Oral History

Henry Borsook–

How It Was

The Institute Archives, borrowing a page from Herodotus, has now initiated an oral history program. The staff, under the direction of Judith Goodstein, began by inviting a number of emeritus professors to share their memories with them. Recollections of childhood, anecdotes about others, and memories of the Caltech that once was are the stuff out of which these oral histories come.

An oral history, however, is made up of more than memories. It takes the diverse skills of the researcher, interviewer, transcriber, editor, and typist to produce an edited, indexed, and bound transcript from the interviews. The two people, interviewer and subject, typically spend three or four sessions, each an hour or so in length, talking to each other. Once transcribed, the manuscript is read and edited by both people; the subject signs an agreement regarding its use; and the transcript is then deposited in the archives.

One of the first completed accounts in this program is from Henry Borsook, professor of biochemistry emeritus, who was interviewed by Mary Terrall. Borsook, noted for his work in protein synthesis and for his contributions to the field of nutrition, was born in London, England, in 1897, and came to Caltech in 1929. After his retirement from Caltech in 1968, he continued his research until 1978 at the University of California at Berkeley on the function and production of red blood cells. The Borsooks are now living in Santa Barbara.

E&S *has made a shortened version of the original transcript and presents here Part One (of two parts).*

Mary Terrall: I'd like to start with your childhood and educational background. I know you were born in London. What did your parents do there?

Henry Borsook: My father was a tailor. My mother was a housewife. My father was born in Russia. My mother was born in Romania. They emigrated to Canada in either 1906 or 1907 and, of course, I went with them. So my early schooling was in London and in those days you went to school at the age of three and it wasn't a kindergarten. You started right off learning to read and do arithmetic and such things. So when I went to school in Toronto, Canada, I was a year ahead of the other children as far as schooling was concerned. But otherwise I had all my schooling in Toronto. Public school, high school, university, medical school.

MT: When did you first get interested in science?

HB: Well, even as a child I had intended to become a doctor. And so I first went to the university. The course I took first was in physiology and biochemistry, which was really a kind of premed course. It was then that I became interested in science and specifically in biochemistry. If you ask me why, I can't tell you. It was just one of those things. So I stayed in the department of biochemistry and took my PhD there. But I went on to the medical school afterwards to get a medical degree as a grubstake. That is, I wasn't sure that I could make a living in academic work, but as a doctor, well, the chances were I could. And so after I graduated in medicine, I rejoined the department of biochemistry at Toronto for one year. The man I worked with for my PhD was a friend of Dr. Thomas Hunt Morgan in New York, and when he learned that Dr. Morgan was going out to Caltech to start a division of biology there, he wrote him about me, and Dr. Morgan offered me a job.

MT: What were you working on in those days?

HB: Well, for my PhD I worked on the synthesis of protein. It was a subject that had always interested me, and in my last 15 years at Caltech I took it up again. When isotopes became available, it began to be really possible to study the synthesis of protein, which it really wasn't before. For my PhD I was working under difficulties; the system I studied was, as we now realize, an artificial system. It wasn't really one that normally operates in animals, plants, or bacteria.

MT: Were there many people back then working on this system?

HB: No, I was all alone in that field until isotopes became available.

MT: What did you know of Caltech?

HB: Nothing, except that it was a famous place for physics because Millikan was a famous man.

MT: Was Morgan personally interested in biochemistry?

HB: No, and apropos of that I think I might tell you a story. When Einstein came to Caltech, in 1931 or 1932, everybody wanted to meet him. But Morgan was a reticent person and didn't seek out people. So Einstein came to see Morgan, and they spent most of an afternoon together. After Einstein left, Morgan felt he had to talk to somebody about it so he came in and talked to me. The first thing Einstein said to him (and this is in answer to your question) was, "What in hell are you doing in a place like this?" And Morgan said, "Well, my belief is that the future of biology rests in the application of the methods and ideas of physics, chemistry, and mathematics." And Einstein shook his head and said, "No, that trick won't work. Look, even in physics we can handle only the very simplest molecules — hydrogen and helium and a few others. We can't do anything about organic chemistry. Do you really think you will ever be able to explain in terms of chemistry or physics so important a biological phenomenon as first love?" So I said to Morgan, "Well, what did you say to that one?" and he said, "Well, I tried to explain something about the connection between sense organs and the brain and hormones." And I said, "You didn't believe that yourself, did you?" And he said, "No, but I had to say something to him."

Morgan was like that. He was a witty person and he could pick up like that, quickly.

MT: You were the only biochemist?

HB: I was the only biochemist. Of course, he brought with him, as you know, his whole genetics group. But he felt that he had to have biochemistry, animal physiology, plant physiology, experimental embryology, and that's why he got me.

MT: Were there plans to expand the biochemistry?

HB: No, there were no plans to expand anything in biology. As budgets go, it was relatively small. It was nothing compared to physics or chemistry, for example. You see, the budget in those days was $75,000 a year. And so you can see how much would be spent on all the others. No. Morgan thought that was enough and, of course, time would tell.

MT: How closely did he follow the work that was being done by these different people?

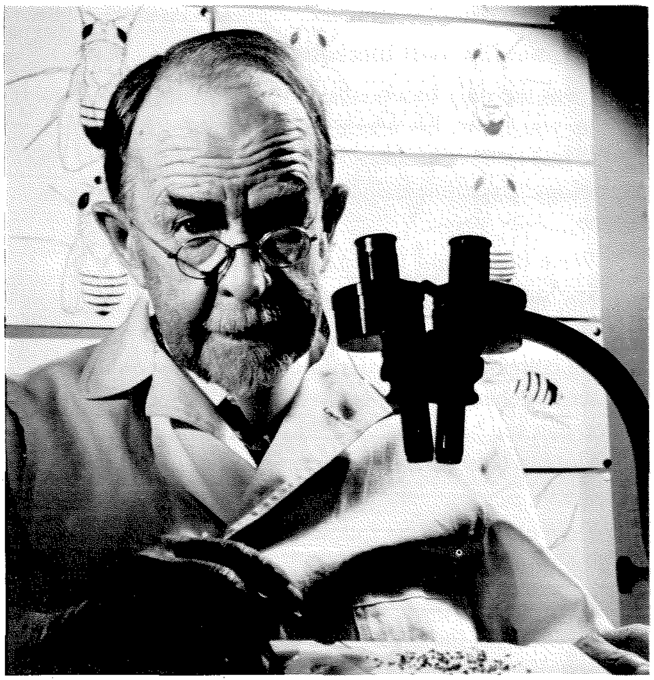
HB: He couldn't. He didn't have the background. Morgan really — he was the greatest biologist of his time and it's an interesting commentary that he knew very little chemistry, he knew very little physics, and he could only do the simplest statistics that he needed for genetics. I got to know him very well. We lived one block apart and I used to drop in often. I said to him once, "Look, why don't you take one of these Caltech graduates who are well schooled in physics, chemistry, and mathematics, to work with you for a year. They could do all these things that are hard for you to do, and it would be a wonderful experience for them." And he said, "No, my work isn't important enough." So he never did accept a graduate student. People would come to Caltech to do genetics, and they'd go to the other geneticists — Sturtevant or Emerson or Bridges or Schultz. But Morgan wouldn't have them with him.

Of course he could follow the genetics. But he couldn't really follow, except in its larger outlines, what the others of us — like in plant physiology, or I, or in animal physiology — were doing. But he was an extraordinarily intelligent person and even if he didn't know the details, he had a very sound judgment in the main about how valuable the work was. He made very few mistakes on that score. He was really a wise person.

MT: I guess part of what you're saying is that he was relatively old by the time he came to Caltech, and he had already done his major work.

HB: Well, Morgan's major work had been done between the years of 1911 and 1921. Before then he was doing experimental biology; that is, really, developmental biology.

Henry Borsook



Thomas Hunt Morgan

Actually he was the first professor of experimental biology in the world. No one had been appointed to such a position before. And then after 1921, as he told me, he had become bored with genetics. He said it was just algebra problems, so he went back to working on what he had worked on originally. He knew very well that it wasn't any way nearly as important, but that's what he was interested in and what he was going to do. So, let's see, how old was Morgan? Morgan came to Caltech in 1928.

MT: I think he was 62.

HB: Yes, and he died in 1946, when he was 81 or 82. Yes, so Morgan continued to work. He used to go every weekend to the marine biological lab at Corona del Mar and he worked during the week. He and I taught the first general course in biology to all students of science — it was a required course for all sophomores regardless of what science they were going to major in. He gave the first ten classes, and I took the rest. That pattern went on until 1935 when we thought it would be time to change and get somebody else.

MT: Was he a popular teacher?

'B: I don't know. I can't recall now any comment. But he s such an interesting and commanding person. And of se they knew he was a great biologist, and that's all as necessary. What made it particularly interesting is that Morgan's own career spanned the development of biology from Darwin to experimental biology, and he lived through that whole time as well as living through the time of the rediscovery of Mendel's work. I don't know whether someone has already told you this but Morgan, after he had a job at Bryn Mawr, used to go every summer to the marine biological station at Naples to carry on his own research. The teaching load at Bryn Mawr was too heavy to do much work there. At the end of the summer, first he would spend a week in Siena, and then he went to a friend, an amateur biologist in Basel who kept all the current journals in biology. And that's how Morgan caught up with what was going on, because at Bryn Mawr biology was a relatively backward subject. It was there that he learned about the rediscovery of Mendel, and this is how his interest in genetics was first aroused. But he didn't work at it until about 1911 and then he worked at it very hard, with Bridges and Sturtevant especially and with Muller, a pupil of his at that time. That's when the great development in modern genetics occurred — until recent times, with the relation of genetics to DNA and all of that, which Morgan didn't understand and had nothing to do with.

MT: Morgan was also on Caltech's Executive Council.

HB: Yes — right from the time he came. Naturally, I know nothing about that, but it was one of the remarkable features of Caltech that it was run with almost no administration. If anybody on the executive council wanted any information, he would never think of going through channels, but just call up the person who had the information. He could be a graduate student or a professor, and you would go over and talk to him. And that's the way Morgan was in relation to the administration of the biology division. I'm sure that his contributions to the proceedings of the Executive Council were important because he was such a wise person. He had such good sense, and he was quick to understand even things that he wasn't really schooled in, and I think they appreciated that.

MT: Was he easy to get along with personally?

HB: Yes, oh yes! But if you put on any dog or any pretense, he was very quick to puncture it, and sometimes he would do it anyhow. And this is where Millikan found it difficult to understand Morgan because Morgan was such a tease. Let me give you two stories about the relationship between Morgan and Millikan. At the time when the biology division was founded, Millikan had been writing a number of articles in the *Atlantic Monthly* on the relation of science to religion, and Millikan was making the point that there was no necessary conflict. In those days at Caltech there was a Friday morning assembly where all the undergraduates came and different people talked to them. One morning Morgan talked to them about biology as a career. He started out by saying, "Well, there's this kind of a job that you might be qualified for and so on, but that is of secondary importance in your taking a course in biology. The important thing when you take a course in biology is that you will lose a lot of superstitions." Millikan was sitting right in the front row, and Morgan said, "One of the superstitions that you will lose is that there is no conflict between science and religion." Everybody appreciated what was going on.

Another time the National Academy of Sciences was meeting in Pasadena. Morgan at that time was president of the National Academy and so he was presiding and papers were being presented in chemistry and biology. And he said, "Now I'm going to turn the chair over to Dr. Millikan because the next group of papers," and then he looked at Millikan and said, "are on celestial rays — and Millikan is a lot nearer to heaven than I am."

I like to talk about Morgan. He was a really important person — and a humble person. To finish this off, this side of Morgan, I had a book on vitamins published by the Viking Press *(Vitamins: What They Are and What They Will Do For You*. New York: The Viking Press, 1940). The president of Viking, Ben Huebsch, was coming out to see Upton Sinclair. (They published Upton Sinclair.) So we asked him to come to dinner, just himself. And I surmised that Ben Huebsch and the Morgans must have had a number of mutual friends in New York, so I thought it might be pleasant, and I called the Morgans up and said Huebsch was here and wouldn't they come up. So they came, and it developed that they did have a number of mutual friends, and they had a very good time together. Then the Morgans left, and Huebsch said to me, "We would like to publish Dr. Morgan's memoirs, so will you put this proposal to him? Let him choose a secretary and she could come and he could talk and she would type it up. He could check it over and have the secretary send us the bill, to make it as little bother as possible." I said I would, but I thought I had better speak to Mrs. Morgan first. And Mrs. Morgan shook her head. She said, "He won't do it, but I'll tell him anyhow." So a few days later we met in the corridor. (She was still working in genetics herself then.) And she saw me and she shook her head, no.

Well, Morgan was already getting on by then, and since I had medical training and I was a friend, I used to come in when he was ill to see what was up and what I could do. So I was with him in the hospital in his last illness. He sensed this was the end for him, but he was a brave man, and he was witty even though he knew he was dying. He said to me (by then, he called me by my first name, but I always called him Dr. Morgan; I couldn't do otherwise), and he said, "Henry, you get yourself a good secretary and you write my biography, but you must make me a promise that it won't be published for a hundred years."

And that was the only remark that he had ever made about the offer of Mr. Huebsch.

MT: What about the other geneticists, the younger people who had come with Morgan?

HB: The two principal ones were Sturtevant and Bridges and they were distinguished geneticsts in their own right. By the time they came to Caltech, they were working independently. Morgan wasn't working in genetics any more, but Morgan felt that he should continue the genetics group. A younger man who came along with them was Jack Schultz, and he also brought along Albert Tyler whose field was, like Morgan's, experimental embryology on invertebrate forms. Tyler kept on working on that but independently of Morgan. Morgan wouldn't work with anybody else and, of course, from the very beginning he insisted that Mrs. Morgan should work independently and not with him. She had learned genetics from him, but after that she worked by herself, and published by herself.

MT: Did the group of you have discussions about the direction the biology division should go in? Was there any discussion about changes?

HB: Each of us did what he liked, but we had certain teaching responsibilities. The geneticists divided up the genetics, I taught biochemistry, Wiersma and Van Harreveld taught animal physiology, Went taught plant physiology. That's the way it went, but that's all. And our staff meetings consisted really only of the approval of applications for graduate students. We all went over them, and we all had a say in who was chosen and who not, as well as new appointments.

MT: What about contact with faculty in other divisions?

HB: Oh well, that was one of the great features of Caltech when I first came. See, it was a community, and everybody knew everybody else. In those days we often had lunch together in what was called the Greasy Spoon before the Athenaeum was built. One of the things that struck me was that everybody was really intelligent, quite apart from their professional competence, with wide-ranging interests in other people's work and in politics or literature and in art. The chairman of the humanities division, Clinton Judy, who was very good, used to run once a month in his house a seminar open to everybody. Everybody took turns at giving a review of a book or an author, and there would be active discussion and so on, which is indicative of the smallness of the place and of the wide-ranging interests of everybody.

You probably have heard this story, but it's worth retelling. As an example, again, of the mutual interest in what everybody else was doing, Charlie Lauritsen had just finished building the first of what we used to call "the million volt X-ray tube," the high voltage X-ray tube. And that morning there had been a piece in the *Los Angeles Times* that Joliot in Paris had created artificial radioactivity. So we were talking about that at lunchtime and around the luncheon table a long telegram was drafted to be sent by Charlie to Joliot asking for more details, and the next day Joliot's reply came. That was discussed, and then that afternoon Lauritsen went and did an experiment to check up on Joliot. That's the kind of place Caltech was in those days. And although we all respected each other, there was no deference. Millikan was there, and if he'd get into an argument, he had to take his chances like anybody else, and it was the same all the way through. And Millikan often came. You sat down wherever you could. Of course when the Athenaeum came, with separate small tables, that relation was broken. It was really better in the old Greasy Spoon with a long table where you just sat down.

About the only administration that we knew was Ned Barrett, who was the comptroller, and he would come too and we all knew him. The administration was there to do things for us but one didn't sense it was administration. And this was one of the wonderful things about Caltech, this closely knit community — faculty, Executive Council, Board of Trustees. We all trusted each other and knew each other, and it went very harmoniously.

MT: Was there a reaction to having biology at the Institute as a new division?

HB: The reaction was that they were interested, and they wanted to know what it was about. They could easily understand. You see these people were interested in lots of things. They were interested in understanding, and sometimes they would drop in and talk to one another. We were friends, you see. They'd keep asking "why?" I must tell you in this connection — and it's indicative of the relation — about our daughter when she was very small. I used to walk to school with her, and like all young children she would always be asking questions. One day she said, "What does Mr. Millikan do?" So I said, "He's a physicist." And she said, "What's physics?" I began to talk about the relation between energy and matter, and she became impatient and she said, "Is it asking 'how' and 'what' and 'why' back and back and back?" And I said, "Yes. That's a good answer to all science."

MT: Can you tell me something about Morgan's style?

HB: Morgan was the first non-medical person to get the Nobel Prize in medicine. I happened to drop in one Sunday at Corona del Mar and watched him working. He was working on a certain invertebrate that produces both eggs and sperm and yet they could not fertilize themselves; the sperm of one animal could only fertilize the egg of another. He had found out that if he suspended the eggs in an acid solution, it would break down their resistance to self-fertilization. He told me the acid did not always work. And how was he making the acid solution? He had an eye dropper and a dish of seawater, and he would drop a certain number of drops from the eye dropper into this dish of sea water. So I said, "Well no wonder it doesn't always work." I didn't say anything but went back to the lab and made him a set of standards, so he could measure the acidity colorimetrically. I brought the whole set to him, and he said, "Goodness gracious! Nobody has ever done this to me before in my whole life." I thought he might be offended, because I was interfering in his affairs, so I said, "Well, really, Dr. Morgan, you know yourself you were getting variable responses, and that's the reason, but if you will use this, then you will know." And then the following week he came down — my lab was in the basement then — and he said, "If you will promise me that it won't interfere at all with your work, I would like another set of those standards."

MT: He didn't want to impose on you.

HB: Of course. He was most diffident about that. And that was when I asked him why didn't he take one of our young graduates and, of course, what I did would be nothing for them. But he wouldn't do it. But children often would come into the lab — his door was always open. Of course, they would come in and want to know what he was doing, and he would lift them up on his lap and have them look through the microscope and show them. He was wonderful with children.

MT: What about Millikan? Was he interested particularly in biology?

HB: No. When I first came, of course, I was introduced to him and he said he thought physics was finished and that the future of science was going to be in biology. He wasn't interested in the details of what anyone was doing except as, shall we say, a statesman of science. That was his interest in biology. Of course he was wrong, but so was many another. They couldn't see the future of things.

MT: When you came to the Institute in 1929, what were you working on?

continued on page 34

Henry Borsook . . . *continued from page 32*

HB: I felt I had to leave behind what I was doing in Toronto and knowing how strong the Institute was in chemistry and physics, I began to teach myself the kind of chemistry that the undergraduates were getting. They knew much more chemistry than I did, especially physical chemistry. I was interested especially in thermodynamics, and they were very strong in that. And I began to apply thermodynamics to biological phenomena, which turned out to be very interesting.

Before then, the concept of an animal organism was that it was like an engine; it burned the fuel that you poured into it, and there was only a minimal amount of wear and tear. But by using thermodynamic data, I was able to show first that this wasn't so — that even the waste products, which you would think would just be degradation products, weren't that at all. They were synthetic products. There was urea, the chief waste product of nitrogen, that took a good deal of energy to build up. Before then it was taught that when protein was broken down to amino acids — there are 21 amino acids — then when they built up, the reaction was reversed. Well, I was able to show from thermodynamic data that this was impossible. It was too far uphill. To rebuild them into protein you had to put energy into the system. You had to couple an energy-donating system, like burning sugar or something like that, with the synthetic apparatus; and it was entirely different from the breakdown process.

Now the important thing about this was that it removed from physiological thinking what we call teleology. The organism was not a machine, and we were able to show that 55 percent of the urea that was daily excreted came from the breakdown of one's own tissue protein. So proteins were continually breaking down and continually being rebuilt, and this was a much more biological concept.

MT: When you were doing this work — applying thermodynamics to biology — was this being done also at other places?

HB: No, I was the only person doing it. It's not a virtue, but if anybody began to work in something I was working at, I would drop it and turn to something else. The big advances in all science are made in the fashionable branches because lots of people are working at it, but I couldn't do that. I have to do my own things in my way. Maybe it was an amateur's way of looking at science rather than a professional's, but that's the way I was.

This thing that I did — using thermodynamics and certain experimental devices was in 1932-1933, published in 1933. I had no isotopes then, and it was only in 1939, when isotopes became available, that my idea was proved.

MT: What kind of contact did you have with other people working in biochemistry around the country? Did you travel to meetings?

HB: Yes, but it was only afterwards that I realized that to be financially able to travel to meetings I, like all the other people at Caltech, owed it to Millikan. You know about that, that all of the fees he got for lectures was our travel fund. We didn't know that.

MT: What about funding when you first came?

HB: It just came out of the Institute budget.

MT: It was just for your salary?

HB: My salary and whatever expenses the work called for. There was no funding at all. It was only after the war that government funding came into it.

MT: So you didn't do any application for outside funds before the war?

HB: No, that was discouraged at Caltech. And I must say we didn't feel the need of it. If we needed equipment, we built it instead of buying it.

MT: What about salaries for, say, research staff, lab assistants?

HB: Salaries were low. I had one assistant and a couple of graduate students. The graduate students worked on their own, and it was understood that I would not put my name on any piece of work done by a graduate student, even though I may have told him what to do and guided him. That is, whatever I put my name to I had done the work myself. I don't think I was alone in this respect, in the biology division at any rate. I don't know about the other places. But I never felt the lack of money. I may have felt the lack of ideas, but not of money.

MT: I know that some private foundations gave Caltech money before the war, like the Rockefeller Foundation.

HB: Well, they were famous people and we weren't. And I think biological research, biochemical research, wasn't in those days the kind of thing that attracted money. I didn't really need it. Teaching wasn't heavy. I just taught for one term, and there was a seminar, of course. And so with one assistant — and I had a very good assistant, Jacob Dubnoff, for about ten years — we could do all that we wanted to do. Everybody else was working by himself — even Morgan was doing his own work and washing his own dishes, let alone the other people. So this was the style of the place. □

Part Two of Dr. Borsook's recollections of his years at Caltech will appear in our next issue.