PROFESSOR EDWIN GRANT CONKLIN

Dr. Elsa Keil Sichel: The introduction to this tape is found at the end of the conversation.

Dr. Conklin: but I have my doubts. I'm going to try it tonight. Sit on the bed (various other voices--Parpart, Fankhauser and Isabel Conklin were present.)

You know I've been for several years the senior member of the National Academy of Sciences and I've been next to one old fellow who won't die (?Liberty Hyde Bailey?), I've been the oldest member, the senior member, of the Philosophical Society, which is a much more difficult thing to attain because philosophers never die. And I've often wondered why I came to attention and why people took any notice of what I have been doing. Well, in Philadelphia, it was very easy. I've told the story of how Dr. Pepper, the elder, who had been the Provost of the University, met me--was introduced by Cope to me and told me how they were going to have a symposium and that I had been selected (I had just come to the University only a month or six weeks before) and that they wanted me to take one of the principal parts. And I was flabbergasted! I said I couldn't do it. But he urged me; he said, well we can do what we have to do. Then I begged off and said I was so busy. "Oh, well," he said, "we had hoped to get acquainted with you." I said to him, "Well, if that's what you have in mind, I can't turn it down." And so I took it. And that introduced me to the Philosophical Society--very favorably, for if I do say it, I had the only modern paper on the program. It was a paper that had been built around all that we had learned at Baltimore about the fact that evolution doesn't take place between adult organisms. An adult dog or cat doesn't give rise to an adult animal of another species; it has to go down through the valley of the germ cells and come up in that way. And that was my theme.

It was not Weismannism and it was novel at that time in 1896. Well, it was published and I had many very congratulatory letters. And I had worked on it like everything. David Starr Jordan, begged to have it as a chapter in a book of his on evolution. Of course I was glad to have him use it. Charles S Minot, of Harvard, and others wrote me how very much they were pleased. E. B. Wilson wrote, and all that. Well, that got me started, got me started very philosophically.

In the meantime, I had been working, of course, since 1890 on the embryology of Crepidula, which was an animal that didn't amount to a damn, and why anyone should be interested in. But Brooks had just turned me loose; told me to do things which were impossible: study at Woods Hole that group of hydroids, the siphonophores--study siphonophores. I had studied some siphonophores at Baltimore. But Brooks had received from Alexander Agassiz . Brooks didn't know what they were. He handed them to me; he said, "This is either jellyfish or fish jelly. Find out what it is." Well, I hadn't anything to do; I was crazy for a subject. I had to get through! I had to earn a Doctor's degree; I was in debt; I just had to get through! I had only one more year to go. I went to Woods Hole and I occupied the Johns Hopkins table at the Fish Commission.

I went in to the supper table. H. V. Wilson sat at the head of the table; he was in charge of the scientific work. Morgan and Wilson and all the older fellows--Patten and --they sat around the table.

He said (Wilson, H.V.), "Well, Conklin, what are you going to work on?"

Well, I said, "Brooks sent me here with the idea of working on siphonophores."

"What!" they all shouted. "Siphonophores? Why, there's only one siphonophore ever found here; that's the Portuguese Man-O'-War. And you won't find that more than once in the summer. Brooks has just turned you loose."

And that's exactly what he had done. He hadn't given me any hint as to what I could do. Well, I wanted to find some embryological subject. I hunted around for all sorts of things. I found *Crepidula* had a great many eggs, nice looking. I looked at goose barnacles; and they were breeding. And goose barnacles are very beautiful things. It was a question whether that would be a better subject for working with the cells, the nuclei and the cellular phenomena. I was specially interested in that. *Crepidula* turned out to be more favorable for what I wanted and I followed individual cells through the development. I followed them until many people laughed about it--called it cellular book-keeping. That's what it was, really. But it showed that these individual cells had definite fates. They went into certain parts, anywhere and everywhere. We'd always been discussing the question of the origin of the mesoderm: where does the mesoderm come from?

The earliest differentiations were said to be the germ layers, the germinal layers--ectoderm, endoderm, mesoderm. Ectoderm and endoderm were pretty definite; we knew about where they came from, in general and yet we never did trace them back to the unsegmented egg. I worked on that and finally found that three groups of cortex--

the egg was divided into cortex, and each one gave off little cells. And there were three sets of these that came off, making twelve cells, all with ectoderm. The animal came from those twelve cells. Well, that was new. Then I went on and found that (there were) all the endoderm came from the four bit cells; and that was definite. Then the mesoderm came in between the two as a single cell, the single cell 4-D it was called later, of course. That was not my name; that's a name that half a dozen people had part in making.

But the idea was mine, that the mesoderm came from a single cell. Well, I thought that was important; and so did the graduate students. But Brooks didn't. Brooks looked at it and he brought the thesis back. I had written it out at great labor. Mrs. Conklin--I was married, this was my last year at Johns Hopkins--was my amanuensis. She wrote it all out for me as I dictated it. I submitted it all to Brooks, with the plates. I had eight or ten plates made with pen and ink. Brooks took it home with him, kept it for several days, brought it back.

"Well," he said, "Conklin, I don't know what to say about this. I don't know where in the world you can ever print this. There's no place; we have no money--Morgan has used up all the money allotted to the biology department in his work on "
He said, "We've got no money; we can't help you in printing it and we've got to have a hundred copies of your thesis. I don't know where in the world you can print it. "Furthermore," he said, "I don't know that it's worth printing. There is no morphological importance in the mere duplication of parts. Cell division is merely a duplication of parts and there's no morphological significance in it." That was his thesis.

Well, the result was that he came back to see me after several interviews in which there were always a half dozen students in the room. We didn't have individual rooms; we were all thrown together our graduate students; and finally he came back and said, "Well, Conklin, I have decided that this university has given Doctor's degrees for counting words, in ancient languages for counting words. I don't know why they might not give one degree for counting cells."

Well, ?that ended that and it was accepted for a Doctoral thesis and a preliminary note did appear at that time.

(Gap in tape here)

. . . . University and I sent a copy to the Anatomische Anzeicher (?) which published it on the early cleavage of the egg of Crepidula, with this definite separation of the germinal layer material into these few cells.

Well, it was better regarded by the public than by Brooks. I went back to Woods Hole in 1891 to an assigned table at the Fish Commission to continue the work, to a later stage of the development. Well, the first few weeks at Woods Hole--I was wrong in saying I had met E. B. Wilson that previous summer--I hadn't. I had never met Wilson until my second summer at Woods Hole. And Watase, who was a graduate student at the Johns Hopkins and who knew about my work, told Wilson of what I was doing. Well, Wilson was intensely interested. He sent word over asking to make an appointment for the next Sunday morning. On Sunday morning next, he came over with his drawings of the cell lineage of the Nereis. Of course, it wasn't called that; the cell lineage of Nereis was one of the later things--and in the development of the paper. Well, I'd never (?or I've never) spent such a forenoon in my life as I did that day with Wilson!

We went over these things: the whole of the ectoderm in twelve cells, the same in _Nereis_, but in _Crepidula_ spread out on a plate; in _Nereis_ in a sphere, much more difficult to follow. So much clearer, plainer, in _Crepidula_--everything out on a plate. Finally, the mesoderm in one cell (the 4-D cell)--well, that simply capped the climax! And then, of course, we were following the individual ectomeres, the various cells, as far as we could.

Wilson had just been appointed to his position at Columbia University. He had been for five years, Professor of Biology at Johns Hopkins University--oh, no, at Bryn Mawr College. He was greatly admired there as a teacher; he was a prince of teachers. He had just gone to Columbia, with the understanding that his first year was to be spent in Europe. I had made arrangements just before I got my Doctor's degree to go out to the Ohio-Wesleyan University, my old college where I had graduated six years before and had taught for three years in the interval, and then had spent three years at the Johns Hopkins. I had made arrangements to go there and start a laboratory; they had no laboratory of biology. I had to equip everything: take a bare room, get tables, to put in sinks and lockers and all that sort of thing. And I had to organize (I didn't have to, but I thought I ought to)--I had a great idea of getting things started in a pretty good way. And I had advanced students, students who knew a great deal of biology, especially ?Harold Heath, who was the finest sort of an assistant. I had to provide advanced courses for him in zoology, and for half a dozen others. The result of it was that to fit my work into the old college curriculum, to which there had been added not a single professor in more than twenty years--I was the first man in twenty years to be added to the faculty as a professor.

Well, everything was terribly backward. All the hard work! And that was my first duty. I worked at it as hard as any man ever worked at anything. I got the work ~~rolling~~ going, going well. But by going back to Woods Hole, I kept in touch with research and the second summer and the contact with Wilson brought me in contact with Whitman. Whitman had me in for a half day. He solved one of my problems at once. He said, "I want that for the Journal of Morphology, which was the finestpublication in morphology in the world at that time. My goodness! I took my breath! He said, "I want you to do that work over, making it as perfect as it can be made. Don't spare the time or the labor. We'll print it and we'll give you the finest lithographic illustrations."

Well, I'll tell you what! I worked at that and the result was that the whole of that summer of '91, when Wilson and I were both at Woods Hole, I was getting my paper into shape. And during the following winter and spring; and in 1892 the work went ahead and grew and grew and grew. I was afraid that it would have been turned down; it was bigger than it need have been, there's just no question, it could have been cut down. But it turned out to be some 240 or 250 printed pages in length, with nine lithographich plates in three or four colors. It was a book, literally a book, occupying a full number of the Journal of Morphology, and was one of the things that caused the collapse of the Journal! (laughter)

Then the question was to get it printed.
In spite of the fact that the plates were all made by ?Werner and Winter
(I don't know that they were made before 1893) but about that time they were made and I had the proofs. But Ginn and Company said they would lose two thousand dollars on printing that number and they couldn't afford it. In the meantime, I did everything I could to increase the subscriptions to the Journal of Morphology and to