

11522 Tulane Ave.
Riverside, CA 92507
Jan. 28, 1973

Dear Marguerite and Josh,

This letter has long been in mind, to thank you for the delicious persimmon pudding, which we consumed promptly. The apricot preserve lasted 'til Sally's arrival. She and I agreed it was the freshest tasting fruit preserve we'd ever had. That's a wonderful way to make it. And we thoroly enjoyed seeing you three, in case you think I only remember the food, not the companionship!

John may have mentioned in an earlier letter, when thanking you for your various booklets, etc., that Sally arrived with the London flu - 1030 fever the next morning. The only good thing about it was that she extended her visit by a week, to make up for her first part. John got it from her, thought it was his sinuses acting up, so he neglected to take care of himself, ending up with viral pneumonia. This past week for the first time he has felt like himself, but he still tires rapidly.

I had not heard your conversation with John about when did DNA come to Columbia, or who brought it to the Zoology Department, but I am sure it was Arthur Wagg Pollister. He worked with Mirsky, isolating nucleohistone, but that was before I got interested in microspectrophotometry of DNA (about 1949 I guess I learned the method), so all I ever knew was that he and Mirsky had had a falling out; and that was never discussed by AWP, tho I worked on his grants from 1955-65. But it had to be in the late '40's that I have this vivid memory of Arthur standing at his lab table with a beaker containing fibres wrapped about a glass rod and asking him what it was, and he saying it was ~~xxx~~ desoxyribonucleoprotein! and I thought it most exciting.

So far as I know with my first contact with AWP's apparatus for measuring DNA ~~it was~~ it was thoroly understood that DNA was the genetic material and should be constant except for ploidy classes and during replication at interphase before mitosis.

I am mailing to you a reprint of mine ^{intrinsic}, not because of its value, but because it has historical value. It was in 1947 that AWP's method for measuring DNA was published. The constancy of DNA was a religion with Swift in 1950. In this paper I challenged that position, so I was something of a heretic in Arthur's lab. You will notice that I have no mention of Avery et al, which surprises me, because I knew that work well in 1952, as I remember when I gave a talk at the University of Sydney that year we were there I discussed their work. However, you will note that Boivin, Vendrely and Vendrely are referred to. I remember that Mirsky reproached me via John that I had not referred to his similar results; but in the literature Mirsky's data ^{were} published a year later than the Vendrelys'. I think of the work as the Vendrelys, who visited Arthur very early in the work, and remained very good friends of his into the 60's.

Chromozoma 4: 563-76 52
not reviewed
[I did more DNA measurements, in diploid & haploid hybrids, 1957 J. Morph. 101, p. 227, where again I had variable amounts.]

I have a reprint of Pollister, Swift and Alfert, 1951, J. Cell. and Comp. Physiol 38, p. 101, in which they refer to Avery et al of '44 and Boivin et al '45, which confirms my impression I well knew of Avery's work.

Josh, you ought to write to Pollister. His address is

Box 456
Port Norris, N.J. 08349

He never goes into Columbia since his retirement. He spends sometime in Dixfield, Maine 04224.

You would get some fascinating information out of him, if he would respond. Arthur has a bumbling manner, which may be misleading, because I think he contributed enormously to biology, with a minimum of recognition. He is just marvelous with apparatus and optics - take his microspectrophotometric apparatus, modifications of which are in worldwide use. And he knew the problems that could be solved with such an apparatus. And his mathematical ability is enormous. He was one of the first to work with the electron microscope, actually he described nuclear pores first, just at a meeting and in an abstract, but that has been lost from the literature; not that it matters. And he has had a distinguished group of students. And he always maintained the Golgi to be a real entity, even published on it when it was most unfashionable. He had a fascinating paper on autoradiography, introducing a correction factor re grain counts due to self-absorption of tritium radiation, in 1965. Lima-de-Faria, the cytologist at Lund, mentioned to me what an outstanding paper it was ~~xxx~~ and at that time in a discussion of Pollister's work said that he thought AWP had made some extraordinary contributions to cytology. *And AWP was an excellent chemist and developed cytochemistry.*

Also in my memorabilia of DNA I have a folder with lecture notes from ~~me~~ a course on nucleic acids given by Chargaff. I sat in on the course, the notes have the month and day of each lecture, but no year! It was the first time Chargaff gave such a course; I think it was the only one in the world at that time - on nucleoproteins. You might check with him as to when he did that. It was in the fall semester, and on the Columbia campus, not at PandS.

We never did find your glove, or see it. So I'm going to use that envelope to mail you these items.

Lovely weather here now. We have a pink flowering peach which is just about to burst into bloom. Come see us again.

Affectionate greetings,

Betty

FEB 2 1973