In Their Own Words: Paul Ehrlich

In Their Own Words chronicles the stories of scientists who have made great contributions to their fields. These short histories provide our readers a way to learn from and share their experiences. Each month, we will publish in the pages of BioScience *and on our podcast,* BioScience *Talks (http://bioscienceaibs.libsyn.com), the results of these conversations. This history is with Paul Ehrlich, president of the Center for Conservation Biology and Bing Professor of Population Studies Emeritus at Stanford University. He is also a past president of AIBS.*

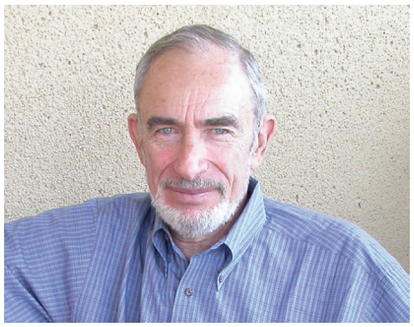
Note: Both the text and audio versions have been edited for clarity and length.

When did you first know that you wanted to work in the life sciences?

I can't really remember when I didn't. I was very interested in nature when I was a kid. My father was a businessman, and my mother was an intellectual who encouraged all kinds of interests. I was sent to a Boy Scout camp, where there was a nature program, and I got interested in nature, and eventually, I was introduced to butterfly collecting.

Later, when I was 15 and living in north Jersey, and I went to the American Museum of Natural History and was introduced to Charles Michener. He was then the curator of Lepidoptera. He was the world's greatest expert on bee taxonomy and the behavior of social insects, but the only job he could get, getting out of Berkeley, was to be curator of Lepidoptera, and he conned me into mounting butterflies for him by giving me, for my butterfly collection, specimens from around the world without data. And he encouraged me to join the Lepidopterists, Society, of which I suspect I'm now the oldest living charter member.

I should mention, I had a huge debt to Michener. I met him when I was 15, and we were still friends in his last year, when he was 97, and I was 82. We had perhaps the longest friendship of a student and a mentor in the history of biology, or close to it. Mitch was absolutely wonderful.



When, through the Lepidopterists' Society, I got to spend some summers in the Arctic; that was when I was certain I wanted to go into science. I was very interested in anthropology, too, and after living with the Eskimos for about four months one summer while working for the Canadian Defence Research Board and Department of Agriculture, I almost went into anthropology, but I decided butterflies were better. Ultimately, it was meeting Michener that determined for me that I wanted to go professionally into science.

What was the draw of anthropology to you?

Well, I learned a non-Indo-European language, or part of it, and was able to observe the way the acculturation of the Inuit was going. I learned to admire the Inuit and to appreciate their sense of humor, and I tried to learn as much as I could about their culture. I put together—and I still have it—what might be one of the first dictionaries of Inuktitut, the Inuit language, in my field notes for that summer. I found human behavior fascinating, and I still find human behavior fascinating.

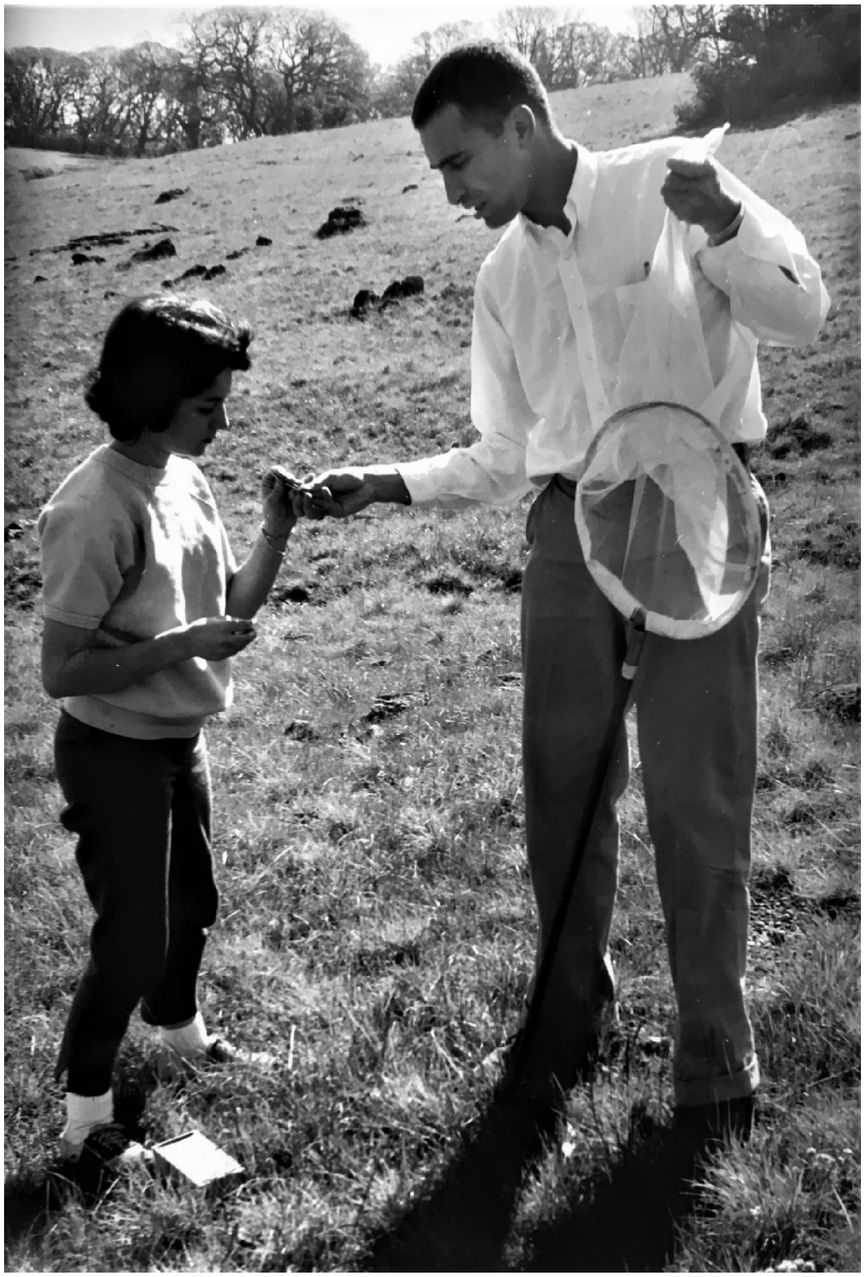
Although we're involved in the AIBS, I think the real action in science today has to be in the social sciences, anthropology being one of them, and I have tracked anthropology and written on anthropological and psychological topics a lot over my career.

But I've always been interested in science, and the big worry with the butterflies was whether I'd be able to make a living at it. The question was should I go into business like my father had and make enough money to live on and then have 2 weeks a year of vacation to work with the butterflies, or should I go professionally into working in evolution and taxonomy, which is what I was thinking about at the time and take the chance of starving?

What was it like at that time, starting out a career in science?

It was exciting from a number of points of view. First of all, I was very lucky my parents could afford to send me to an Ivy League school. We weren't rich, but we weren't poor, either, and my father was willing to put up the tuition, which wasn't as ghastly proportionally as it is now. I ended up rooming with several veterans of the Second World

BioScience XX: 1–8.
doi:10.1093/biosci/biaa120



Ehrlich doing fieldwork with assistant Jody Wilkerson in 1960, at Jasper Ridge, in the eastern foothills of the Santa Cruz Mountains, in California.

War, and I got very much involved in discussions of politics and of the environment, and I and several of the others read books by William Vogt and Fairfield Osborn on the environmental problem of those days, and we used to argue about it.

Meanwhile, I went to the museum in Philadelphia, the Academy of Natural Sciences, where James A. G. Rehn, the curator of Orthoptera, and Ruth Patrick, who was a young then-starting-up biologist, were working. I met those people, and Rehn let me become the volunteer curator of the butterfly collection there. A friend that I had met through the Lepidopterists' Society, Nick Gillham, who later had a great career as a *Chlamydomonas* geneticist, he and I published a couple of papers in butterfly taxonomy. So I had a semiscientific career there.

This was all, of course, in the days before the National Science Foundation (NSF), and it was the start of the time when a lot of money was injected into science, following up on the scientific aspects of the Second World War. All of my buddies at Penn in the Zoology Department were going to be doctors. They were premeds, and fortunately, Rudolph Schmieder, who was the editor of the *Entomological News*, was a Penn faculty member who encouraged me in my butterfly work, and he helped arrange through John Fogg, who was the vice provost of the university, to get me to leave Penn early two springs to go to the Arctic, because Tom Freeman was running the Northern Insect Survey, and I had met him at the first Lepidopterists' Society meeting, and one thing led to another.

I ended up working in the Arctic, having wonderful adventures, or sometimes not so wonderful, and when the time came to go to graduate school, the Entomological Society of America had its annual convention in Philadelphia, and I tracked down Michener at the meeting, and he remembered me. I gave him reprints of a couple of my papers. I said that although I had spent my undergraduate days drinking and chasing women, I had continued my work with butterflies, and would he give me a chance to go to Kansas, and I'd show him what I could do? I also applied to Cornell, which was the biggest entomology department at that time, but Michener accepted me.

So I wrote a letter to Cornell, withdrawing my name as an applicant, and they wrote me back a letter saying it's a damn good thing you withdrew your name, because we wouldn't have accepted you either. That actually gave me great pleasure later on, because Tom Eisner, who hopefully everybody in the AIBS knows, was a good friend of mine and invited me to give many decades later a distinguished lecture at Cornell.

Tom had been turned down by Cornell for an undergraduate career, although he was on the faculty there for probably 40, 50 years. He was able

Paul and Anne Ehrlich, circa 1950s.

to introduce me and say that both of us had been refused entry into Cornell, which was a nice moment.

What would you say is the biggest surprise of your career?

The biggest single and most important surprise of my career came at Kansas, when I was a graduate student and working on my dissertation, which was a phylogenetic study of the higher classification of the butterflies. I'd gone to Michener and said what I'd like to do for a dissertation is a revision of the Satyridae, which was the group of butterflies that occurred commonly in the Arctic and alpine zones that I had collected and worked on in the Arctic.

He said, "The butterfly higher taxonomy is all screwed up. Why don't you do a study of the higher classification and phylogeny of the butterflies."

Being, of course, an egotistical kid, like I'm an egotistical adult, I thought, *Sure, the bigger, the better.*

We had a seminar, Michener, Sokal, myself, and Jim Chilcott, who I had met in the Arctic. We were discussing evolution, taxonomy, and phylogeny, that sort of thing, and Bob said one day, "You guys aren't studying phylogeny. You're just putting things together according to their similarities."

The rest of us, Mitch, me, Jim Chilcott, Earl Cross said, "That's bullshit. We are studying the phylogenies."

Sokal said, "No. If somebody just measured, say, 50 or 100 characteristics of your organisms, you could then do correlation coefficients and come out with what you call a phylogeny but is really just a dendrogram showing the similarities."

We argued about this for a couple of weeks, and then Mitch and Sokal decided they had to put it to a test, and the test was that Mitch got one of his graduate students to examine, say, 100 different bees and measure 100 different characteristics, either linearly or say curved, straight, fuzzy, not fuzzy, and so on, and gave Sokal 100 lists of characteristics. Sokal was the perfect controlled experiment. Bob didn't know a bee from a butterfly; he was a theoretician.

He went away and did product-moment correlation coefficients among the different numbers and devised a way of making a dendrogram out of it, a treelike diagram, and came back to Michener. Michener had to agree that not only had Bob created a proper phylogeny of the bees, but he had correctly placed a couple of parasitic species that Michener felt he had misplaced originally, and that was the beginning of numerical taxonomy.

I was stunned, because there was something that I thought I was absolutely right on, a test was run, and I was shown to be absolutely wrong. That's a good experience for any scientist, I think. Everybody should have an opportunity to be shown really dead wrong. I've had many subsequently, but that was the first and most stunning surprise I got.

My other surprises are things like finding out how stupid I'd been when I got together with an orthodontist, and she pointed out to me that there was some evolutionary problem in the fact that all the kids around had to have braces in their mouths (see https://doi.org/10.1093/biosci/biaa073). It had never dawned on me that that is really, evolutionarily, a funny thing. Today, orthodontists still think that malocclusion is somehow "genetic," even though that's absolutely nonsensical.

I'm often surprised at the conservatism of scientists. People still write papers on how to define species, even though that problem was totally solved in the 1960s by papers by me and Sokal and Crovello. But they keep at it. Scientists are like other people. They can be extremely bull headed.

How did you go from that early study of insects into looking at questions of human population, the carrying capacity of the globe, and those types of things?

The transition there was partly due to my assistantship with Bob Sokal. That's what Mitch got me, an assistantship working on the evolution of DDT resistance in *Drosophila*. It was wonderful from my point of view to actually see selection at work in the lab. In 10 generations, you could make fruit flies totally resistant, basically, to DDT, and in 10 generations of sib selection, you could have them drop dead if you said the word "DDT." The implications of that, for example, for antibiotic

Paul and Anne Ehrlich photographed with a panda. Photograph: Jack Liu.

resistance and so on were crystal clear to us in the early '50s.

Most of the stuff on population and so on is not in any way deep, intellectually. There are deep intellectual questions related to evolution, obviously, and in understanding reproductive behavior and politics, but not in the impacts of human numbers on the environment. When I was hired at Stanford in 1959, the department chair said to me—and it's the only instruction I ever got from Stanford, having been on the faculty there for something like 60 years, and now being emeritus—he said, "Could you teach a course on entomology and one in evolution?" and I said, "Yes."

I started teaching an evolution course on a quarter basis, which meant there was 10 weeks of course and three lectures a week. For the first 9 weeks, I lectured on evolution, where we had come from. The last week, I lectured on evolution, where we were going. The course became very popular, and particularly the last week. I began to get requests particularly through Stanford's alumni program to talk to people about the environmental situation, which was basically where we were going, and I started doing it. Through some alumni, I was invited to talk to the Commonwealth Club in San Francisco, and I did not know at the time that talks at the Commonwealth Club were often broadcast on a series of radio stations.

So I talked at the Commonwealth Club. The topic was "The Food from the Sea Myth: The Biology of a Red Herring." It got a lot of attention from the radio, and I talked a lot about the population side of the issue, and Dave Brower, who was then at the Sierra Club, and Ian Ballantine, who invented paperback books, came to me and said, "Could you put down the things you're saying in that lecture in a short book? Maybe we can change the results of the election, get this injected as an issue in the election," which was coming up with Lyndon Johnson running. That would have been '68.

Anne and I wrote the book in a week of evenings, and that's where *The Population Bomb* came from. A celebrity of the time liked the book and sent it to Johnny Carson. I got asked to do *The Tonight Show*. With Charlie Remington, who was the founder of the Lepidopterists' Society at Yale and an attorney, I had started the organization ZPG, Zero Population Growth, and Johnny let me give the address on *The Tonight Show*, and it led to the largest flow of mail ever into the headquarters of ZPG, which at the time had six chapters and 600 members. By the time I'd been on *The Tonight Show* three times, it had 600 chapters and 60,000 members, which showed me the power of the mass media.

How have you enjoyed doing that sort of outreach over the course of your career?

Love it. If you're a born loudmouth, when you can talk to 50,000 people at one time, or 5 million, instead of 50 in a class, you love doing it. Of course, it has some downsides, like the threats and so on, but the upside is, if you get that kind of notoriety and you're a field biologist, you get to go to places you ordinarily couldn't possibly go to.

For instance, it led to a career in cruise lecturing. Everybody predicted I would hate being on cruises. Actually, I loved it, because the deal normally was, if I'd give two lectures a week, they'd take Anne and me, and sometimes Anne, me, and our daughter, Lisa, on a cruise. Two lectures a week, for a loudmouth who teaches all the time, is absolutely nothing, and yet you get to go to places which are financially out of your reach and often fantastic.

I got to travel all over the world through cruise lecturing on ships and through Stanford having a travel study program, which you lecture to Stanford alumni, but leads to things like around-the-world flights, going to places like Micronesia, Korea, and Taiwan, that I normally would never have gotten to, but great places to study reef fishes, butterflies, and politics. As far as I'm concerned, being a public scientist had huge benefits, as well as, of course, some disbenefits.

On a ship, I hate to tell you, when you're not occupied lecturing, you can do whatever you damn well please, and that means, often, reading and writing. You have your own cabin. You don't have to repack every night

Ehrlich getting a Florida jay as a "lifer." Photograph: John Ogden.

like you do when you're traveling or doing fieldwork on the move, as we have done. It's just wonderful. It's an opportunity to be very productive in wonderful circumstances, and you can do your hour walk on the deck early in the morning to get your exercise, watch flying fishes if you want to relax, and during the day, your big decisions are where to eat lunch, whether to go to movies after dinner, when to make love. It's a tough life.

Sounds brutal.

It is terrible, but thank god, I had guts.

Do you think it would be harder now to be a public scientist now than it was then, because of things like social media, the openness to a greater degree of criticism from various circles?

It's very hard to say. Again, I was very lucky. There was, of course, associated with *The Population Bomb*, an enormous amount of criticism, including a lot of threats. But first of all, I believed, and I think most scientists believe, that you live by your reputation with other scientists. Anne and I have always been very careful, if we have a new idea on something, to try and get it into the peer-reviewed literature.

The Population Bomb, for example, was read by a whole bunch of top scientists, including Peter Raven. Don Kennedy, who later became the editor of *Science*, the head of the Food and Drug Administration; Stanford's president, read it. As long as I knew I wasn't out on a limb, I didn't mind being heterodox within science. But I always want to be in a position where scientists recognize me as heterodox—maybe wrong, but possibly right. I've been fortunate that way, and I was also fortunate in being able to be one of the first, thanks largely to Johnny Carson, to be able to address very large audiences on central mass media.

The Tonight Show had audiences of tens of millions. I did it more than 20 times, I think, all told, and I did a whole series of similar shows, and John and I used to have a really good time. I found it very enjoyable to do those things, and today, I don't think I've seen a scientist get the kind of time I got on *The Tonight Show* recently on any of the central media, but there are all the blogs and so on. The trouble is to penetrate the remains of the mainline media today is very difficult.

A good example currently is everybody's focused on the coronavirus. A pandemic, of course, is something all of us had predicted for years, in detail. But no attention is being paid to the much more serious problems. A little bit is being paid to climate change, because we're seeing the results of climate disruption now, but the things like the toxification of the planet, the fact that human sperm counts are dropping precipitously—in one study I saw, there's the prediction that by 2050 the average sperm count in the West will be zero. This stuff is being ignored, even though we have these huge existential threats, not the least of which is accidental or purposeful large-scale nuclear war.

At a time when we need scientists speaking out even more, it's tougher to find the platforms on which to speak. Anne and I have not seen a single thing on what the re-fuzing of our nuclear triad has meant to the stability of the nuclear standoff with the Russians and the chances of an accident or nonaccident large-scale nuclear war, which would end civilization. We, of course, went into detail about in the '80s with the nuclear winter studies. But that's not even mentioned these days, because of the focus on the bungling of the pandemic by the government.

As I said, actually, to a group in the National Academy not long ago, I think every scientist's nowadays main duty—if they're not working on trying to solve the many open issues on SARS-2—should be working to get rid of the thugs in the government, particularly Trump and the people who are enabling him, the Republicans in the Senate.

Anyone who thinks that science in the form that most of us would like to see it is likely to persist in any way if Trump is reelected or refuses to leave office, I think is just whistling in the dark. I think it's a duty of scientists to get out in the public and not just talk about how to deal with the epidemic, which of course as I said is being totally bungled, but also make the parallels to what happened in Germany in the early 1930s. Keep watching the Reichstag.

Scholars have a responsibility to inform people, and boy, we sure have failed in the educational system to get people aware of the existential threats to our society. That goes for Stanford University, too. Stanford, Harvard, and Yale are all pathetic in this area, and you can get all the way through Stanford as an undergraduate—or even a graduate student—if you don't take the right courses, and not know what the second law of thermodynamics is, what an ecosystem service is, what the size of the human population is and what difference that ignorance makes in your ability to function as a citizen.

Do you think the problem there is shortcomings of present practitioners of science, who aren't getting out there enough, or do you view it as a structural issue, in which we no longer have the media outlets that are mainstream?

Well, I think part of it is structural, no question about that. there are all sorts of structural issues, which are complicated and I can't even give you a full answer to. One night, Robert MacArthur and I had long, drunken discussion—at least I was drunk. I don't remember whether he was or not. But it was a conversation over whether the NSF had been worth it. In other words, steering science and steering to the big science has led to some things like much too much emphasis on the medical science's cure areas, much too little on the environmental and prevention areas, and so on.

But would we have been better off if there was no NSF, and then how would the research have been done? It's interesting that when I first came to Stanford in 1959, there were still big issues of how science could interact with, for support, business and the military, and that has somewhat faded away, maybe faded away too much.

***It occurs to me that the point you were just making about the relative spending on medical science versus ecosystem science is a point that you made in 1990, in a publication in* BioScience.**

One of the things that bothered me at the time, and in a way still does, was the success of the American Medical Association in getting huge amounts of support for medicine, basically. I wasn't against getting huge amounts of support for medicine, but then, as today, I considered the health implications of what's going on environmentally to be even more serious than the needs in medicine at the time.

If the climate gets 3 or 4 degrees warmer, the public health consequences are going to dwarf the SARS-CoV-2 pandemic. In a sense, the main medical advances we have made are primarily getting clean water to a lot of people and keeping mosquitoes out of our sleeping areas and developing vaccines. We put huge effort into taking care of old farts like me with very expensive procedures and so on. It's not clear that that's the best way to go. On the other hand, we could probably afford it if we didn't waste gigantic amounts of money on military idiocy, the balance of nuclear idiocy that we now face with the Russians, for example.

The situation today I think is much tougher for young scientists than it was in my day. First of all, there weren't so many scientists. There wasn't the need to spend a lot of your time writing gigantic grant proposals. If you were lucky enough to get access to the media, it was a big deal and not competed against. In other words, there were very few major outlets.

But today, of course, you also have other ways of spreading the word, and social media—I tweet. Even though I don't know how many people ever see the tweet, I do it in support of the MAHB [Millennium Alliance for Humanity and the Biosphere], the mahb.stanford.edu, which is an organization designed to get the civil society focused on the big existential problems. It's very effective in one way. That is, a lot of people got involved, but it doesn't seem to have any impact, and one of the things that I find very distressing and disappointing is how hard it is to have any impact on the system the way it's designed now.

To be really pessimistic, I don't think we're going to solve the existential problems without a huge change in human culture. A major issue of how people should be educated, what the responsibilities of people are—we're a small-group animal, as I've said many times, that's struggling to try very rapidly to learn to live in large groups. We've utterly changed our environment. We brought Stone Age genes into a McDonald's world, and it's not working out well.

You still maintain a lot of the same concerns and pessimism that you've had for most of your career?

I'm much more pessimistic than I was earlier, because until Ronald Reagan, I thought we'd been making progress—too slowly, but progress in the right direction. Reagan started to turn it around, and then you end up with autocrats like Trump. Maybe it's impossible to reasonably govern and have 300 million-plus people cooperating. When we spent most of our history as hunter–gatherers and in groups of, it depends on who you read, but averaging around 100, later on with connections to other groups, but early on, with very few connections. Those were groups of people who spoke the same language, who were genetically closely related, who looked alike, had a common culture and so on, and where leadership went in most cases to the person who was best at doing what needed to be done.

The hunt leader was one person. The person who dealt with sickness was a woman who had spent a lot of time with herbs and had a memory of, say, where the best water holes were and so on. Now, we have a totally different system, and much of it traces to the idea of personal property and that people could be personal property. We still teach mostly bowdlerized crap in our histories of what's happened in the world. It's mostly not about what's really happened. It's about kings and who fought whom and who won whom. There's a wonderful book on racism by a psychologist at Stanford

named Jennifer Eberhardt, called *Biased*, which I think any scientist who wants to work in today's world and have some impact on our culture needs to read. Even though I have worked for my entire career on issues of equity, and I organized with Ralph Barr sit-ins to desegregate the restaurants of Lawrence, Kansas, when I was a postdoc there, I found myself rethinking my own biases in reading that book.

There's also a PBS series, which Anne and I watched just recently of about six one-hour shows called *Liberty: The American Revolution*, which actually told much of the real story of the American Revolution, what they were after, who they were and so on. I've always recommended to my students that they should read the Federalist and Anti-Federalist papers, or at least sample them, because the problems which the 13 colonies, which were really separate states, faced after the war was how to get organized so they could retain their distinct characteristics and still work together to solve their overall problems, like how to keep the British away and so on.

It's exactly, in a sense, the same problem that 195 separate states are trying to solve globally, where they want to remain distinct in many ways, but they can't solve problems like climate disruption, the loss of biodiversity, global toxification, one at a time. It's got to be solved by everybody, and we haven't found a way to do it.

We see the same problem repeated but on a different scale.

On a larger scale, and we're still evolving on the wrong track. Unhappily, we followed the cultural course that the chimpanzees took, whereas if I had been back there several million years ago, I would have voted to follow the bonobos. In other words, we solve our problems by threatening people with nuclear weapons or blowing them up with conventional weapons. The bonobos solve their problems with genital rubbing, and that's my way of going.

Do you think that these large-scale problems are solvable through cultural evolution?

I think we know perfectly well that we can solve these problems, at least in theory, through culture. First of all, we obviously don't have time to solve them by genetic evolution. People have often said things to me like, "Well, look, if fruit flies can become resistant to DDT, why should we worry? We can just become resistant to DDT." And I always say, "Yeah, in about 10 generations or 200 years. If you just kill 98% of human beings by poisoning them with DDT, you'll probably have a population that's quite resistant at the end of that."

People don't understand that generation time is a key thing in biological evolution. But look, most everybody in the United States manages to override one of our main ingrained genetic urges, and one of the central ones—that is, to maximize your reproduction. Culturally, they don't do it. You can have 10 kids, but very few women or men I've talked to have or are planning to have 10 kids. In fact, most of them have or are planning to have one or two. Of course zero or one's better, but nonetheless, we see around the world that cultural evolution can overwhelm the most genetically ingrained of our habits.

Most of the stuff that says DNA is driving our bad behavior is just people who are fascinated by DNA but don't know the numbers. There's all kinds of crap out there, including that our jaws have been shrinking rapidly—particularly over the last 300 years—because of genetics. It happens wherever people industrialize, or even starting with agriculture. Who in the world would think that in 300 years, there would be heavy selection for small jaws? Or can it be drift? If it's drift, how come it drifts in the same direction in every culture that becomes industrialized?

In other words, the lack of knowledge of how evolution works should tell everybody that if we're going to change our ways, we've got to change them with cultural evolution, and that's why we formed the MAHB, originally. The original title of the MAHB was the Millennium Assessment of Human Behavior, and Anne and I set it up with Don Kennedy, the idea being that we had in the Millennium Ecosystem Assessment discovered that the ecosystems of the planet were in deep trouble, which of course most of us already knew.

What we didn't know was why we weren't doing anything about it, and that's why we had the idea of having a Millennium Assessment of Human Behavior. Rather than looking at how rapidly things are going downhill, we were trying to figure out how to stop them from going downhill, and we've of course failed there. At the moment, for example, the Trump administration is doing everything it can to make the existential threats closer and worse, and they're succeeding.

Do you see a greater role for organizations like scientific societies to collaborate and work together to solve these problems?

Absolutely. Human beings are social animals, and there's a lot of evidence in the psychological literature that groups can solve problems easier than individuals. That doesn't meant that some extraordinary individuals don't get great ideas, but if they're not picked up by groups, they don't get implemented, generally, so I think it's very important.

For example, why I went into the AIBS was to try and make the groups more effective at what they do. One of the problems is there's still a hangover from the, shoemaker, stick to your last thing, that if you're a scientist, you only can talk science only in your most narrow field and not have any opinion on what society ought to do about your scientific results.

I think we're slowly but surely getting rid of that, but there's still enormous pressure in the wrong direction. For example, I tried to convince the Ecological Society that they should pay more attention to the social sciences, and I actually got a group of distinguished social scientists to give them that message at one of the meetings. It

had no impact. The 100th anniversary of the Ecological Society was coming up, and a group of people, not including me, tried to get them to focus it on population, because demography is the most neglected thing in the overall picture of why we're going down the drain. They basically refused.

Colleagues asked me to try and intervene, and I tried to intervene by saying, why not hold the meeting in Washington, DC, where the media might pay some attention and focus on all the things that ecologists think are being neglected in our social discourse. They wouldn't do that either. The Ecological Society held the meeting, the 100th anniversary meeting, in Baltimore, that media hub, and it was on the great achievements of the Ecological Society.

But that doesn't mean that there aren't huge advantages to having the societies. I loved going, when I had the chance, to scientific meetings, because I want to see my buddies and drink with them, rather than just do it over email. But, of course, we can stay in closer touch now. The meetings, face to face, seem to be less critical, although I still think that face to face is very important for scientists. I've been lucky, because I've been able to travel around a lot and see colleagues, and colleagues come here, at least until the pandemic.

How has your pandemic experience been?

It's been hideous. Anne and my social life, our pleasure, for decades has been having dinner with colleagues and friends in restaurants and drinking wines that we're interested in. Now, I think it's something like 30 weeks we haven't been in a restaurant. We've seen our friends, with a couple of exceptions, only in two dimensions, on the screen.

When our granddaughter drove up from LA, we couldn't get her into our building. We had dinner with some close friends, with masks on, social distancing at the end of a driveway, in smoke. But the most disheartening thing is we're in good shape. We're in an air-conditioned building where we have air purifiers, which are helping with the smoke, and being retired, we still have an income. Many people around here don't have an income. For example, many of our favorite restaurants, they're closed, and the people that work there are out of work. People are hungry in the United States. People are dying, sick, and unemployed. The total, unbelievable incompetence of the Trump administration is a constant pain for me and many of my colleagues.

There was, from the last administration, an organization designed to deal with possible pandemics, and Trump closed it before the pandemic came along. I feel like I'm in Germany in 1931 or 1932, seeing the parallels. These actions are destroying not only our democracy but I think, in even the medium term, probably destroying human civilization. I don't see any path at the moment, even if Trump is removed, that is likely to take us away from the growth path, which is leading to a collapse of civilization.

Nonetheless, I don't see any choice but to keep trying.

For young scientists, what advice would you have for them in terms of navigating these difficult times?

My first advice to people wanting to go into science is don't do it if you don't love it. People have often accused me of being a workaholic, which is super-silly, because originally, my hobbies were being out in nature, collecting butterflies, watching birds, that sort of thing, and that's what I do professionally. I love doing it, so why in hell shouldn't I do it? First of all, love what you do.

Second, don't wait until you're fully established before you get involved in the political issues. Get involved early on. It's advice I gave many years ago to John Holdren, who at the time I met him was a graduate student working in plasma physics, the world's expert in the potential environmental impacts of fusion generation of power and many other things. But he started early and went on to become the top science adviser for Obama, de facto science adviser for Clinton, worked with the Russians on all sorts of environmental problems, was central to the American response to Fukushima, worked very hard on scientific relations between the United States and China, and did brilliant work on the confinement of plasmas and also in environmental issues in general. He's the living proof, even in those days, when there was a lot of this stick-to-your-last crap, that a young scientist can have a real impact even before he or she gets fully established. I could make similar speeches about Gretchen Daily, Michael Soulé, Jane Lubchenco, and many others.

Do great science. You will if you love it. And then, always focus on how it impacts the really big picture. Science is an absolutely critical part of our civilization now. We have built ourselves into that situation, and therefore, scientists should pay great attention to other things besides what is strictly "science."

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