show what kind of numerical data are needed" (Haldane 1964).

It does not take great intelligence to see that both Mayr and Haldane were partly right and that both were also partly wrong. Of course considering genes as independent

units is sometimes misleading but it is equally obvious that a one-locus, two-allele model can often tell us great deal about the rate of evolutionary change. Why then do geniuses of the stature of Mayr and Haldane appear to behave so unreasonably? I think we should be glad that they do so. Few can be so competent as Mayr to bring out the defense of beanbag genetics in a manner that few other than Haldane can provide. It is due to the uncompromising position of competent radicals like Mayr and Haldane that the scientific community can discover the genuine limits of the various imperfect ways we devise to solve nature's mysteries.

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