Interview with Ernst Mayr

This year is the 60th anniversary of the publication of Ernst Mayr's Systematics and the Origin of Species, *a key book in the evolutionary synthesis of the 20th century. On May 9th,* BioEssays *went to Bedford, Massachusetts to interview Dr Mayr at his home there. What follows is an edited transcript of that interview.*

BIES: *Dr Mayr, I'd like to begin with several questions about your history and the history of ideas in evolutionary biology, then focus on some questions about specific issues in evolutionary biology and, finally, conclude with a discussion of some matters in the philosophy of science and of biology in particular. Let's begin with the matters of history. This year is the 60th anniversary of publication of your book* Systematics and the Origin of Species, *one of the great classics of evolutionary biology. It was published 5 years after another classic in the field, Theodosius Dobzhansky's* Genetics and the Origin of Species. *You were a colleague and friend of Dobzhansky's, but you clearly felt that his book neglected certain important issues. What were the specific factors that led you to write* Systematics and the Origin of Species? (Figure 1)

EM: The reason I wrote the book was that I was asked by Columbia University and, more particularly, by Prof. [L.C.] Dunn of the zoology department at Columbia University to give one of the Jesup lectures. This was a famous lecture series dealing with many problems, but particularly with evolution and Dobzhansky had given a previous lecture. Now Dobzhansky had a feeling that he had not done a good job with respect to the problem of speciation and the whole question of biodiversity and its evolution. In his 1937 book he has no chapter on speciation, and he was very much interested in a more detailed treatment of this subject. He knew from many conversations that we had had, that this was a field in which I was not only interested but was also really an expert, and so he suggested that I give a Jesup lecture in this field, with the understanding that it might lead to a book later. Now to give the full details: the 1941 Jesup lectures were given by two people. I gave two lectures on speciation and the species problem in animals and Edgar Anderson, a botanist, gave them on plants. Anderson got sick, however, and he never submitted a manuscript. I was, therefore, asked by Prof. Dunn to expand my two lectures into a book. That book was *Systematics and the Origin of Species* and there I dealt with the neglected parts of Dobzhansky's book.

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BIES: *Your book incorporated Neodarwinian ideas about genetics but when you first entered the field of systematics biology you believed in the inheritance of acquired characteristics, at least to some extent. At what point did those ideas fade away and what caused you to abandon them?*

EM: It is indeed true that at the time when I started working at the American Museum of Natural History in 1931, that I was still a Lamarckian, believing in the inheritance of acquired characteristics and there were good reasons for that which are usually forgotten by the historians of genetics. When in 1900 Mendel was rediscovered, there were three geneticists who were particularly interested in the evolutionary aspects of genetics. These three were Bateson, DeVries and Johannsen. All three of them were typologists; all three thought new species originated with major mutations, 'saltations', and all three of them rejected natural selection. That is usually carefully concealed by the geneticists, but this was what we, the naturalists, were fighting in particular. We all knew that speciation and evolution was a gradual process and since geneticists believed it had to be by saltation, we had to find a different answer for the gradualness. The only answer that was available was Lamarckian gradual acquisition of new characteristics by use and disuse etc. So the important thing that had to happen, and this did indeed happen in the years between 1916 and 1932, was that the geneticists completely rejected the saltationist views of the early Mendelians and showed that genetic changes could happen through very small mutations and that even very small mutations in the long-run could be of great evolutionary influence.

For me, a crucial interaction was with an ornithologist at the American Museum of Natural History, namely James P. Chapin who had got a PhD at Columbia University in 1920 dealing with bird geography and ecology. He was, of course, fully familiar with the modern genetics of T.H. Morgan, who was in the same department. He and I had numerous conversations on evolution and he convinced me of the importance of the findings about the effects of small mutations etc. and of the invalidity of any belief in inheritance of acquired characteristics. Most importantly, he helped me to see that the gradual evolution that we naturalists had insisted on could be explained by the new genetics of Fisher and the other modern geneticists and didn't require any of the saltational interpretation of the early Mendelians.

On the other hand, the geneticists during the 1920s and 1930s completely adopted natural selection which they had previously rejected and so there was an entirely new basis for a reconciliation with the naturalists. Yet the geneticists, and you can look at the writings of Fisher, Haldane and Wright to see this, did not understand speciation at all, and correspondingly

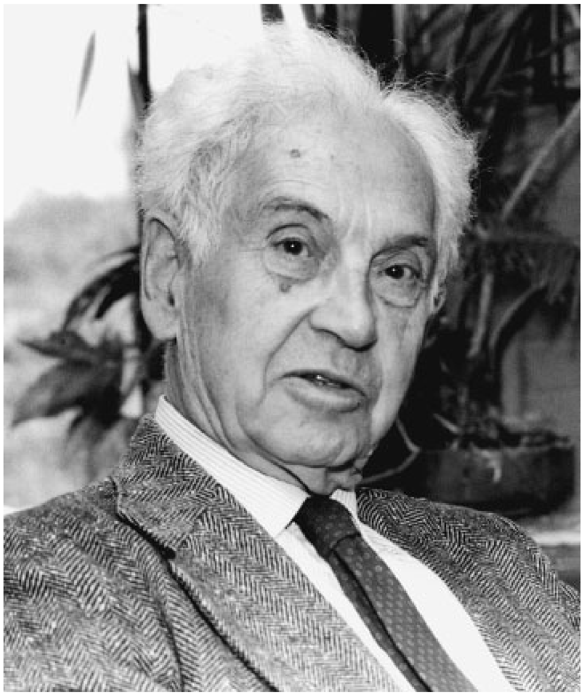

Figure 1. Dr. Ernst Mayr. (Photo courtesy of Helen Reed/Harvard Photo Services.)

they had little to say about speciation in their books. They either say the usual things without any detail, or they even said wrong things. The entire half of evolutionary biology that deals with the problems of biodiversity, speciation and macroevolution, that half was left out of the writings of the leading evolutionary geneticists and this is where the Synthesis came in beginning with Dobzhansky and was followed by myself and by people like Julian Huxley, and, of course, on the fossil side by G.G. Simpson. The naturalists of various kinds, working on living things and on fossil types filled in the gaps left by the geneticists and showed how biodiversity could evolve. Finally, since we had the two fields (one knew about biodiversity, the other knew about adaptive evolution at the gene level) they could be brought together and this happened through the writings of Dobzhansky, myself and several others.

BIES: *I'm curious, while you were at the American Museum of Natural History, did you have much interaction with Simpson himself, whose interests and thinking were of course an important part of the evolutionary synthesis?*

EM: Well the interesting part is that, although I had lunch at the same staff lunch table with Simpson for something like 20 years, I cannot remember that we ever had a scientific conversation. I was a little bit shocked by this realization many years later, so I wrote to Simpson myself and said 'what is your impression?' He wrote back and said 'Yes, you're quite right, we had no scientific conversations'. But this is not surprising if you knew Simpson; he just wouldn't talk about scientific things and this was the major complaint from his students. One of the students once made a malicious remark: 'He's so afraid that you might steal one of his ideas!'.

BIES: *Let me ask you about two other men whose names are not widely familiar today. I'm referring to Erwin Stresemann and Bernard Rensch. Would you comment on how they influenced you?*

EM: I mentioned earlier that the geneticists had no idea about speciation and in fact American taxonomy was very backward. It was still very typological, or what's sometimes used as a derogatory word, very Linnaean. Now Europe was way ahead of America in understanding biodiversity and speciation and two people that were in this category were my teacher Stresemann and my Berlin colleague Rensch. Stresemann had published on the biological species concept since 1919; Rensch had published a whole book on the new systematics in 1929 and the school of these Europeans was just miles ahead of anything that existed in North America. That, of course, was a considerable advantage for me because I came as the 'prophet' of this new way of thinking. [E.O.] Wilson, in fact, has a paragraph in his autobiography (1994) about his teacher having discovered my *Systematics and the Origin of Species* as the new wave of developments in systematics and this was indeed true. This whole way of thinking came into America primarily through my 1942 book. It is still actually being used and Harvard Press brought out a new printing just last year with a new introduction.

BIES: *Let me go back before you arrived at the Museum to what was your first major foray into field work, namely a remarkable expedition you undertook in 1928 at the age of 23/24 to study the Birds of Paradise in New Guinea. What did this experience feel like? It's the kind of experience most young men would dream of. Did you feel you were following in the footsteps of previous evolutionary explorers? Did you see it as a great adventure?*

EM: Well, actually it was my dreaming about expeditions and exploring the tropics that led to my whole career. I was a medical student but I met Stresemann in connection with some ornithological discoveries I had made. Although I intended to become a doctor, I had always been interested in natural history and I worked during the Summer vacation as a volunteer in the Natural History Museum in Berlin. There, during coffee breaks, I would rave about the wonderful tropics and the expeditions of Humboldt and Bates and Darwin, and one day Stresemann said to me 'Young man, if you become a medical doctor, you will never see the tropics; it's just out of the question' and when he saw how my face fell, he said 'Now, look here, I could make it so that you can see the tropics, but you would have to stop your medical training for a while, get yourself a PhD in zoology and then I can get you on an expedition'. Well this would have been a major step in my life, but I was sufficiently enamoured with the dream of the tropics and exploring that I did take that major step. After I'd got that first medical exam behind me, I took up zoology and got my PhD, incidentally at 21, then waited for the expedition. Stresemann tried first one thing, then another thing, and finally heard that Lord Rothschild in England had a collector in New Guinea who had just had a stroke and couldn't collect any more. Rothschild still had a programme of collecting, however, and he was looking for a replacement. Stresemann had a lot of influence and managed to persuade Rothschild that I was just the right person for the job. One could easily call that a lie because I had never skinned or shot a bird; I'd never been in the tropics. However, I was energetic and resourceful and full of enthusiasm and Rothschild, after he talked with me at an International Congress, was also convinced that I was the right person and I was sent to New Guinea as a straight collector. Not to study the Birds of Paradise, nothing as high-ranking as that. I was sent as a straight collector, to collect particular species of Birds of Paradise. There were some very rare species of which only 1 or 2 existed in collections and nobody knew where they had come from. All the expeditions had failed to find them but there were several mountain ranges that hadn't yet been properly explored and my assignment was to go to three of these ranges and collect all birds, in particular Birds of Paradise, hopefully including the rare ones. So I went to New Guinea, I was incredible successful, primarily because I was sent help in the form of Javanese assistants who had been on many expeditions and they taught me how one runs an expedition. Anyway, I finally collected more than 3000 bird skins, a very complete collection of the three mountain ranges (two of which had never been explored before), but not a single one of any of the so called rare species of Birds of Paradise. Stresemann thought there was something funny going on if we couldn't find these missing birds; they must not exist. So he went to the Rothschild museum and studied the skins of the rare Birds of Paradise and finally concluded that they were hybrids between two other species. So even though I was unsuccessful in collecting them, my expedition resulted in the clarification of the problem of the rare Birds of Paradise.

You asked about my feelings? Of course, it was absolutely fantastic; I stepped from the shore into the tropical jungle and at that time New Guinea was virtually untouched. On walking one day into the interior, I came to villages where no white man had ever been. In some places, I walked 3–5 days into the interior. And of course to wake up in the morning and hear all these tropical birds calling and singing around you was an overwhelming experience. Still now when I think back, I can hardly believe that I was so lucky to have had that experience. In a way, I was more in the wilderness than, for instance, Alfred Russel Wallace; if you read his descriptions he always stayed very close to settlements. There are so many things that happened that I could recount but which would take us away from the science. I was reported massacred by the natives and the Dutch sent out a punitive expedition on my behalf and in another place the chief wanted me to marry his daughter! The number of interesting experiences I had is just endless. I also had some very tough travelling. For instance, I went from Dutch New Guinea to the Mandated territory, that is a stretch of about 100 miles and there were no roads or means of transport and I just partly walked, partly used native canoes. The natives were very reluctant to help me but I survived it all.

BIES: *Let's move forward into the 1940s. The impression one gets from reading the literature is that, in a sense, what we call the Evolutionary Synthesis really gelled as something in people's minds at the 1947 conference in Princeton. Is that perception true or was the sense of the synthesis having been created already present several years before?*

EM: No, the conference was only a confirmation because when we came to Princeton, Dobzhansky's book was out, my book had come out in 1942 and at the same time there was also a summary of all these things by Julian Huxley. In the meantime, the geneticists had realised that there was a whole field of study in biodiversity and its origin, and the naturalists had realised that the ideas of geneticists which had been derived from the early Mendelians had been all wrong, so by 1947, there wasn't really any more argument because both sides, by each learning what the other had achieved, realised that there wasn't a conflict any longer between the thinking of the geneticists and the naturalists.

But there was one exception: the majority of geneticists at that time still thought the gene was the unit of selection. The naturalists always said, as had Darwin, that it was the individual organism that was the real unit of selection, the individual either survives or it doesn't; it either reproduces successfully or it doesn't but a gene is never isolated, it never walks the street by itself, therefore it cannot be a unit of selection because you can never select a gene. Eventually by about 1975, most geneticists had come round to the idea that the gene itself was not a unit of selection and by 1984, there was a well known paper by Lewontin and Sober which emphatically said so but this had been my view for years. I wrote papers about 'beanbag' genetics showing that taking genes in and out of a population is not the important thing because it's individuals that really count. The mistake the gene selectionists make is that they do not distinguish between selection 'of' and selection 'for'. Selection 'of' is 'What is the target of selection?' Very clearly there are only 2 or 3 possible targets. One is the gametes; they are directly selected. How it's done, why certain gametes can enter an egg while others cannot, all of this sort of thing is still very nebulous, but the gamete is still potentially a target of selection. The next one is the individual. Then the third one, of which there is a great deal of confusion in the literature, that is the social group. People are either for or against group selection but that is a lot of nonsense; it depends on what kind of group. If it's a casual swarm of fish in the water and the composition changes hourly, then of course it's not a target of selection, but if you have a group of hunter gatherers and they have a particularly close cooperation then that particular troop is selectively superior to the next one with whom they are feuding if there is fighting within that group. So the minute you mention it in detail, it's very obvious that a social group can be a target of selection. It can be superior to some social groups in encounters between them. So we only have three possible targets of selection. Whereas there can be selection for certain gene variants, such as the sickle cell hemoglobin allele but that is an indirect effect.

It is sometimes argued, however, that the species is also a target of selection. Well the species cannot be a target because it's not a thing as such which is selected or not; all cases of so-called species selection are cases where individuals are selected; individuals of one species are inferior to individuals of another and so the species goes under. This was described a long time ago by Darwin. For instance, importing British species of plants and animals to New Zealand often led to extinction of native species. But it was that the individuals of British and NZ species were all one mixed population now and British individuals were superior to the native individuals. I prefer to talk of species turnover or species replacement because the species itself is not selected.

BIES: *I'd like to ask about your personal interactions with the other founders of the evolutionary synthesis. You've already talked about your non-interaction with G.G. Simpson, but can you tell me how you got on with some of the other men and what they were like?*

EM: OK, I'll start with Julian Huxley. I knew him for a very long time as an ornithologist and we had met at international ornithological congresses and he was an enthusiastic outdoor birdwatcher so we got along just fine. Now Huxley was a really good friend; we visited each other's houses and our wives were friends. My only criticism of him was that he was so full of ideas and plans that there was very little cohesion between the things that he did. His book *The Modern Synthesis* was very good in detail but was chaotic in its composition. I now know also why he used to come and visit me in New York. He would sit across the table from me, he would put out a pad and say 'now Ernst, what's the latest in evolutionary biology?' I would tell him and he would eagerly scribble and then I think he worked it out in a little more detail, fed it to his secretary, she put it together and there was his book! I'm exaggerating but you get the spirit. And so there really was very little in his 1942 book that was his original ideas. And no one ever quotes him as the originator of this or that idea.

BIES: *Is it correct to say that in evolutionary biology, he's known mostly for the term 'the modern synthesis'?*

EM: That's right and he had that term before there was a modern synthesis.

BIES: *With respect to Sewall Wright, is it accurate to say that you were strongly influenced by his ideas? What were your interactions with him?*

EM: Well, the story that I was greatly influenced by Sewall Wright is mostly the concoction of Michael Ruse. Actually I was not influenced very much, if at all, by Sewall Wright. He was a mathematician and looked at everything from that point of view and it just didn't make sense [to me]. I used to sit down next to him at Cold Spring Harbor when there were no other dinner companions and I'd try to get a conversation going and I never was successful.

However, what I am about to say is more of a joke than an anecdote but I always like to say that I basically owe to Sewall Wright my whole scientific career. I will explain to you why. In 1939, the American Association for the Advancement of Science organised a big symposium on speciation at the Ohio State University. I was totally unknown at that time, I was just a museum curator and not one of the University people. But Dobzhansky knew me and invited me to give a talk. The main symposium took place in the largest hall which seated 3000 people and had a huge platform where they had concerts and plays, and for the sake of the conference lectures they had a lectern at the front of the platform with a fixed microphone on top of it. Sewall Wright was the speaker just before me and he went up there and started to talk into this microphone. Then he needed to write his mathematical formulae on a blackboard so he walked to the back of the platform where there was a great long series of blackboards and started to write. But of course, he was a mile away from his microphone and nobody could hear a thing! Every once in a while he would get the feeling that he should talk to the audience and he'd walk towards the front of the platform, but he was still too far away from the microphone and they still couldn't understand anything. And then, as was always the case with him, he over-ran his time but finally he concluded. As he was such a famous person, there was great applause and since many people had come primarily to listen to him, as soon as he had finished, there was a mass exodus from the hall. I was the next speaker and I went to the lectern and I never removed my mouth from the microphone! I talked about an interesting subject, the speciation of the island birds, and I had beautiful painted coloured slides, prepared for me by an artist at the American Museum of Natural History. I talked for about 30 minutes and got tremendous applause from the few people left in the hall. About 30 minutes later, L.C. Dunn, who was the power at Columbia University, came up to me and asked me if I'd be willing to give one of the Jesup lectures. And so I always say the reason I was considered such a brilliant lecturer was because I followed Sewall Wright!

BIES: *I believe that your relationship with J.B.S. Haldane was much warmer. You discovered, for instance, that he too was an enthusiastic naturalist, is that correct?*

EM: Yes, I never knew that Haldane was such a good outdoors man until one day when I visited him in Calcutta he took me to Orissa and we stayed overnight at the government guest house in Bubaneswar. The guesthouse was right at the edge of the town, and right next to us were fields and a little native village and really untouched Indian nature. I was an ardent birdwatcher at that time, this was in 1960 when I was about 56 years old. In the morning at about 5am I was out there and there were the most marvellous birds, also a jackal feeding on mice, the natives came out of their huts, it was a brilliant morning and I was just absolutely inebriated by this beautiful landscape. I came back to the guesthouse at 7 AM for breakfast and I was still full of enthusiasm and I held forth on how wonderful it was and all that. Suddenly Haldane interrupted me rudely saying 'Ernst, why didn't you take me along?' I said 'Well, I didn't know you'd be interested' and he said 'Of course'. So the next morning the arrangement was that he would knock at my door at 5 AM and he surely did and I took him out and we saw all these wonderful things again the next morning and he was just as enthusiastic as I was. We came home and we had had the most wonderful outdoor excursion and ever since that time I realised that in addition to all his mathematics and physiology, how much he was basically also a naturalist.

BIES: *One of the other key persons in the evolutionary synthesis was R.A. Fisher. Did you have much contact with him?*

EM: Yes, I had a number of interactions with Ronald Fisher. I was editor of the journal *Evolution* and he and E.B. Ford submitted a joint paper in which they criticised a paper by Sewall Wright that I had published the issue before. In his paper, Wright showed, or thought he showed, that a particular change in a polymorphic pattern of one of his butterflies that Ford worked with was not a systematic selective change but could as well be due simply to random drift. Fisher and Ford were infuriated by this paper and they sent a rebuttal which not only used language unsuitable for a scientific journal but also didn't in any way answer Wright's criticism. They simply reiterated that it was systematic selection. Well, I read it as editor, and I said to myself 'I cannot publish this as it is'. I gave it to two other readers and they both agreed it would have to be changed, first of all the language had to be cleaned up, and secondly, Fisher and Ford had to come to grips with the actual point of Wright's criticism. So with both of these things I returned the manuscript to them together with the reviewers' letters and I got a very curt reply back from Fisher saying that since obviously the editor of *Evolution* refuses to publish any paper that doesn't conform to his ideas, they didn't want to embarrass him and they were herewith withdrawing their paper.

Now there is a continuation to this story. A year or two later, one day at the luncheon table at the American Museum, Simpson said to me 'Oh, Sir Ronald came to see me this morning and when we had discussed what we wanted to discuss, I was going to bring him up to your office but at that moment Sir Ronald realised or remembered that he had an important appointment in downtown New York and left in a great hurry'. About another year later, I was in Cambridge, England, visiting Bill Thorpe. He was a very friendly person and as soon as I was there he said now we have to make a schedule for you, whom you have to see and all that and he mentioned Sir Ronald. I said 'Oh, don't bother. Sir Ronald doesn't want to see me' and Thorpe being such a kind person said 'Of course he would, naturally we must see him'. Well, I said 'OK, but it's your responsibility'. So he called his secretary and Sir Ronald wasn't there but she said that he'd be back at 11 o'clock. And so we timed it so that Thorpe herded me to Fisher's villa at 11 o'clock. Sir Ronald himself opened the door and he stepped outside just between me and Thorpe and in such a way that he turned his back to me and was facing Thorpe and started to talk to Thorpe and I was totally cut out. At this point, I thought well I can play that game too and I rotated around, facing him again from the other side. Then he had to give up and we went inside and, after that, everything was reasonably civil.

BIES: *How did your career change after you moved to Harvard?*

EM: Well, going to Harvard really changed my life because in New York I had these huge ornithological collections, I could work as an ornithologist there for years and years. The collections at the museum in Harvard, in contrast, were quite small and there wasn't much work for me and since by that time I was very much more interested in evolution and beginning to be very interested in philosophy and all that, I completely switched over to those other fields and worked very little on bird material and worked mostly on conceptual papers. That was expected of me and of course, I was not teaching any ornithology but I taught evolutionary biology and so forth.

BIES: *Let's come right up to the present and your recent book*, What Evolution Is. *This is I think quite an important book because it's the first one aimed at the general reader. It really tries to give a complete overview of concepts of evolution in a clear, jargon-free way.*

EM: That is true and, in a way, it's rather astonishing that this hasn't been attempted before. My book will be most successful in the hands of a teacher who has wide knowledge and if the pupil doesn't get what I say in concise sentences, the teacher can fill in. People ask why didn't I say more about this and that. Well, the minute I would have done that it would have become a huge tome.

BIES: *Let's turn to some of the specific ideas in* What Evolution Is *and in particular with your idea that the primary nature of natural selection is elimination of the less fit. This is something that you emphasize. To some people it might seem, however, that this is a fairly conventional idea of selection and is what most selective events consist of but you obviously feel that this emphasis is not given in many other treatments. Is that correct or am I misreading it?*

EM: Well, I feel strongly that if natural selection is primarily selecting for the best, you would never have got a peacock's tail. So selecting for the best means eliminating most anything you see, eliminating most variation altogether. If you eliminate only the worst, however, you still have a lot of variation left. Selecting against the worst means two things in particular: A) It gives full steam to sexual selection because much that this produces is not the best outcome in the sense of natural selection. B) It leaves a lot of variation with 50:50 chances of survival. Then it may be largely a matter of chance which one will happen to succeed. [S.J.] Gould made this point in his book *Wonderful Life*. He's quite right that if you have a lot of variation left after eliminating the worst, it's just a matter of chance which of all these variants will be picked up. That's why the unpredictability of evolution is much more easily explained by the elimination concept rather than under a strict election concept that gives primacy to selecting for the best. The minute you realise that, you will agree that there *is* a difference between selecting for the best and eliminating the worst.

BIES: *One of the points I really liked in it was your explanation of why survival of the fittest is not a circular idea. It is often said that evolutionary biology is based on this tautology and you explain that fitness really is a statistical property reflecting survival in relation to adaptation.*

EM: Well, all of natural selection is of course a statistical thing.

BIES: *Do you think that this might be a reason why many non-biologists have trouble understanding the basic idea of evolution and natural selection? That the idea of probability and statistics is influencing outcomes is actually not always a comfortable one for the layman.*

EM: Well I deal with the problem in a somewhat different way. The majority of people are typologists. When they speak of black, it's a type etc and so they do in natural selection. Certain types are selected or selected against. The idea that what we are dealing with in living nature exclusively is biopopulations is one of the hardest to accept.

BIES: *I'm also curious about your view of molecular evolution. In the first part of* What Evolution Is *you talk about the various proofs that evolution has taken place and you show that they are really overwhelming and indeed you include some of the evidence from molecular evolution. Yet, if one looks at the history of that field, it really seems to have taken place fairly independently of work on organismic evolution. Organismal evolution, after all, is about phenotypes while most of molecular evolution does not go anywhere near phenotypes. What do you think are the most important contributions of molecular evolution to the larger issues of evolution?*

EM: That question is frequently asked, what really is the contribution of molecular evolution, or more broadly, molecular biology. The first one, of course, is that molecular evolution and in particular the work on the genetic code have shown that life on Earth as it now exists clearly derived from a single origin. Now that's an extremely important thing. There may, of course, have been other separate origins of life, but they all have become extinct since. A second major contribution is Francis Crick's Central Dogma, in that information can go only from nucleic acids to proteins but never from proteins to nucleic acids and that principle was of course the final nail in the coffin of the inheritance of acquired characteristics. A third contribution of molecular biololgy was the discovery of Francois Jacob and Jacques Monod that there is a distinct class of genes, regulatory genes.The fourth is really just a consequence of the third one, namely the revitalization of the field of developmental biology, which after the downfall of Entwicklungsmechanik in the 1930s had been sort of 'dead' until well into the development of the field of molecular biology, And 'Evo-devo' is an outgrowth of that. Now developmental biology again is a very important, if not the most important branch of biology, and that is strictly due to molecular biology.

BIES: *Let me ask you though a specific question about where molecular evolution might lead to one set of interpretations, whereas a more organismal approach could lead to a different set of interpretations. That is specifically the Three Kingdom Hypothesis of Carl Woese, in which, based on molecular distances and some biochemical characteristics, he divided life into three separate kingdoms. You have criticised this idea. Could you summarise what you think is wrong with it?*

EM: Only a non-biologist could have come up with this 'Three Kingdom' declaration because you find that the prokaryotes and the eukaryotes differ in about 30 different characteristics, one of them of course being the presence of a nucleus, others being the presence of cellular organelles, sexual reproduction, the existence of well-formed chromosomes etc. Now, if you had to divide the group of prokaryotes from that of eukaryotes, quite obviously that is the place where the cut has to be made. Now let's compare the Archaebacteria and the Eubacteria and you find that in 27 of the 30 characteristics they agree with each other, it's only in three of them they differ. These three are fairly minor, having to do with ribosomes and ribonucleic acid. Only in three of them are the Archaebacteria closer to the Eukaryotes. That is simply an idiotic classification to say that these three things justify that the Archaebacteria have the same rank as the Eukaryotes. Anyone who knows anything about organisms would say that this is nonsense. Even some of Woese's own followers have frankly admitted that this is very bad! In a sense, this kind of thinking followed the cladistic idea that branching points are the places where new taxa start. I have written a very large paper in which I show that this whole cladisitic way of making classifications is a total failure. So, I have no doubt that this will eventually be seen because classification means just one single thing, it means making classes and then you have to define what a class is. The dictionary has several definitions but the one that applies to the field of systematics is that classes are groups of things that are similar to each other, so that to be included in a class, these items must be similar to each other. That is true for any Darwinian classification, and it is not necessarily true for most phylogenetic assemblages, therefore I don't even refer to these systems as classifications. In fact a young philosopher has said that speaking of cladistic classifications is an oxymoron, and he's absolutely right, it's just nonsense! You can't have branches that are highly diverse, with the stem and the crown of the branch completely different. That's just in complete conflict with the whole concept of classification.

BIES: *I therefore take it that you are unhappy with the idea that birds are dinosaurs, which follows from cladistic thinking?*

EM: I am very delighted that the cladists speak of birds as avian dinosaurs because it shows how idiotic their whole scheme is!

BIES: *It is ironic in view of this conversation, that it may have been you who invented the name 'cladistics', at least according to David Hull in his book* Science as a Process *(1988). Is that true?*

EM: Well, I've invented some other terms too. That which a cladist produces and calls a classification I call a cladification because it's not an assemblage of classes it's an assemblage of clades.

BIES: *Let's go back to the subject of speciation and, in particular, how you view an idea that you proposed in 1954 on the effect of pleiotropy and connectedness of gene action in speciation. In effect you said that once you've selected for one genetic change in a new group that finds itself in a new environment, that because of the interconnectedness of gene action and pleitropy itself, you then create pressure for new changes. This consequence is that once you start down a certain road you're likely to select more and more in order to re-optimise the genetic machinery. I believe you called this process the genetic revolution in speciation. How do you view this idea today?*

EM: I still think that this is an important idea. The key thing is that the smaller the population, the more important the chance factor becomes because it no longer involves a very strong selection for just one kind of thing. By pure chance a lot of genes and gene combinations simply drop out and so, in small populations, improbable combinations are quite frequent and they may be the starting point of something very interesting which, in a large population, would never happen. Among the classical population geneticists, the only one who saw that was Sewall Wright. Fisher and Haldane never saw it and they would say that small and large populations are one and the same thing. In fact Fisher said something that was completely and utterly wrong, he said 'the larger a population, the faster it will evolve'. We now know that the truth is exactly the opposite. The larger a population, the more inert it becomes because it is difficult for any change to penetrate through a large population. That is why Wright had his shifting balance theory by which a little piece of a population went outside and changed outside, in the spirit of my 1954 paper. Then as a unit, it goes back inside again and spreads into the large population. The critics of Wright, in particular [Jerry] Coyne, claim that this whole thing of Wright's is quite impossible, it just can't happen for many reasons. But you have to give Wright credit, that at least he saw there was a genuine problem here, one which Fisher and Haldane didn't see.

BIES: *In effect, therefore, do you still believe in the idea of the founder effect in speciation, an idea that has largely fallen out of favour with many evolutionists, not just Coyne?*

EM: I observed that in tropical island regions among closely related species the most aberrant ones invariably were peripheral and peripherally isolated and also small. I concluded that small gene pools were less conservative than large ones. They are more easily reorganised.

BIES: *I believe that Dobzhansky also believed in the importance of founder effects. Did you have any important points of disagreement with Dobzhansky?*

EM: Well, we disagreed occasionally. One thing in particular was that he was a much more devoted follower of Sewall Wright than I and he also thought that a great deal of variation in nature was neutral, was just chance. For instance over his work on the little desert flower Limanthus, I disagreed with him. Another example was the issue of human blood groups. For the longest time, Dobzhansky was convinced that the different blood groups had no selective significance at all while I was convinced of the opposite. And so, I tried to work with some haematologists to prove that there was a difference. I was unsuccessful in New York and when I came to Harvard I tried it here. The first person I tried was quite a famous geneticist interested in blood groups, but he turned me down. Finally somebody said that there was a Professor Diamond at the Children's Hospital who was a great blood group person. So I called him up and presented him my problems, and he said 'Well, I know nothing about these things but seeing as no-one else has tried what you're trying to do, I'm all for it'. So I came and worked with him and we indeed demonstrated that two human diseases were very strongly correlated with a particular blood group. This collaboration led to something else. One evening I was invited for dinner at the Diamonds and I met their teenage son Jared there. I didn't realise this at the time but I inspired him later on to go to New Guinea and collect birds. Still later, he would be a co-author with me on one of my major books.

BIES: *Does the difference with Dobzhansky about the extent of variation without immediate selective consequence, which you have just mentioned, conflict with the statement you made earlier about there being lots of tolerated variation in populations?*

EM: The reshuffling of genes through recombination results in many genes occurring in very different genotypic environments and having different fitness values in different genotypes. Much of that variation is tolerated.

BIES: *Let us return to matters of speciation and in particular I'm interested in your current views on the importance of* *allopatric vs sympatric speciation. In* Systematics and the Origin of Species *and your 1963 book*, Animal Species in Evolution, *you make a very strong case for the overwhelming importance of allopatric speciation. In your current book* What Evolution Is, *you seem to have more sympathy for the position that sympatric speciation may be more important in certain groups than you had previously given it credit it for.*

EM: Well, I entirely agree that here is an area where I definitely changed my position. First of all, I never denied the possibility of sympatric speciation. I only said, particularly in my arguments with Guy Bush, that none of the cases that have been claimed for sympatric speciation had been particularly well-established. And the thing that particularly bothered me, was that there should be a simultaneous preference among females for a particular host plant and for the particular males that lived on that host plant, considering that there was a continual exchange of individuals between the host plant and the original host of the particular species. Well, it turned out that I was wrong. It has now been clearly shown, for a number of kinds of fishes, not only cichlids but sticklebacks and white fish, as far as I know all fresh water fishes, that the females have a definite preference for the particular part of the environment, the limnetic or the benthic part, and simultaneously for the particular males that occur in this particular environmental niche. That both of these things could be selected at the same time is something that had not occurred to me. But it is now clear. But the question is whether we should now go to the other extreme and say that, well sympatric speciation is the more common form of speciation. But there is a lot that is against this conclusion. First of all, we know a lot about speciation in mammals and apparently all of it is allopatric just like it is in birds. And we know quite a bit about speciation in butterflies and in many cases except some highly host-specific forms, again it is allopatric speciation that is the prevailing form. I once took a group of sea urchins, in order to go way out into an entirely different kind of group of organisms and it turned out that they were clearly speciating strictly by way of allopatric speciation. So, at the present time, there is too little known about speciation in many of the phyla of animals in order to make a generalisation. Now, what pertains in plants is more difficult to determine because most botanists are still reluctant to recognize the biological species concept.

BIES: *I would like to ask you about just that, the biological species concept, the concept that species can be defined as a reproductively isolated interbreeding population. This has been, on the one hand, a tremendously useful idea in evolutionary biology despite the existence of groups of organisms, such as the bacteria, where it doesn't apply. Nevertheless the idea has stirred up more disagreement and controversy than one might expect, such that the way one defines species today remains a controversial subject. Why do you think the biological species concept remains so controversial?*

EM: There is a recent book that came out, supposedly on the phylogenetic species concept. For this book, I was asked to write a chapter on the biological species concept, I was asked to defend it, I was asked to criticize the competing species concepts. Well, what was my final conclusion? My final conclusion was that my original idea and that of a good many naturalists is that there are only two basic species concepts, the typological species concept, in which species are defined on the basis of presumed invariant morphological characters, and the biological species, in which a species is defined as a population or community of interbreeding individuals, a reproductive community. I said that only these two exist. Why are there so many people proposing other species concept? In this book that I just referred to, there are three proposals of a phylogenetic species concept. First of all, it is sort of amusing that they would have three *competing* concepts of a phylogenetic species. But then when you look at them closely, they fail—and this is the reason for all the confusion in the modern literature—these authors fail to make a distinction between the species as a concept and the species as a taxon. Now, the species concept describes the meaning of a species in Nature and there are only two basic concepts, as I said before. But what about all these others? Well, they are descriptions of taxa. Species taxa are what the cladists are interested in and they do not realise that what they are really saying is that the so called definitions of species are [really] criteria by which they separate species taxa from one another. But there is nothing said in their descriptions and definitions about the role of species in Nature and that is the role of a species concept.

BIES: *I would like to ask you two questions about one particular species, namely ours. In the final section of* What Evolution Is, *you talk about human evolution specifically. I would like to ask you if you think that there is a puzzle or a paradox in the fact that seemingly our species is between 100,000 and 200,000 years old, judging from the fossil evidence, yet it's clear that the things that set us apart from the other hominids probably took place within the last 30,000–35,000 years. Do you attribute to this either to the fossil record not mirroring some important aspects of neural evolution, which may have taken place comparatively recently, or do you think that there was a critical cultural breakthrough that took place on the basis of some untapped genetic potential for intelligence?*

EM: The classification of the fossil hominids of the last half million years is still controversial. As far as I'm concerned and that is what I described in my book, there is the lineage that leads from *Homo erectus* through Heidelberg and to eventually Neanderthal. And there is the other lineage, also beginning with *Homo erectus*, leading through African populations eventually to *Homo sapiens*. Now they finally met again about 100,000 years ago and more or less peacefully lived side by side until about 27,000 years ago and then Neanderthal became extinct, quite likely through genocide by *Homo sapiens*. Now the question is: just exactly what happened with *Homo sapiens*? How did *Homo sapiens* evolve? Well, the point is that the difference between Neanderthal and *Homo sapiens* are really, physically, comparatively small. In fact, as far as brain size is concerned, Neanderthal were larger and more robust than *Homo sapiens*. *Homo sapiens* was a rather gracile form, the original ones. In fact, the brain of Neanderthal is actually larger than the average brain size of *Homo sapiens*. It goes well above 1600 cc while the mean brain size of *Homo sapiens* is less than 1500 cc. So, brain size does not really tell you everything. What do we know about *Homo sapiens* and its original properties? Well, extremely little. Both *Homo sapiens* and Neanderthal of course had stone tools—they were somewhat different from each other—and they shared a lot of characteristics and we now know that the later stages of *Homo sapiens*, about 30,000 years ago and more recent, *Homo sapiens* had culture, had highly developed cave art, Chauvet and Lascaux, 30 and 16,000 years ago but we do not really know what *Homo sapiens* was like in the 100,000 years prior to that. So the kind of questions that you can ask about the evolution of *Homo sapiens* is quite limited. There are big arguments among specialists about when the more elaborate language developed among *Homo sapiens* and to what extent the development of certain brain parts were correlated with the development of language. All of this field is so controversial that I personally at the present time do not want to commit myself in any way.

BIES: *Nevertheless, can you describe for us how your ideas about human evolution differ from those that are in the conventional anthropology texts.*

EM: Well I've been rather unhappy about the way human evolution has been treated in the textbooks. First of all, it is very much that every fossil that is found gets a new species name, and there is no cohesion, you do not know who evolved from whom and all the books deal with the fossil finds in Africa as if fossil man had existed only in eastern and southern Africa, and yet the vegetation in other parts of Africa were such that it made it quite possible, and I would even say probable, that hominids also lived in those areas and they are ignored in all the constructions. In my new book I decided to go away from all the text book stuff and propose something that is a little different. I said that the history of hominids could be explained as three different stages. The early stage is that of chimpanzee and that lasted until say 7 million years ago. The chimpanzees lived in the tropical rainforests. The next stage is what I call the australopithecine stage and this descendent of the offspring of the chimpanzee lived in a tree savannah and differed, among other things, from chimpanzees in being bipedal, that is by being able to move on two legs without use of their hands, as the chimpanzees do in their knuckle walk. This australopithecine stage which includes the well-known fossil Lucy and many other of the recent finds is characterised by a small brain. The brain of Australopithecus was about 450cc; roughly the same as that of chimpanzees. They had no stone tools and they were really quite primitive. And in the two million years they existed, the australopithecines showed no particular change, no increase in brain size. And then all of a sudden, a very different kind of hominid appeared, we call it *Homo*. *Homo* is larger than the australopithecines. It had a larger brain (700–900 cc) and it produced stone tools. The question is how and why did *Homo* originate? First of all, there is evidence to indicate that *Homo* originated because an arid period occurred in Africa and the tree savannah was converted into a bush savannah. The australopithecines could no longer escape from their predators into trees and they didn't have the speed of running of a cheetah or lion or any powerful weapons, or the big canines of the lions, leopards etc; in other words, they didn't have the ability to fight their enemies. Most of them apparently didn't survive at all but, in a few places, some managed to survive by their wits, they had none of the natural weapons to make them successful in a battle with carnivores. At that stage something happened which has only recently been properly appreciated, and that is the invention of fire. Fire was important as Wrangham showed the importance of cooking more nutritious food and also having campfires at night meaning the predators didn't dare to come near. So we have perfectly good reasons why in the transition from the tree savannah to the bush savannah suddenly a new type of hominid originated, the genus *Homo*.

BIES: *Just to clarify one point: where do you stand on the out-of-Africa hypothesis, the idea of a single origin of* Homo sapiens *from an originally African population? Do you agree with it or do you favour a modified multi-regional hypothesis in which* Homo sapiens *arose independently from* Homo erectus *in several locations?*

EM: Well, I entirely endorse the out-of-Africa hypothesis. I knew Carleton Coon, I was a good friend of Carleton Coon, who had the other theory, that all over the world *Homo erectus* became more and more similar to *Homo sapiens* and that the Mongoloid race came out of Peking man and that other races came out of other groups, *Pithecanthropus erectus* out of Java man, etc. But this has been thoroughly refuted by anthropologists and the molecuar studies also show that the break between Neanderthal and *Homo sapiens* goes much further back than was originally believed. The molecular evidence indications are that the split between those two lineages goes back several hundred thousand years, maybe as far back as 600,000 years.

BIES: *One of the last subjects you treat in* What Evolution Is *is human consciousness and you give a series of commonly asked questions and then some answers. The last one is where did human consciousness come from and you say that its origins are not a mystery, that it evolved from animal consciousness. One of the intriguing things about animal studies these days is how more and more of the mental attributes that* *we have tended to think about as uniquely human are, in fact, being found in animals, traits such as symbolic thinking. Do you see this as an area in which we are apt to see many more exciting things in the near future?*

EM: I am, of course, very much delighted about this development because it removes that tremendous barrier that some people try to erect between the animals and man. Anybody who knows animals, let's say dog owners, practically all are in agreement that dogs have any number of human characteristics, naturally not as fully developed as in man but the beginnings are there. Everyone who knows dogs, for instance, knows that they have guilt feelings; if they do something bad, then they show it by their behavior, by crawling on the floor and so forth. People who want to have this break between animals and man say that it is something very different, that it is the fear of punishment, that it has nothing to do with consciousness. Well, if you always explain away all the clear-cut signs of continuity between animals and man, well naturally you will have a break but it is your definitions that are responsible for it.

BIES: *I would like to shift gears and talk about the philosophy of science and in particular how the philosophy of biology differs from that in physics. My impression is that you first gave serious attention to this in your writings in* The Growth of Biological Thought *though obviously you had been interested in the history of biology and evolutionary theory much earlier. Can you tell me what prompted this particular interest in how biological science does not fit the models of Science as proposed by people like Karl Popper and Thomas Kuhn?*

EM: The fact that I have a philosophical interest goes way back in my life First of all, I come from an educated family; my father had a library of several thousand books, with several shelves devoted to philosophical books, mostly German of course, authors like Schopenhauer and Kant. And then at the University of Berlin, you could not get a PhD if you had not had philosophy as a minor subject. I had to take courses on Kant, positivism, and the history of philosophy. When I went to New Guinea, and this I myself am a little astonished at, the only two books I took with me were two books on the philosophy of biology, one by Bergson and the other one by Driesch, both of them vitalists. I had the feeling that biology was something different from the physical sciences and I wanted to know more about it and I was very much disappointed when I finally read them carefully that they made out all sorts of claims but they had no real facts. Later on, I was often in academic life in contact with physicists and I was rather annoyed that the physicists usually looked down on me as a biologist. They acted as if there was only one good science and that was physics. And as one physicist published, he said biology is a dirty science, it does not have clear cut laws, it doesn't have exceptionless theories and so forth. As I was studying, particularly Darwin's work, I realised that there were certain aspects of biology for which there was no counterpart in the physical sciences, such as population thinking. And then I realised that everything that happens in a living organism is controlled by two sets of causes. One set of causes are the natural laws that control everything in the physical sciences. And contrary to the vitalists and other earlier so called philosophers of biology, these laws apply to everything. There are no phenomena in the living world that are incompatible with the natural laws that the physicists deal with. However, in biology, everything that happens, any process, is also controlled by a second set of causes, the so-called genetic program, and there is no such thing as a genetic program dealing with any inanimate phenomenon. So I began to realise that biology was far more different from physics than the philosophers usually claimed. And then I realised in due time that every so-called philosopher of science of the last 100 years was, according to his background, a physicist, mathematician, or a logician. Take the whole Vienna school, take Carnap, Noland, Nagel, Hempel, you name them, one after the other, not one of them had a background in biology. And if you go to more modern times, Ruse, who was trained as a mathematician, Hull, who was trained as a philosopher not really as a biologist, Kitcher who was trained as a mathematician, there were no philosophers of science with a background strictly in biology. I realised that what was being peddled as philosophy of biology was really only putting biological phenomena into the straightjacket of a philosophy of physics. And this is the condition right now. I have published a number of books and papers showing how different a philosophy of biology has to be from a philosophy of physics. And I was thunderstruck when earlier this year a Chinese professor of philosophy who is temporarily taking courses at Harvard had written a paper, titled roughly 'Ernst Mayr's Philosophy of Biology'. He has not been brainwashed by the philosophical schools in his country according to which you have to be a physicist, a mathematician or a logician in order to be a philosopher. And he has, out of my writings, put together a philosophy of biology, strictly based on my ideas. Now, I personally never had the nerve to do such a thing but I was delighted that somebody else had done so.

I have, however, recently written a paper on the autonomy of biology in which I show that in very many ways, biology really is a very different science from the physical sciences. Most systems in biology are biopopulations—this doesn't exist in the physical world. In the physical world any electron is always like any other electron and of course the old platonic definition of the archetype fits the physical sciences extremely well in that all seeming variation is not really variation but different trivial aspects of the same essential thing. In biology, where one has, for instance, six billion humans, every one is genetically different from the others and that's the thing that always overwhelms a physicist when you make it really clear to him!

BIES: *I would like to ask you, your opinion on what might be called the limits of reductionism. James Watson is on record as* *having said that, in the end, everything reduces to physics. Would you care to comment on this?*

EM: Well, this question reminds me of an experience I had in 1953 in Copenhagen. I was asked to talk to an interesting club consisting half of professors of the University and the other half eminent citizens of Copenhagen. The subject I was given was the differences between biology and physics. Among other things in my lecture, I said that there was such a phenomenon as 'emergence' which characterised biology and which was non-existent in physics. At that time emergence was a very unpopular concept, it was too much like vitalism and teleology and that sort of thing. At the end, someone stood up, and it turned out to be Neils Bohr, the famous physicist, and he said 'I disagree with what you said about emergence'. And I said to myself, well, of course, as a physicist he would disagree. But then he continued and said you're quite wrong when you say that emergence is restricted to biology, emergence is also very common in physics. Then he quoted the traditional example of water that T.H. Huxley had given in the 1880s. He said that if you break down water into hydrogen gas and oxygen gas, you have reduced it to the simplest components but you still don't understand what water is. And this is what emergence is all about; it deals with the interaction of these smallest parts. The main reason for much of the argument in this field is that people don't make a distinction between reduction and analysis. Of course, analysis is something that is as necessary in biology as it is in physics, but the reductionist says that once you have broken things down to the smallest parts, you have all the answers, while the holist, the one who opposes reductionism, says no, once you have the smallest parts the next thing is that you have to know how they interact with each other. Only that knowledge gives you the complete knowledge of the system. So, the important thing is to make the distinction between analysis, which is always necessary, and reduction which says that once you have the little pieces then you have everything.

BIES: *You have also commented in some of your recent books on the fact that in biology today there's a tremendous emphasis on discovery of new facts, and that synthesis and conceptualising is often relegated to a second place as a less important activity. Would you comment on this?*

EM: Yes, discovery of new facts is of course very important, but the trouble is that some people think discovery is *all* that we need. It's quite interesting that if you read the regulations about Nobel prizes, it is always that a Nobel prize is given for a discovery in this field or that field. If there was a Nobel prize for biology—and there isn't—and Darwin was still alive, he couldn't have got the Nobel Prize because he didn't make discoveries in that sense, he contributed to our understanding of things by developing new concepts and that is not mentioned in the rules for awarding the Nobel prize. I feel that much of the progress that we have made in biology in the last fifty years is due to the development of new concepts. In fact, I'd go so far as to say that theories in biology are based on concepts, while theories in the physical sciences are based on natural laws. But there are no natural laws on which to base most of the theories of biology. Look at natural selection, take all these major terms from behavioural biology, ecology, evolutionary biology; they are all concepts and not natural laws. Yet, the theories we have are based on these concepts and the importance of these concepts which is so obvious to me but which is often remarkably ignored by the majority of people writing about such matters.

BIES: *A larger problem is that in many countries there is a kind of large-scale resistance to acceptance of basic evolutionary ideas in the face of what is overwhelming evidence for the existence of evolution. One sees this in the United States for instance; in the fact that apparently 50% of people polled say they do not believe in evolution. In your book*, The Evolutionary Synthesis *(1982) edited with William Provine, you have various chapters on how the evolutionary synthesis was received or rejected in different countries during the period of its formulation. Clearly, national cultures and religious beliefs can affect the way scientific ideas are received. Are you optimistic that in the long-run evolutionary ideas will triumph or do you feel that there will be continued resistance due to these interesting cultural factors that affect their reception?*

EM: Well frankly speaking, and I'm sorry to say this, I'm not very optimistic because the basic outlines of evolutionary theory have been well established for more than fifty years and have been taught, both in colleges and even in high schools, and yet the students hear it and then they go on and say what they've learnt in church and in religious education as if science didn't exist and if the truth of scientific findings could be easily questioned. So I think as long as we have non-thinking people, there is a chance that almost anything will be believed except what science has established as the real truth.

BIES: *May I ask a personal question? You were born in 1904 into a prosperous, upper middle class German household. Was religion important in your family? Did you at some point give up religious ideas as a result of your scientific learning, or were you never strongly religious?*

EM: I rather suppose that both of my parents were agnostics although my mother would go to church very often on a Sunday to hear the sermon because she thought that the minister gave such interesting sermons, but I went to the regular school curriculum of religious education and we read the Bible and religious texts. It wasn't the findings of science that made me turn away from religion eventually, but simply what I considered the implausibility of all the things that Christianity preached like resurrection of the dead and heaven, miracles, all this sort of thing. These were all so impossible for me to believe that I said I cannot possibly adopt such a religion that asks me to believe such impossible things.

BIES: *Let me ask you a question about the future of evolution of our species. Does it seem to you that now that we* *are effectively a panmictic population of six billion people, that the biological evolution of our species is probably effectively at an end?*

EM: Yes, there's no indication that we have any selection in the modern human species that would lead to an improvement of the human species, or as it's usefully called in the literature, the development of superman. On the contrary, there may well be a deterioration, and the only selection that we have is that individuals which are grossly deficient in something or other will not be able to reproduce themselves but there is no selection in favour of improvement of the species.

BIES: *You have spoken recently, in publications and earlier in this interview, about what an exciting field evolutionary developmental biology is. One of the findings that you discuss in* What Evolution Is *is the intriguing finding that the gene* Pax-6 *is used for eye development in many different kinds of animals. You seem to reject the idea that there could have been an ancestral* Pax-6 *employment for eye development because the eyes of the animals that use it are so very different: from the multifaceted eye of* Drosophila *to the camera eye of cephalopods and vertebrates. You propose that there were probably several independent co-options of* Pax-6 *from a more general ancestral anterior neural function. I think many biologists would find this idea of independent* Pax-6 *co-option for a specific biological function somewhat implausible. Would you comment on this please?*

EM: Well, to begin with about thirty years ago, together with an Austrian morphologist [L. V. Salwini-Plawen], we found that eyes had evolved at least forty times independently throughout the animal kingdom. Well, after a while, a prominent molecular biologist [Walter Gehring] found that in each case, the eye was found together with a regulatory gene, *Pax-6*, and he came to the thesis that *Pax-6* was the explanation of the origin of new eyes, and that these 40 supposedly independent origins were really one origin of the original *Pax-6* gene that had later split in different lineages into different kinds of eyes. The objection to that interpretation is two-fold: first of all, the *Pax-6* gene is also found in quite a few invertebrates that have no eyes. One cannot say that they lost eyes because they are usually rather primitive forms which clearly had never had an eye and furthermore, why should an eye which is such an important structure ever be lost and why should it be lost in a good many lineages of all these primitive organisms. The second reason is that of course the origin of eyes can be traced from the origin of a light-sensitive spot on the epidermis and the gradual accumulation of pigment around the spot and then the thickening of the epidermis eventually culminating in the production of a lens. So where does *Pax-6* enter the picture and why is it needed to organise the production of this sequence of development? Even the author of the paper who originally questioned the independent origin of eyes has I believe now taken it all back and admits that *Pax-6* must have originally had some quite different usage and was only later co-opted to the evolution of eyes.

BIES: *You are sceptical that eyes can be lost in evolution but isn't that exactly what has happened in many species that have evolved in caves?*

EM: But there is no common environmental cause why so many primitive marine invertebrates have lost their eyes. An absence of eyes is the standard situation among small primitive invertebrates. Photo-receptive organs are subsequently acquired.

BIES: *Can you tell us your views on SETI, the search for extra-terrestrial intelligence?*

EM: Well the joke in my group is why search for extra-terrestrial intelligence, why not search for intelligence on earth? If you follow what the people do who are searching for extra-terrestrial intelligence you certainly wonder whether this question of searching for intelligence on earth isn't fully justified! Let me be serious now, somewhere there are people, and I must say it completely puzzles me, who say they feel alone if we on earth were the only living human-like individuals, and they would like to have contact with some other thinking individuals somewhere else. Well, the question is what chance is there of such a search being successful? Now the trouble with much of the past discussion on this subject was that no distinction was made between searching for life outside the earth and searching for high intelligent civilisations outside the earth. Now the first one, searching for life outside the earth I would not consider totally hopeless because the first life even on earth, although we don't know exactly what is was but it consisted apparently of rather simple molecules which could easily be produced by the chemicals available in the Universe and with the sun's energy present as enough source of energy to take care of that part of it. So if the question is simply is there a chance of life somewhere else, then the answer is yes, but that life would be something even more primitive than bacteria and that of course basically isn't very interesting to people. They want to find the kind of life-forms they can communicate with, and that leads to the second question, what chance is there that there are intelligent civilisations elsewhere. There the blunt answer is that there is no chance because even on earth as some people, for instance [Stephen Jay] Gould, have correctly pointed out, the fact that intelligent life eventually evolved was really a very improbable chance. We can take all sorts of measures, such as how many species have ever existed on earth? Well the answer is probably about 5 billion species, well how many of those had high intelligence? Well, it's one, so the chance of finding high intelligence is one in 5 billion, which is not a very encouraging figure. There are other ways of asking the same question. Let's say we have intelligence, and let's assume we have civilisation. So on earth, how many civilisations did mankind produce so far? Well, the answer is maybe about 20, including 3 in the Americas and the old ones in the near East, civilisations in India and in

China and the European civilisations. Let's say 20, and of these 20 civilisations, how many of them produced an electronic system that would permit exchanging signals with other places where there was intelligent life? Again the chance of this happening is virtually nil, and so any measure you take of possible existence of electronic civilisation leads to the answer the chance is virtually nil and if you multiply all the different measures that you take the final total is an improbability of absolutely astronomical dimensions and so I personally consider it a pure waste of time and money to wait for signals from other civilisations elsewhere in the Universe.

BIES: *Let me ask you a question about your intellectual legacy and that of the co-founders of the modern synthesis, the men and women who made the evolutionary synthesis in the 1930's, 1940's and 1950's. Many of those insights have become such an intrinsic part of biological thinking that many younger scientists in particular would have trouble distinguishing who contributed what. Does this in any sense bother you that your own contributions and legacy have become absorbed in the general wisdom or general knowledge?*

EM: Well it doesn't really bother me but I would like to be mentioned once in a while for things where I was the first one who developed it, first proposed it. The literature is full of references to people who proposed wrong theories, and the wrong theories continue to be refuted year after year as if this was something very important while the correct theories are just quietly incorporated into the general knowledge.

BIES: *When you turned 90, you are alleged to have said that you had 5 more good books left in you.* What Evolution Is *was one. Can you describe to me the other books that you have written and the ones that you are working on now?*

EM: Well you see a few years earlier I wrote a book called *This is Biology* and part of the major message of that book was that biology really is something very different from the physical sciences and many aspects of biology are still not yet understood by people who come to biology via mathematics or logic or the physical sciences. That is all rather clearly stated in that book although it will have to be repeated and stated again and again.

BIES: *And the other books that you've written? One of them is* The Birds of Northern Melanesia *which you have just done with Jared Diamond.*

EM: Well, *The Birds of Northern Melanesia* has an interesting history. When I came back from Northern Melanesis in 1930, I knew that I had to describe the new birds that I'd found and classify them properly and then I decided that I would write a general volume about the meaning of all these findings. This, mind you, was in the 1930s. Well, every once in a while I said that I must write this book and then another major project intervened and I didn't. Finally in the 1960s, I started working on it and within the 1960s I got the idea that Jared Diamond might be a good co-author because he was more interested in the ecological aspects of the bird fauna and I was more interested in the speciational aspects. So we decided to work together and exchanged manuscripts in the 1970s and then of course he got side-tracked in several book projects and so did I, and then finally in the 1980s and early '90s, we put on full steam and wrote up the book. The reason the book took such a long time is that it is an incredibly detailed and complete analysis. In fact sometimes I wonder whether this book is going to scare people away from the subject because they will say 'Oh I can't do anything anywhere near as complete as Mayr and Diamond have done'. There are 195 species of birds in the area. For each species not only do we describe what happened in this area but also the history of how it arrived in the area and the changes that happened to its ecology and the relationship between the location of the islands and the speciation phenomena and the relation between the size of the islands and the speciation pattern in relation to size and so on. All these relations are analysed in detail. There are 20 endemic mountain species and every one of these is considered in the analysis. In many ways it's a unique volume, there's really nothing like it in the literature and Diamond and I really hope that it will stimulate other people to think, well this is birds, how about this in ants, how about this in butterflies or land snails or in plants. Once we have produced this model system, it should be compared with other groups of organisms to see whether the generalisations we made are true also for other organisms or whether they have their own regularities and laws.

BIES: *And what are the books that you are working on now?*

EM: One is an essay volume largely of previously published papers on evolution, theory of systematics and the philosophy of biology. The other is an extremely detailed commentary on the first edition of Darwin's *Origin*. I have already drafted one half of this volume.

BIES: *For most of the past three decades at least, certain areas of biology have been quite eclipsed by the successes and advances in molecular biology. Systematics is one of those areas and yet it clearly remains a field of intense interest for conservation and the ways that we understand and think about our world. Do you have any sense that systematics is rising again to greater recognition among younger scientists as an area worth working in?*

EM: Well, as I've said, at the present time in the field of systematics we are in the midst of a great controversy between the classical, traditional Darwinian classifiers and the cladists. A major paper of mine together with Professor Walter Bock of Columbia University is just ready to go to the publisher. This will be the beginning of a long controversy and I think it will be interesting and exciting to young people in the field and I do think that with the realisation that the destruction of the forests leads to a tremendous amount of extinction that more and more people will be attracted to the diversity of organisms because it's an area where there is still so much to be done.

BIES: *Have you ever been tempted to write your autobiography?*

EM: Having had this interesting life in New Guinea and then this whole set of other interesting things in my life, everybody says I should write my autobiography. However I have always been more interested in something else that I was writing at the time, and I have a very factual style and so fear that any autobiography would lack the charm and personality of EO Wilsons delightful autobiography. Personally I also have a very genuinely old-fashioned view-point that one shouldn't blow one's own horn. Having had such an interesting life, I couldn't very well talk about myself without bragging. This I consider rather unpleasant—I mean who else has had a private dinner with the Emperor and Empress of Japan? Who else has had honorary degrees from Oxford, Cambridge, Harvard, the Sorbonne, Uppsala, Berlin, and so on and so forth? Who else has been able to celebrate the 75^{th} anniversary of his PhD? (The University of Berlin gave me a special certificate certifying that this is indeed the case.) So if I wrote my autobiography, it would have been bragging the whole way through and that didn't appeal to me.

BIES: *Still, it would be a real loss if the full story of your life weren't told. Do you know if anyone else is planning to write your biography?*

EM: Actually, two biologists in Germany are in the midst of working on my biography. I send them dictated texts about any aspect of my life of which they want further details. So far no publisher has been selected.

BIES: *Dr Mayr, this has certainly been a wide-ranging interview. Temporally, we have covered the last 3.5 billion years of life on Earth and spatially, from New Guinea to the furthest reaches of outer space. It has been a great pleasure talking with you. Thank you very much.*