



# The scientist of neglected diseases

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Professor Erney Plessmann Camargo ended the interview below decisively: “I like to do research, and I no longer need to think about my career.” At age 78, the parasitologist referred to his latest interest: studying protozoa of the genus *Trypanosoma*, which have no medical importance. His confession of this topic of research seems amazing to those accustomed to seeing him as a researcher and administrator concerned with finding solutions that have an impact on public health and science management. At the same time, this interest seems natural for a researcher whose love for science always came first.

Camargo’s stories bring to mind the teachings of Professor Samuel Pessôa (1898-1976), department chair at the School of Medicine at the University of São Paulo (USP), who influenced generations of students to study medicine related to Brazilian social problems. Camargo sought to be faithful to these teachings and produced important scientific papers related to Chagas disease and malaria, two neglected diseases that affect the most disadvantaged.

Persecuted by the military regime in 1964, the parasitologist, who never denied his connection to the left, departed the country to work in the United States. Upon his return to Brazil in 1969, with no place at USP, he worked in private business for two years until he was hired by the São Paulo School of Medicine (EPM), today part of the Federal University of São Paulo (Unifesp). He remained there for 15 years and recreated the Parasitology Department.

By 1986, he was back at USP as a full professor. Hundreds of people attended the lecture

that he gave as part of the official examination for the university position in the name of the many treated unjustly by the regime. Back at his home institution, he also restructured the Parasitology group and was the first Dean of Research before becoming President of the National Council for Scientific and Technological Development (CNPq). He was President of the Butantan Institute and today is in charge of the Zerbini Foundation, which manages the USP Medical School’s Heart Institute (USP-InCor). Married with four children—all scientists—Camargo granted this interview to Pesquisa FAPESP.

***You finished your undergraduate degree in 1959, an important time for the USP Medical School. Your classmates and mentors at the time became renowned researchers, such as Luiz Hildebrando Pereira da Silva and Victor and Ruth Nussenzweig, among many others.***

Luiz Hildebrando and Victor are a little older than I. That was an exceptional time at the School of Medicine. Many students became respected scientists. Sérgio Henrique Ferreira, Walter Colli, Nelson Fausto, and Ricardo Brentani, for example, formed a small group that was beginning to do research. I, however, decided to stay in the Parasitology Department as early as my second year. The principal groups at the school that accepted students and researchers were Biochemistry, where Isaías Raw worked; Physiology, where Gerhard Malnic and Maurício Rocha e Silva worked; and Histology, the best department at the school, where the cream of the crop worked, directed by Luiz Carlos Junqueira. And there was Parasitology, which was also

**AGE** 78

**SPECIALTY**

Parasitology

**EDUCATION**

University of São Paulo  
(undergraduate,  
PhD and tenure)  
Pasteur Institute  
(post-doctorate)

**INSTITUTION:**

Institute of Biomedical  
Sciences (ICB),  
University of São Paulo

## The best place to learn natural history was at the School of Medicine

top quality. Hildebrando, Nussenzweig, and Luís Rey were all there. Samuel Pessôa [the Head of Parasitology] had already retired by the time I got there.

### ***Was there some special characteristic of that time that attracted students to research so early?***

It was a very important period in the history of the biological sciences because the double helix had been described shortly beforehand, in 1953, and they were beginning to understand how DNA worked. Peter Mitchell had discovered the process of energy production in mitochondria in 1961 and created an entire area of research around it, which, until then, had been a mystery. With electron microscopy, the cell structure began to be understood. They discovered the ribosome and how the synthesis of proteins occurs at about that time, when we were undergraduates. There

were very favorable circumstances that encouraged us to become interested in science and great professors who taught us this, such as Michel Rabinovitch, Isaías Raw, Roberto Carvalho da Silva, Luiz Carlos Junqueira, Ferreira Fernandes, and the folks in Parasitology. We had enthusiastic, informal seminars in which everyone wanted to know what was going on. The folks in human genetics, Pedro Henrique Saldanha's group, also met with us. Every week, someone would talk about the most varied subjects, not just parasites. I gave

one on the T4 virus, which was a lot of work. They were just beginning to map it. It was the start of understanding how information is stored in DNA. We studied a lot. It was very exciting.

### ***The medical course was a kind of undergraduate degree in sciences, then? [In Brazil, future physicians begin studying medicine at the undergraduate level, rather than as graduate students.]***

It was an undergraduate degree in sciences, certainly, although we could not escape the obligations of medical students. The school was very good. Even if we hadn't wanted to, we would've ended up learning medicine well. It didn't matter if your interests were elsewhere. I delivered two or three dozen babies, for example. It was mandatory. I had to do an appendectomy, with an experienced surgeon at my side, of course. It

didn't matter that I didn't want to be a surgeon. The obligatory residency in medicine had been created not long before that. My class was one of the first to do it. But I have no complaints, on the contrary.

### ***What was your research in parasitology like?***

I really liked natural history. At that time, in the 1950s, the best place to learn natural history was at the School of Medicine. Biology—today's biology is very good—was just beginning. Great researchers of that time, such as Paulo Vanzolini, studied medicine and later became biology professors. Moreover, there was the advantage that a doctor could be a biologist, but not the reverse. [A biologist would have to take the undergraduate course in medicine to be a physician, whereas someone with an undergraduate degree in medicine could continue on to graduate work in biology.]

### ***You have four children. Did they follow your example?***

Two are doctors: Marcelo, who works in Rondônia, and Fernando, who is a doctor at the Albert Einstein Hospital here in São Paulo. The other two are also scientists. Luis Eduardo is at the Luiz de Queiroz School of Agriculture (Esalq/USP), and the other, Anamaria, is a biologist at the Ludwig Institute for Cancer Research. I did not demand that they follow this path. It was natural. My wife, Marisis, is a researcher in literature and was a director of the Pontifical Catholic University (PUC-SP). But none of my children studied literature, only biological sciences.

### ***Your most cited article, on the growth and differentiation of *Trypanosoma cruzi*, the protozoan that causes Chagas disease, was the first that you wrote. Why was it important?***

The paper already has 704 citations and is still cited. There was a problem: *Trypanosoma cruzi* was very difficult to cultivate. We used what was called Muniz medium, a base of blood with agar and very little liquid. To obtain 1 gram of *Trypanosoma*, I needed 50 bottles of Muniz. I had already graduated, and it was my first research project. I wanted to study the biochemistry of *Trypanosoma*. To this end, I needed a lot of it, and one nest would not be enough. One of the things I wanted to know was how *T. cruzi* differentiated. Cellular differentiation is, even today, a fundamental problem in biology. To obtain the ideal cultivation medium for *T. cruzi*, I spent a lot of time doing culinary activities: I removed one salt, I added another, I added something else. At the same time, I was studying the parasite. This is why the article was entitled "Growth and differentiation of *Trypanosoma cruzi*" and not

“Production of the culture medium.” There was a basic culture medium that a researcher from Florida used for bacteria. I used it and started adding other ingredients. I then arrived at a medium that is called liver infusion tryptose (LIT). It was very important not only for me but also because it allowed everyone working with the protozoan *Trypanosoma* to produce it on a larger scale. I started in 1962 and spent a year doing this. But the article came out in 1964.

***Everything happened thanks to your interest in natural history...***

Parasitology was the closest thing to natural history at the School of Medicine. I could have chosen microbiology, too, but the department was not as good as Parasitology. In my second year, I became friends with the staff in the department and started to visit it. I must say that there was something more that drew me to the Parasitology Department: everyone there was politically left leaning. It was the Red Department of the School of Medicine. My political sympathies were on the left, and this encouraged our friendship. Initially, as an undergraduate, I worked with Luís Rey and Kurt Kloetzl on schistosomiasis. They gave me some small tasks, but most importantly, I participated in departmental meetings. Later, I started working with Luiz Hildebrando. He was working on his thesis to obtain tenure, and I helped a bit. Luiz did not receive a doctorate but went straight to the tenure thesis, something that was allowed at the time. When I graduated, I thought I should learn a bit more biochemistry and did a two-year internship with Sebastião Baeta Henriques at the Butantan Institute. During this period, I met Leônidas Deane, a very important figure—perhaps the most important in our group, though not the most famous—who was highly respected by all of us. He was the guru after Samuel Pessôa. Samuel was far above the rest based on his academic, political, and social activities. He was our reference. But Deane was second after him and took over the chair in Parasitology for a year. He obtained an opening and brought me in. I was a lecturer. That was in 1962, and it was there that I began working on the LIT culture, published in 1964. After the military coup of March 31, 1964, our work was suspended by Institutional Act No. 1. Me, Hildebrando, Thomas Maack, Pedro Saldanha...There were five or six in medicine. I had accumulated all of the research data and intended to work slowly. Because of the suspension, I decided to finish it quickly and began to work 24 hours a day on the LIT article. It was very difficult because it was my first article, and I had no idea what I was doing. Hildebrando and Victor had already left, and it was Michel Rabinovitch who helped me write the paper.

***What was the political activity like at the School of Medicine?***

Professor Samuel Pessôa took over the Department of Parasitology in 1931. He was untouchable in that position and was able to be a communist at an extremely conservative institution like the School of Medicine then was. The academic spirit prevailed over political convictions. Pessôa always tried to perform medical research linked to social problems. He wanted to solve the problems of the Brazilian people. I’m not exaggerating. He said that himself. He was a Communist Party candidate and a friend of Luís Carlos Prestes and had charisma and reach beyond the school. Jovina, his wife, was an ideologist, even more communist than he was. Personally, they were very pleasant people—engaging, charming. This brought great unity to the group. Over the years, I became friends with Pessôa, even though I was just a boy. It was not just a professor-student relationship. We would go for drinks at the Riviera, a bar on the corner of Avenida Paulista with Rua da Consolação, which later became popular with leftists. Jovina would get angry. When he retired, he went to work at the Butantan Institute. For free.

***Were you ever arrested?***

Pessoa was arrested several times, before and after 1964. I was when I returned from the United States in 1969. Police Chief Sérgio Paranhos Fleury went crazy and created the Fishing Net Operation (Operação Tarrafa), as it was called in repression jargon, to arrest leftist intellectuals. It was an outrage. The professors in the Parasitology Department became targets after the 1964 military coup. Those extremely interesting scientific meetings were classified as subversive meetings. In a sense, they were. In experimental science, when we want to discover new things, we must go against established knowledge. And the extremely conservative School of Medicine was founded on established knowledge, on erudition. That group was really anti-erudition, as it created new ideas. In that sense, we were subversive. But strictly speaking, no one conducted communist party activities. We met at the building on Rua Maria Antonia. I knew everyone, and we talked a lot. Sometimes, a group of intellectuals who we admired would join us: Fernando Henrique

## Parasitology had various communists and was known as the Red Department

Cardoso [later President of Brazil], Florestan Fernandes [a sociologist], Mario Schenberg [a physicist], or Vilanova Artigas [an architect].

***Did you leave the country preventively?***

After 1964, there was a Military Police Investigation (MPI) at the School of Medicine. One soldier and two assistants interrogated us in a room. At the Ribeirão Preto Medical School, this did not happen. The director, José de Moura Gonçalves, was a spectacular figure—he was my symbolic PhD advisor—and he did not let them conduct an MPI at the school. He said that if they wanted to carry out their investigations, it would have to be at the police station. At the School of Medicine here in São Paulo, the situation was quite different. The investigation was clearly supported by the administration. They interrogated everyone over a three-month period. It created a very bad environment. However, even

then, we had some surprises during this period. Since we were targets, and especially those of us in the Parasitology Department, several close friends withdrew and no longer spoke to us. Some, who were not such close friends, offered their help. I left the country because the MPI formally accused us before a military court, and we would be judged shortly. At that point, an American researcher, Walter Plaut, invited me to go to Madison, Wisconsin. I left before the trial, in which everyone was acquitted. I received a good salary in Wisconsin.

***What was the surprise that occurred during this period?***

During this period, between being fired by Institutional Act No. 1 and leaving the country, we received no salary. I had a wife and three children, and Hildebrando did, too. A group of people—I will not say who—joined together to collect money and pay our salaries. The leader of this group was a militant who belonged to the National Democratic Union (UDN), a very conservative party. We received full pay during that period with the help of colleagues at the university, and we did not even know who they were. Difficult times always bring these surprises. For example, Moura Gonçalves was a wonderful person but was not on the left. He not only prevented the MPI from being conducted within

the Ribeirão Preto School of Medicine but also helped us when the government issued Institutional Act No. 5 [a decree giving almost absolute powers to the military dictatorship], shortly after Hildebrando and I returned from abroad in 1969 and were planning to start working in Ribeirão Preto. Moura secretly gave me his full salary to purchase plane tickets so that Hildebrando's family could leave the country again.

***How was your time in the US?***

They were five very good years. I wanted to continue my research on *Trypanosoma cruzi*, but I was in a cytology lab, and I was not allowed to perform research with pathogens. I had to find something else and chose to work with a local aquatic fungus. I could carry out decent biochemistry research there. I joined Professor Jack Strominger's group. He had discovered how penicillin worked. My friends Carl Peter von Dietrich and Julio Pudles were there. We worked together on synthesizing the wall of my fungus, which is made of chitin. Together, we discovered the synthesis mechanism for chitin. It was an important result and is still cited even today.

***Why did you decide to come back in 1969, when the regime was worsening?***

It didn't seem like the regime was worsening. The government created a reintegration program for scientists and invited us, Hildebrando and me, to return with some perks. The program was coordinated by Paulo de Góes at the Federal University of Rio de Janeiro. That's why we came back. But four months later, Institutional Act No. 5 was issued. It was clear that there were conflicts within the regime. Without a job at the university, I went into industry. I worked at Editora Abril, a publisher, at the invitation of Pedro Paulo Popovic, an intellectual in the true sense of the word who was highly regarded by the Civita family, owners of the publishing house. He brought in many people from the left. In my case, I was hired to work on the Medical Encyclopedia and Medicine and Science. The articles came from Italy, and we adapted them for publication here, inserting information on Brazilian diseases. I also worked in the analytical laboratory Lavoisier. I lived that way for two years and earned more than I would have in 10 years in academia. But what I really wanted was to return to the university.

***Was that when the invitation came from Unifesp?***

During the period in which I was at Editora Abril and Lavoisier, I also worked at the Gastroenterology Institute, run by José Pontes. I established an analysis and research laboratory there. When I was at the Institute, Professor Leal

More than 200 people attended the lecture I gave as part of my official examination for the USP position as a sort of redress

Prado invited me to go to the São Paulo School of Medicine (EPM), now part of the Federal University of São Paulo (Unifesp). They had created a Biomedical Sciences course and invited me to teach Microbiology and Parasitology. I thought it was great, but I warned Prof. Prado of the problems with Institutional Act No. 7, which prevented people who had been suspended from being hired for a government position. He told me to talk to the dean. And that was when I was surprised again. The dean was Horácio Kneese de Mello, who simply hired me, saying that he was not obligated to obey any institutional act. I went to the EPM and began as an assistant professor and soon rose to associate professor and then full professor.

### ***When did you finish your doctorate?***

The graduate course, as it is known today, was created in 1967. I was given the choice of following the old or the new doctoral program. I chose the old system, in which I only needed to write a dissertation. Since I had done work in the US, all I had to do was organize it and talk to my pro forma advisor, Professor Moura Gonçalves.

### ***How long did you stay at the São Paulo School of Medicine?***

Fifteen years. When I arrived, the Parasitology Department was not strong. I hired biochemists and biologists, but no parasitologists, to change the department's outlook a little. The people there worked a lot and gained national recognition. The department is small today but scientifically spectacular. That was something important in my career: the recovery of the department and the creation of a graduate degree in Microbiology, Parasitology, and Immunology at the EPM, together with Luiz Trabulsi and Nelson Mendes. The course had a Capes [Coordinating Agency for the Improvement of Higher Education Personnel] grade of 7 (the highest) right from the start.

### ***Why did you decide to leave?***

At the EPM, the Parasitology group had three or four professors and could not grow. At USP, the group included professors from eight schools, with a total of 20-25 professors. The difference was enormous. I returned to USP at the invitation of Flávio Fava de Moraes [later President of USP], who was Director of the Institute of Biomedical Sciences (ICB). Everyone understood the reasons that I returned to USP and supported me. The official examination was beautiful. There must have been 200 people there for my lecture, which is rare. Normally, official examinations for full professors attract at most 20 people. Mine was a sort of redress. People from all over the university came. When I arrived, Hélio Guerra Vieira was the president of USP, and José Goldemberg was the

president after him. Goldemberg asked me what I needed to change the department and gave me all of the support I needed. I hired eight or nine new professors, and we were able to purchase machines and material. What a leap. Scientific production went from 0.2 articles per year per professor to 4. During the same period, the University's structure changed. A dean of research position was created around 1989-90, and Goldemberg nominated me for the position. I was the first Dean of Research at USP. Roberto Leal Lobo succeeded Goldemberg. At Lobo's request, I continued on as Dean.

### ***You redesigned the department and set up the dean's office at the same time?***

Yes, I worked a lot during that period. But since there were a lot of good people here in the department, I did not need to supervise my colleagues much. What took a lot of time, during Goldemberg's tenure and then Lobo's, was obtaining a large loan from the Inter-American Development Bank for the University.

### ***With all of these management activities, did your lab work fall to the wayside?***

I always continued working with my staff. My production level fell, of course, but never to zero. After I was no longer the department head, I went to the Butantan Institute to direct it and then to the National Council for Scientific and Technological Development (CNPq). There were no crises while I was at Butantan. It was a quiet year. Since I am a member of the Institute's board, I had to take on the presidency two other times to calm people in moments of crisis. But it was only for two months each time.

### ***What about during your time at CNPq?***

It was at the start of Luiz Inácio Lula da Silva's first term as president. In the beginning, it was complicated because a lot of people rejected Roberto Amaral, the Minister of Science and Technology. The CNPq had no money and was not paying the funds it had granted. To our surprise, the President gave us the support we needed. When Robert obtained funds, the first thing I did was pay off all of the CNPq's debts because I would have no credibility with the scientific community if I didn't. Later, I could think

To study malaria, I saw that we had to go to Rondônia and not just stay in São Paulo

## Evaluating science using the impact factor is dangerous because the measurement is for different areas

about future projects. It was a good strategy. In the end, Minister Roberto Amaral proved to be a great minister, sensible and competent. He gave me great support. We are friends even today.

### ***Was the Lattes Platform [for storing researcher CVs] created under your management?***

It had already existed offline. Researchers had to download a program, fill in the data, and send it back. It was very complicated. We simplified the process and put it online. That was in 2004, shortly after I got there. At that time, we also created the Carlos Chagas Integrated Platform for use by researchers and for contact with the CNPq.

### ***Before CNPq, you spent time doing research in Rondônia. What was that experience like?***

It was important. Before working there, in the 1980s, I took a post-doctoral position at the Pasteur

Institute to learn more about molecular biology. At the time, I was trying to determine what had become the largest challenge in parasitic diseases in Brazil. Chagas disease had been practically controlled. I concluded that the biggest problem was malaria as a result of the decision by the military government to promote the transfer of people from the south to the Amazon region. There was an increased incidence of cases, from 1 million to 1.5 million cases per year, just in Rondônia. When I came to the department at USP, I saw that we could

not forget about malaria. We had to have a field project. We set up a project for the Amazon region and took advantage of the experience of Professor Marcos Boulos, who ran a research center in Rondônia. I proposed a joint project to Hildebrando, who was then at the Pasteur Institute in Paris. If need be, we could do the molecular biology research at Pasteur and in my lab, but the fieldwork would have to be done in Rondônia. I went in 1982, and the project began in 1990. The project was funded in part by the World Health Organization, by FINEP—to create the framework in Rondônia—and later we obtained a Pronex [Centers of Excellence Program] grant for the Rondônia project. The ideology of this process was “we need to participate in the national health program, and malaria is the best thing to work with.” There were two things to do. One was to better understand

the epidemiology of the disease, and we published many papers on the topic. The second was to use molecular biology to clarify many unexplored aspects of the disease. It was a very successful project. The conditions were poor in the beginning. As always, it was difficult to work in the Amazon, but we gradually obtained resources and made it work. My son Luís Marcelo and Marcelo Urbano Ferreira, now head of the USP Parasitology Department, became professors in the department and were allocated to the Rondônia project. Luís Marcelo is still there today.

### ***You set up an outpost in Porto Velho?***

The Rondônia state government had a Tropical Medicine Research Center (Cepem) in a hospital in the city of Porto Velho. We began working there. Later, we moved further inland. Luís Marcelo moved to Monte Negro, and Hildebrando continued on in Porto Velho. Hildebrando established a foundation there, of which I am a director. The second most important parasitic disease in the Amazon is leishmaniasis. However, there was not a single clinic in Rondônia to treat people with this disease. We established a clinic in Monte Negro and have already treated 5 million people because they come from all over the state. It is an official branch of the ICB. In parallel with this clinic, we continue to do research, and years ago, we published a very important article on asymptomatic malaria.

### ***Why is asymptomatic malaria important?***

We thought that prospectors were largely responsible for the spread of malaria. When they arrived in a new region, everyone caught malaria. But it was not the prospectors who brought the malaria; it was the opposite. They mixed with the population who carried malaria and contracted the disease. We did not know this because no one knew that the people living along the river were infected. They were asymptomatic and lived normal lives. They developed a resistance to the plasmodium after contracting the disease repeatedly. It is not a sterile resistance. They contracted an attenuated form of the disease. The anti-malaria program recommended treating those with malaria. If you contracted malaria, you were treated. But it is better to treat those who do not have the disease because they are the reservoir of malaria. In a region of Porto Velho, Hildebrando clearly showed that treating asymptomatic individuals caused malaria to be weaker when it returned the following year.

### ***Is treatment able to eliminate the parasite from the blood?***

Yes, completely. But the person can contract another form of malaria.

### ***Do you believe a vaccine is possible?***

It would not be easy to produce a vaccine, principally due to the plasmodium's polymorphism. I agree with Victor and Ruth Nussenzweig that a vaccine will appear, but I do not know when.

### ***What about for Chagas disease?***

After World War II, DDT appeared and began to be used to fumigate houses and eliminate the vector, *Triatoma infestans*. In 1960, Chagas disease had virtually disappeared from the state of São Paulo but was still very common in the rest of the country. In the 1970s, we started holding meetings in Caxambu on Chagas disease as part of the Integrated Program on Endemic Diseases (PIDE), funded by the CNPq. The meetings involved everyone who was working on Chagas disease. This created an awareness in the scientific community of the importance of the disease. The results were great. Basic science researchers became aware of the importance of Chagas disease in Brazil and eventually convinced João Figueiredo's military government to create a national program to combat the disease. It practically ended household transmission within a few years for less than \$100 million. Today, the program is being adopted by all Latin American countries.

### ***Brazil produces 2.3% of the world's science, but this output has not yet had much impact. The average number of citations of Brazilian research is low. How can this be improved?***

Let's take Brazilian parasitology as an example. Today, it oscillates between the second and third most productive in the world. The first is the United States, of course. The second is England. We compete with France for third place. Parasitology is Brazil's top science. However, the global audience for parasitology is very small compared with that for cancer, for example. Work in parasitology or infectious diseases has a smaller impact because impact is measured by the number of readers and not the intrinsic quality of the work. Judging and evaluating science by the impact factor is very dangerous because very different areas are being compared.

### ***What about your research today? What are you doing?***

Let's see if you can guess. Dengue fever? No. Schistosomiasis? No. I returned to the study of trypanosomes, but now only those of no medical importance. Today, I belong to a team that studies trypanosomes in wild animals and insects. We study the biodiversity and phylogenetic relationships of these trypanosomes. We work and collect material throughout the world: in Brazil; the Americas; and Madagascar, Africa. Samuel Pessôa did the same thing. After he retired, he went to the Butantan Institute and began studying ma-

laria in snakes. We also have studied trypanosomes in alligators, snakes, monkeys, rodents, and especially insects and bats.

### ***Are you trying to trace the evolutionary history of these parasites?***

Yes. Using molecular techniques, we have tried to trace the evolutionary history of trypanosomes. Let me give you an example. There is a trypanosome, *T. erneyi*, described by Professor Marta Teixeira, which infects bats in Africa. In Africa, parasites of the genus *Trypanosoma* cause sleeping sickness; here, they cause Chagas disease. The continents separated about 100 million years ago. The question is how did *Trypanosoma cruzi* appear in the Americas? It's not the same as the African trypanosome. It's different. The hypothesis was that the two existed on the ancient Gondwana supercontinent and that when it split, one was there, and one was here. Now that we have begun to study trypanosomes in bats from Brazil, Africa, and Europe, we have found a trypanosome just like the one causing Chagas disease in bats in Africa. British researchers, together with Marta Teixeira, have already published an article with a brand new hypothesis. *Trypanosoma* (*Schizotrypanum*) *erneyi* might be the *T. cruzi* of Africa and probably came here at some point carried by bats. They estimate that this occurred between 20 and 15 million years ago, when the continents were already separated. Another example of what we have

been doing is related to trypanosomes in alligators and crocodiles. About 10 million years ago, the genus *Crocodylus* originated in Indochina, Indonesia, and crossed the Pacific Ocean, arriving in America. In the Amazon, these reptiles met our alligator, the genus *Caiman*, and then went to Africa. Our studies on trypanosomes in alligators and crocodiles show that during their passage through the Americas, alligators and crocodiles exchanged trypanosomes. Today, the trypanosomes of alligators and crocodiles are very similar, almost twins. These studies have given me much intellectual pleasure. I travel around the world and back to our laboratory, without any other commitments aside from research. There is no lack of resources: we have support from the CNPq, Pro-Africa, FAPESP, and USP. I continue to work, and it is a pleasure. I like to do research, and I no longer need to think about my career. ■

Today, I belong to a team that studies trypanosomes in wild animals and insects