The Evolution of Ernst: Interview with Ernst Mayr

The preeminent biologist, who just turned 100, reflects on his prolific career and the history, philosophy and future of his field

On July 5, renowned evolutionary biologist Ernst Mayr celebrated his 100th birthday. He also recently finished writing his 25th book, *What Makes Biology Unique?: Considerations on the Autonomy of a Scientific Discipline* [Cambridge University Press, in press]. A symposium in Mayr's honor was held at Harvard University on May 10. *Scientific American* editor and columnist Steve Mirsky, who was also at the time a Knight Science Journalism Fellow at MIT, attended the symposium and wrote about it for the upcoming August issue. On May 15, Mirsky, his fellow Knight Fellow Claudio Angelo, a Brazilian journalist, and Angelo's colleague Marcelo Leite visited Mayr at his apartment in Bedford, Mass. Leite presented Mayr with a book featuring various interviews published in the Brazilian newspaper *Folha de S.Paolo,* including ones with Mayr and Harvard biologist Edward O. Wilson. A transcript of their conversation follows below.

Ernst Mayr: Really, thank you very much. They are rather contrasting interviews. I am very much of a realist. And Wilson is an extreme optimist.

Claudio Angelo: When you say you're a realist and Wilson is an optimist, is it regarding the fate of the planet or our own fate as a species?

EM: In all sorts of ways. I'll give you one example. You know that there some astronomers who are looking for intelligent life on other planets. And there are two groups of people: the ones who believe that this will be successful and the ones who are quite sure that this won't be successful. Well, most scientists and particularly biologists are totally convinced it will not be successful. One of the few exceptions is Wilson. He's a good friend of mine. One day I said to Ed, "How can you support spending money for this search when it is so totally impossible that there will be intelligent life?" He smiled and said, "Oh, I know that, I realize that this is totally improbable." And then came another another smile on his face: "Wouldn't it be nice if we did get some messages?" That's what I call an extreme optimist.

Steve Mirsky: Are you convinced there's intelligent life on earth?

EM: That's what I always say. If we would spend that same amount of money to search for intelligent life on earth it would be a much better project.

CA: So we heard you are working on your 26th book.

EM: No, it's not the 26th, it's the 25th. And I'm not working on it anymore, I already have read the page proofs. It will take, I don't know the publishing business, how they spend their time, but the person at Cambridge University Press who is ushering it through the publication process, he told me it won't come out until July or August, most likely August.

CA: And what is the book about?

EM: What the book is about. (Laughs.) Primarily to show, and you will think that this doesn't need showing, but lots of people would disagree with you. To show that biology is an autonomous science and should not be mixed up with physics. That's my message. And I show it in about 12 chapters. And, as another fact, when people ask me what is really your field, and 50 years or 60 years ago, without hesitation I would have said I'm an ornithologist. Forty years ago I would have said, I'm an evolutionist. And a little later I would still say I'm an evolutionist, but I would also say I'm an historian of biology. And the last 20 years, I love to answer, I'm a philosopher of biology. And, as a matter of fact, and that is perhaps something I can brag about, I have gotten honorary degrees for my work in ornithology from two universities, in evolution, in systematics, in history of biology and in philosophy of biology. Two honorary degrees from philosophy departments.

SM: And the philosophical basis for physics versus biology is what you examine in the book?

EM: I show first in the first chapter and in some chapters that follow later on, I show that biology is as serious, honest, legitimate a science as the physical sciences. All the occult stuff that used to be mixed in with philosophy of biology, like vitalism and teleology—Kant after all, when he wanted to describe biology, he put it all on teleology, just to give an example—all this sort of funny business I show is out. Biology has exactly the same hard-nosed basis as the physical sciences, consisting of the natural laws. The natural laws apply to biology just as much as they do to the physical sciences. But the people who compare the two, or who, like some philosophers, put in biology with physical sciences, they leave out a lot of things. And the minute you include those, you can see clearly that biology is not the same sort of thing as the physical sciences. And I cannot give a long lecture now on that subject, that's what the book is for.

But I give you just two examples. One is the bio-population. Bio-population is just something that doesn't exist in the physical sciences, and yet it is the basis of almost all concept formations in biology. And in particular in philosophy of biology. And the second thing that, quite in principle, biology differs from the physical sciences is that in the physical sciences, all theories, I don't know exceptions so I think it's probably a safe statement, all theories are based somehow or other on natural laws. In biology, as several other people have shown, and I totally agree with them, there are no natural laws in biology corresponding to the natural laws of the physical sciences.

Now then you can say, how can you have theories in biology if you don't have laws on which to base them? Well, in biology your theories are based on something else. They're based on concepts. Like the concept of natural selection forms the basis of, practically the most important basis of, evolutionary biology. You go to ecology and you get concepts like competition or resources, ecology is just full of concepts. And those concepts are the basis of all the theories in ecology. Not the physical laws, they're not the basis. They are of course ultimately the basis, but not directly, of ecology. And so on and so forth. And so that's what I do in this book. I show that the theoretical basis, you might call it, or I prefer to call it the philosophy of biology, has a totally different basis than the theories of physics.

If I say so myself, I think this is going to be an important book. The philosophers of course will ignore it, it's bothersome, it doesn't fit into their thinking. And so the best way is to just forget it, put it under the rug. But those who take it seriously will say, well, gee, that's not something I know how to deal with. But this fellow Mayr seems to have something here, nobody else has made that so clear, nobody else has shown that, really, biology, even though it has all the other legitimate properties of a science, still is not a science like the physical sciences. And somehow or other, the somewhat more enlightened philosophers will say we really ought to deal with that. But so far they haven't.

SM: So would you say that before Darwin—you have a period after Newton but before Darwin—in that period, physics is a science that's different from biology?

EM: Absolutely. You have a marvelous historical document that illuminates that. Kant, after he had shown in the *Critique of Pure Reason* how in the physical sciences everything is based on natural laws, that was supposedly Kant's great contribution, and then he went on, in 1790, to show that biology is no exception, that it's also based all on natural laws. He describes [this] somewhere in the early chapters of the *Critique of Judgment.* And he tried to base the generalizations, let's call them laws, of biology, on natural laws, and it just didn't work. It was a complete disaster. And so finally, he said, you have to base them on something else. Well, what? And he said teleology. Aristotle's fourth cause, finality. Everybody has been trying to show how Kant had the right instinct to get away from the natural laws for biology and adopt instead teleology. Well, one of the chapters in this forthcoming book of mine is devoted to showing that this doesn't work. There is no such obscure force in nature like teleology, like Aristotle's fourth cause.

CA: So, would you say that the whole quest of molecular biology to try to ascribe everything to chemical bonds and physical laws is the same mistake that Kant made?

EM: Well, I now will jump ahead to what probably would have come out gradually. When did biology originate? Well, not in the 17th or 18th century. You had fields of biological activity, like anatomy and taxonomy and other things like that. But you didn't have a field like biology. Now the word biology, curiously enough, was proposed three times around the year 1800 by three different authors. My claim, which I make in earlier books, is that biology as a field, something that you can recognize as something different from the physical sciences, that you can really designate with a single word, developed and originated and became what it is, biology, in a relatively short number of years. Around 40 years, between 1828, when Karl Ernst von Bayer organized developmental biology, embryology, and then came very soon after that, the two authors of cytology, Schwann and Schleiden, who caused a tremendous uproar at the time when they published their work in the 1830s, because it showed that animals and plants are composed of the same elements, cells. So that was a major contribution toward the science of biology. And then comes a big period of physiology, Claude Bernard in France and in Germany they had two or three authors that were the great physiologists, Johannes Miller was one. So that's the third field.

And then genetics, of course. Genetics was the last one. The next one in time that developed, of course, was Darwin's and Wallace's evolutionary biology. And finally, in 1865-66, genetics. Now this series of six sciences beginning with embryology and ending with genetics was the founding of biology. And you can really argue about what is biology only after you have nailed that down, because this compound of things, including genetics, evolution and so forth, is biology.

Now, you asked what about molecular biology. Well, let me now again sort of go a step or two back. There was a very crucial period in the early part of the last century during which the so-called evolutionary synthesis took place. And up to that time, meaning the period between 1859 and the evolutionary synthesis, which was in the 1940s, there was a great turmoil in evolutionary biology. There were at least four if not five major basic theories of evolution, for instance. But anyhow, the evolutionary synthesis, initiated by Dobzhansky and then joined by people like myself and Julian Huxley and Simpson and Stebbins and so forth, the evolutionary synthesis sort of put a stop to the major theorizing, particularly in the evolutionary field. And what is very interesting, then you have Avery showing that nucleic acids rather than proteins are the genetic, evolutionary material. And then came Watson-Crick. And then came all the developments in molecular biology and finally the developments in genomics. And each time one of these major upheavals occurred, we expected the theory of the evolutionary synthesis to have to be rewritten. But the fact is, and I don't know whether any molecular biologist has complained about it, or expressed regrets, that none of these major upheavals in the factual structure of this new biology from Avery to genomics, none of these changes really affected what is usually referred to as the Darwinian paradigm, the set of theories that make up modern Darwinism, from let's say the 1950s, let's say from Watson-Crick to today. And new books come out all the time in which the author tries to prove that Darwinism is invalid. Well, I think even if you're a neutral outsider, you will admit that none of these books has been a success. And in the end, it has always been showed that Darwinism was and is correct.

But this is now finally the answer to your question. The funny part is that molecular biology has a remarkably small impact on the theory structure of biology. At least that's the way it looks to me. Of course, they can point out that the genetic code has shown that life as it now exists on the earth could have originated only a single time, otherwise it wouldn't be the same code that it is. And of course there are several other things that molecular biologists have contributed. But none of them really touched the theory structure of the Darwinian paradigm, in my opinion.

SM: If anything, hasn't it been the opposite, that the synthesis informs the molecular biology work.

EM: Right, yes. That's molecular biology's theory structure.

Marcelo Leite: But on the other hand, molecular biology is seen by molecular biologists and also by the public as the kind of defining moment of biology in the 20th century. They kind of reconstruct the whole history of biology as if pointing to molecular biology and the human genome project as the climax of this. So this is a wrong way of seeing 20th-century biology?

EM: There is no doubt. If you go further back, the molecular biologists take everything else that happened for granted. On the other hand, if you're a cytologist, you could say that the Schwann-Schleiden demonstration that all organisms consist of cells, and that the cells have a nucleus and all this sort of thing, that all is as much a foundation of biology as, let us say, that nucleic acids consist of base pairs. I don't see anything more, in fact, I would say that from the point of view of philosophy, these findings, these descriptive findings of molecular biology, are no more important than these, let's say, achievements from the origins of biology in the period from 1828 to 1866. Those findings made in that period are at least as important as anything in molecular biology.

They [molecular biology's findings] are very important, I'm not running it down. But I'll give you another historical example that's very interesting. Let's say, in the 1950s, right after Watson and Crick, a lot of chemists, biochemists and physicists, went away from their physical sciences into biology. And they were very often making the appropriate amount of noise, they were very successful in being considered great innovators in science. And very often they became the chairmen of biology departments, and there are at least three cases known and probably more if you would look at enough colleges and universities, where a biochemist or a straight chemist usually, became the head of a biology department. And he just got rid of every organismic biologist. He said we don't need them, they're not biology, biology is molecules.

ML: Well, James Watson is still writing this.

EM: No, James Watson is broader. If you carefully read what he says, yes he emphasizes the importance of his and his school's things, but Watson has enough of an organismic biology background. Now I can tell you a true anecdote about that. Did you know that Jim Watson was an ardent birdwatcher? When he was, I assume it was the senior class of high school, his mother came with Jim from Chicago, and his mother asked me what college Jim Watson should go to to get his PhD in ornithology. Listen to that carefully, I can swear to it, this is the honest truth. I've reminded Jim of this and he's tried to be quite forgetful about it. So I said to Jim, or rather to his mother, he doesn't want to go to the best ornithology college. What he needs is to get a good education in biology. Let him go to the best school in biology. Because this special knowledge that he needs as an ornithologist he can always acquire eventually. But what he needs in order to be successful in any special branch of biology is a sound foundation in biology.

And so they took my word and probably other people told them the same thing. And he didn't go to any place for ornithology, but he went to Chicago and various other places and got a very good thorough training in biology. So as a joke, and please remember this is a joke, I sometimes say, well, how did Watson ever get a Nobel Prize? Well, I'm the one who's responsible for that!

ML: But he then decided that this was not the kind of biology he wanted to do, but really the life is in molecules and chemistry.

EM: Absolutely.

ML: Some critics in Brazil of molecular biology claim that this field has transformed biology into a kind of industrial enterprise and driven biology away from hypothesis-driven science. Do you agree that this is happening?

EM: To some extent this is indeed happening. I mean, you find that all you have to do, for instance, look at the list of people who get annually elected in biology to the National Academy of Sciences or the American Philosophical Society or the American Academy, and so forth. And that's a complaint in these institutions. The people who get elected there and get prizes, practically all of them are molecular biologists. Molecular biology has the sort of glamour that leads to election to societies and to prizes and things like that. And every once in a while there's a development in organismic biology that suddenly makes organismic biology at least temporarily very attractive. For instance, when it was discovered that molecules are very good clues as to phylogeny, as to ancestry of organisms, and which ones are related to each other and derive from one original, all of a sudden just scores of molecular biologists, molecular physical biologists and so on, suddenly worked on phylogeny of organisms, which is a very organismic branch of biology. And they were proud that they were able to show that whales are derived from artiodactyl ungulates.

And lots of, if you really look at what a lot of molecular biologists are doing, well, they're really doing aspects of organismic biology, not straightforward organismic biology, but ultimately leading to results in organismic biology. And I don't, you see, this is something important to record, because it's often misrepresented. You get a lot of authors, and I will not mention their names, in philosophy of biology, who for instance would say Ernst Mayr was opposed to molecular biology. I never was opposed to molecular biology. And I got several prizes in molecular biology by the people who really knew that I was not opposed. I was the speaker on a number of molecular symposia. And all I did was, I said that molecular biology is not the only kind of biology. George Wald published once a paper in which the final punchline was that there was only one biology and that was molecular biology. Well, that's nonsense. There are lots of biology.

Well, of course, you never know what results organismic biology may produce. Right now I know of some research done by strictly organismic biologists that indicate that it might be possible to breed, by partly molecular and partly organismic methods, strains of cereals, like wheat and so forth, that can be planted on salt-saturated soils. And that is a product of not molecular activities, but organismic ones. By dealing with such typical physiological phenomena like salt tolerance. And so every once in a while, organismic biology is using molecular research in order to produce results that are basically organismic biology.

SM: I remember attending a talk five or six years ago and a researcher was discussing a question about whether two populations were really the same species or were two different species. And then they did genetic analysis and determined that they were in fact two different species, just from the genetic analysis. And I remember being concerned about that kind of approach, the reliance on DNA as the end of the discussion struck me as being dangerous.

EM: It depends on the particular case. In some cases a rather drastic molecular difference is not an indication that these are two different species. But in other cases it will be. So you always have to have the organismic background knowledge in order really to come out with the right conclusions.

SM: Is there a set of rules that govern when a big genetic difference will be indicative of species difference and when it won't.

EM: No, it might be one gene. On the other hand, well, you see, not that this really should get into this thing, but for instance, we have these two schools of evolutionists, the ones who are gene centered and the ones who are organism centered. There was a great belief in single genes. And you go to the definition of evolution, it was change through generations of gene frequencies. Well, no sound geneticist would say that anymore. And at that time there was strict opposition between the so called population geneticists, who were really gene geneticists, who said that the gene was target of selection, and the naturalists, let's call them, who said, no, it's the individual, and the gene is only the way in which the individual might be selected, that it may play a role.

This was the status in 1930. Every geneticist you asked in 1930 would have said the gene is the target of selection. And it was shown in case after case that it depended often on the context of other genes. And therefore a single gene always occurs in the context of a genotype, and the phenotype that is produced by the genotype. And that is indicated to some extent by Dobzhansky in 1937, but not really emphasized. But then came a whole series of authors. Michael Lerner was very important. And I stressed it very much in a number of publications and books, always showing that it was the combination of genes. And then, in the 1970s, a paper by Dick Lewontin came out showing how the single gene couldn't [be the target of evolution], and then in [1982] there was finally the decisive thing, there was a paper by Lewontin and Elliott Sober the philosopher, definitely emphasizing that the single gene [was not the target]. So that gene-centered development that went from let's say 1924 to [1982], it took 60 years for that gene-centered thing. And there are still some authors even today, like Dawkins, who are still gene centered.

SM: But even Dawkins, in *The Selfish Gene,* he gives the analogy of the members of a crew team, and the gene has to be in the right context in terms of where it would be on the boat with the other rowers.

EM: I have a marvelous quotation by Dawkins, where, in a single sentence he admits it is not the gene. He knows it from that point on. It will be in my forthcoming book.

CA: How would you explain those intragenomic conflicts, such as genomic imprinting, if…

EM: I'm going to make a confession now. I've never been able to quite understand imprinting. I have great trouble with imprinting. The genetic imprinting. The behavioral imprinting, there's no problem. But in a way it's a mistake to use the same word for two things that are so different as this genomic imprinting vs. behavior imprinting. We have a great specialist of imprinting here at Harvard, that's David Haig. I've always told myself that I should really go and have a meeting with Haig to tell me what imprinting is. But I'm quite sure that he will be so technical in his explanation that when I'm all through with this meeting I still won't understand.

SM: If you were about to start your graduate school career today, what would you study?

EM: Well, you see, I have shifted so much in the course of my life that I really don't know, and of course part of my choices were a result of what preceded. Now, in a way I'm sometimes surprised at how advanced I was in my 1942 book *Systematics and the Origin of Species.* That was quite a bit ahead of its time. I had no teacher who was that much ahead. How I could see things in such a modern way I still don't understand. But I did.

SM: What do you think the major questions, or even a single question, for a young researcher today is. Where would you point that person?

EM: Well, you know, the genotype. I'll mention something that nobody ever mentions. Let's say you have now a genotype that makes a certain protein. And that protein, and you can see this in every issue of *Science* practically, that protein is a very complex structure, incredibly complex. Now how that step from a group of amino acids to that polypeptide [happens] is an enormous jump. I think everybody leaves it alone because nobody yet has figured out just exactly how to attack this problem.

SM: Are you referring specifically to the protein folding issue or a more overall issue?

EM: The protein folding, yes, that's really probably the most important part. But there are some other things involved and the people who work in this field, and this question that you mentioned, they would say that the folding is just one thing.

ML: The whole thing is so complicated because there are the helper molecules, the chaperones and other things.

EM: All sorts of things, and how they all can work together and what part of this making of the protein is the folding. Even that is a question that isn't really very clear. You see, you shouldn't ask a 100-year-old gent the last burning questions of molecular biology.

CA: What about field work and the work that naturalists do and the work that you began your career doing. Do you think there's still space for people going out there and doing field work?

EM: Oh yes.

Let me now go back to something halfway. And that is, just think that 80 or 90 percent of most base pairs are not coding. Now having so much of the genotype consist of non-coding genes does bother most molecular biologists. And they wave it away by saying, oh, we're going to solve it. The truth is that they're really worried why they haven't yet been able to solve it. Now, you see, as a good Darwinian evolutionist, it's part of my religion I might say, that nothing happens in evolution without being authorized, is maybe the key word, by natural selection. And yet, if we study an evolutionary process, there are all sorts of things going on that seem to be quite opposed to the concept of natural selection. And in my last book on evolution, *What Evolution Is,* I say all sorts of things about natural selection that are not at that level that I just talked about, but that are at the classical level of natural selection. Still, very different from the classic answer. For instance, most people don't realize that the most important step in natural selection usually is the elimination of inferior genes, it's not the selection of the best. And this elimination of inferior genes of course is far less rigorous than the selection of the best. Well, what is inferior? Loads and loads of genes, this is getting clearer all the time, that are not good enough to be selected for, but also are not bad enough to be selected against. And the gene pool that occurs in a normal population has far more variation than is usually considered by the evolutionist. He concentrates on the best genes.

And the result is, let me now talk of this from a different angle. Let's take the peacock. The peacock has this enormous tail, it's not really the tail, but we call it the tail. And if you saw a peacock you'd say, this is the product of natural selection. And then ask, well, how can selection select such a monster of a bird with that clumsy tail that would make it the immediate victim of every predator. So, the answer is, the study of natural selection, this is not molecular, the study of natural selection is really rather primitive as far as I'm concerned, because it really doesn't include all the aspects of variation.

For instance, I'm quite convinced that the people that study early animal evolution, like in the Precambrian and Cambrian and so forth, and some people like Steve Gould, I think he had the right instinct there. One of the amazing things about the Precambrian and the Cambrian is that all these incredible types of animals were produced and became very quickly extinct again. How could natural selection ever have produced such incredibly complex and improbable organisms. And Gould, I have to go over this again once more, I think this is the part of Gould's work that is really the most important. He emphasized that, he said let's not exaggerate, he didn't say that, but he could have said that. Let's not follow Darwin and say that natural selection scrutinizes every moment, every day, every moment, everything, and always selects the best. Well, Gould said, nothing of the sort. From generation to generation, always, lots of genotypes get through to the next generation that were maybe not even midway to goodness, but even below that. And yet made it to the next generation. And that that explains a lot of the types of things that were permitted by natural selection. Particularly the early period of the existence of animals and plants. And I think in this area, and I'm just finding this out, that in natural selection we have a case of a very classical principle, very classical subject, and yet there is still an awful lot of thinking to be done.

I am supposed to write an article in *Science* in connection with my hundredth birthday, a few words about my career and so forth. And I wound up in the last sentence saying that when I look at all this sort of thing that I just described I can preach courage to the young evolutionist that the work of the evolutionist is by no means completed, the world is still full of unsolved questions and perhaps more importantly unasked questions. And this just occurred to me this moment, it is not in my manuscript: I think the young evolutionists never have to worry—if they have enough imagination they will always come up with some really interesting problems that the present generation of evolutionists has not solved.

CA: You think those answers, they should get them from zoology and botanics, from paleontology or from laboratory benchwork?

EM: From everywhere! You see, it's interesting how certain branches of science lag behind because something is missing. Now, this is something that's interesting and you have to record it because it's always misrepresented. Take developmental biology. In the 1930s, when Spemann's theories were pretty well refuted—

SM: That's the organizing principle idea?

EM: Yes. Much of that just simply didn't work. And the experimental biologists sort of gave up on developmental biology, and virtually nothing of consequence was done on developmental biology from the 1930's to the 1970s, 1980s, thereabouts, when evo-devo came up again and so forth. And they required the techniques of genomics in order to solve these unsolved questions. They didn't have those theories. And that's an important thing, you see. Now, for instance, if you look at the writings of Fischer, Haldane, Sewall Wright and their series of famous papers from 1930 to 1932, they give a very good account of evolutionary change, how in a population under the influence of natural selection things happened in improving or maintaining adaptation. However, in all their work you won't find a word about the origin of biodiversity. The whole field of species, speciation and macro-evolution, all this sort of thing, is just absent from 1930, 1931, 1932 literature. Which, many people who came from the outside, including biochemists, thought that this was the end of evolutionary biology. "Fischer and Haldane, they have solved it all." Nothing of the sort!

The whole field of biodiversity was ignored by them and it was not until Dobzhansky, who by training was a taxonomist, before he came to Morgan's lab, it wasn't until Dobzhansky showed that, at least in part, made the first steps, that the best taxonomists already way back in the 1870s, 1880s, had already solved the problem of origin of new species and what species are and all that sort of thing. And then only that evolutionary biology took big steps forward and that was the so-called evolutionary synthesis, when Dobzhansky brought together let's call them Fischer-Haldane type of evolutionists and the organismic evolutionists like myself and like himself, Huxley and Stebbins and so forth. Quite a few historians always refer to Fischer refuting the saltational evolutionism of De Vries and Bateson and so forth and called that a synthesis. That wasn't the synthesis. The synthesis was what Dobzhansky initiated putting genetics together with systematics.

SM: If natural selection doesn't account for everything, if some things are just happening and being taken along for the ride over evolutionary time, when you look at something today, how do you know whether you're looking at the product of natural selection or…

EM: It really doesn't matter. You see, another important step already in my recent evolution book and even more so in the forthcoming book is that I no longer make that sharp separation between selection and variation. Quite a few people think that the Darwinian paradigm is abundant variation and then, on top of that, selection. I say in my recent writings that you cannot separate variation from selection. Selection never would have any effect whatsoever if there wasn't variation.

And variation is meaningless as the machinery of evolution if it wasn't for the fact that there's the principle of selection. And too much of an opposition, opposition is being thrown into the theory, by those who make such a difference between variation and selection. So variation and selection to me are just two sides of the same coin. I expressed this deliberately in extreme terms, but it is just to make people realize what is involved in this case.

CA: Professor, I'd like to step aside a little bit. I've just read *What Evolution Is* and you say in the book that we should no longer call evolution by natural selection a theory.

EM: That's correct, yes.

CA: Still, we see a lot of resistance to the idea, especially in this country, which is the last place on earth where you would expect it. Who do you think is to blame for that?

EM: The American background. You see, some psychologists have pointed out that if small children prior to the age of six are told the same things again and again and again, eventually they totally believe it. And people still very much have the belief in the Bible, that every word in the Bible is the ultimate truth. If this is told to small children often enough up to the age of six, that's definitely the last word.

I found a very funny thing. Recently, I was asked by the editor of *Science* on the occasion of my 100^{th} birthday to write a short piece for *Science* telling a little bit about my attitude toward evolution and how I came to have my ideas and whatnot. And I asked myself, when did I become an evolutionist? Ask yourself that question – it's not so easy to answer. But I found out, my God, I always was an evolutionist. I didn't become an evolutionist, I was born an evolutionist, you might say. And then I said to myself why? And then I said, what about my parents? Well, of course they both accepted evolution, or as in America it would be called "believed" in evolution.

They never pounded that down. It was just so accepted that everything they ever discussed started from the basis, that evolution is a fact. And of course it is! And then came school. Did we ever have a teacher who questioned it? No. In the zoology and botany classes and so forth, evolution always was taken for granted. Everything was explained and described in terms of an evolving world, or as Dobzhansky said it in his famous statement, "nothing in the living world makes sense except in the light of evolution." And then later on at the university, German university, I was first a medical student and then a zoology student, and whenever anything was discussed or mentioned in which evolution played a role, of course evolution was always taken for granted. And the leading textbooks, even in this country, the leading textbook when I came to America in 1931, didn't devote much space to creation or anything like that. But it said there were two theories of evolution: the Lamarckian and the Darwinian! So, people who are not brainwashed with religious ideas, but just have learned about the living world, usually take evolution as something for granted, as obvious.

CA: Do you think evolution will eventually prevail over religious brainwashing?

EM: I tell you, a colleague of mine at the University of California at Riverside made an experiment. He had given for years a course in evolution. And he found that in Southern California that there was always a certain percentage of creationists among his students. So one year before the first class he gave every student a questionnaire to fill out. A very ordinary questionnaire: do you believe in God? Do you believe that every word in the Bible is to be taken at face value? And so on and so forth. And he got a pretty good description of the belief structure of every one of his students. And after he had given this course, in which he presented all of the modern evidence why evolution is a fact, he gave that same questionnaire to the class again to see how many of his students had been affected by his course and what they now believed. And he was quite shaken, because the ones that had been creationists still said yes, I believe in God, yes, I believe that the world was created in six days, yes, all the things in spite of for a whole semester being shown that this is all nonsense.

ML: Well, Stephen Jay Gould would say that those are non-overlapping magisterial.

EM: Yes, well, there is a little bit of truth in it.

SM: As long as God believes in evolution, then we're okay, though.

EM: (Laughs.) Yeah.

ML: So you are still getting up at 6:15 every day to write letters?

EM: No sir. I'm rather dismayed how many days I have to work on my willpower very strongly in order to get out of bed before 8 o'clock.

ML: But you still write letters in the morning.

EM: I write letters, I write manuscripts, in addition to the book that is in press I have two manuscripts that are going to be published some time this or next year, and I have a whole list of manuscripts that I would like to write. And people say, "Why do you punish yourself like that?" And I say, "Punish, hell! I enjoy it!"

SM: I don't want to get off on a tangent, but at your symposium on Monday were you there for a talk in which it was claimed that single-celled organisms classified as being in different kingdoms were mating and producing offspring in a case of instant speciation. It strikes me, though, that when dealing with single cell organisms, even if they're technically classified as being in completely different kingdoms, the fact that they are single-celled and we are dealing with genetic material, which by its very nature is very similar throughout all life, that breaking down the species barrier in single-celled organisms is not such a big deal.

EM: I'll go one step further. All so-called asexually reproducing organisms do not have species. You see, sexual reproduction is one of the things that came in with the eukaryotes. Well, when it comes to the lower organisms, we don't really know yet how, the prokaryotes are difficult enough, but then when you get into the low eukaryotes, there is this group that is a sort of a garbage can called the protists. And there are authors I'm told that recognize 80 phyla of protists. God knows what there is in these 80 phyla. And most of them do not have species in the normal sense. They don't have a proper process of speciation or anything like that.

SM: That's a real Gordian knot-cutting response to a question that's been bothering me for some time. But that's the only thing makes sense, that our conception of what a species is has to be more limited.

EM: Of course.

SM: You said something to me once, I had your book *One Long Argument,* and you said it was your daughter's favorite because it was very clear. And then you said, "You know, my books are so straightforward that the New York Review of Books has never even reviewed one." Which I thought was such a funny line.

EM: This was literally true. My 1963 book *Animal Species and Evolution* was reviewed by Dobzhansky and he sent his review to the New York Review of Books and he got his check. And then after a year or so when the review hadn't appeared, Dobzhansky called up the editor and said why hasn't it been published. And the answer from the editor was, "Well, it wasn't controversial enough."

SM: One of the speakers at your symposium said your three secrets were to walk an hour a day, eat yogurt and keep publishing.

EM: For many years, not any more actually, but for many years I also took vitamin E every day. I had some neighbors who swore by it. In the 1950s. I said, well, I don't believe in this vitamin E business, but I'm sure it can't hurt.

SM: Thanks very much for your time.

EM: You're most welcome. Greetings to everyone at *Scientific American.* And I hope you find my provocative ideas sufficiently useful that you will at least be gentle in your criticism.