

THE PAPEZ MEMORABILIA - TUSCON, ARIZONA

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Initial Discussion regarding Fred Johnson

K.E.L. - When were you at Cornell?

J.A. - 1949-1956 I began my graduate studies there in embryology and histology under the direction of Professor H. B. Adelleman who is still alive. Howard Adelleman is a great embryologist and later on in his life a historian of embryology.

K.E.L. - Was he a Neuroanatomist?

J.A. - No - only secondarily. He worked on the development of the eye, the optic fields of various vertebraes, on the production of ---
-----with different salts and saline. He could produce -----
-----, he worked on the development of extra ocular muscles, he did a lot of work deterring the embryonic field.

K.E.L. - You started with him 1949?

J.A. - Yes and worked with him for two years and got a Masters degree in histology.

K.E.L. - Didn't Papez leave Cornell about that time?

J.A. - Yes - that was exactly the time he was leaving. In the Spring of my first year (1950) I took comparative neurology from Papez and that was the last time he ever gave that course. That was part of my course requirements. It was the Department of Zoology. You could choose from ? ecology, verbracology, vertebrae zoology, endocrinolgy, histology, embrology, neuroanatomy, and physiology. Those were the courses, so I put together my program mostly from the morphological sciences. I didn't take the physiology. I did take endocrinology. As soon as I heard about Papez's course I signed up for that - it was a 6 month course.

I got a very good grade in it, a straight A. I think I mentioned it in my little essay. I don't think I deserved it. We believed and this was somewhat-----admitted by Fred Johnson, Glenn Russell and Dr. Papez greatness by throwing the papers down the stairwell seeing which one flew the longest distance. If that were the case, the lightest essays would perhaps travel the farthest. I never did know how he arrived at our grades.

K.E.L. - How many people would be in that class?

J.A. - There must have been 30 people. Papez was the lecturer. He gave most but definitely not all the lectures. I remember lectures given by Fred Mettler and Glenn Russell. I don't want to say there was a secrecy in the course but I want to give you the picture that there were perhaps a dozen of us as graduate assistants in that building. Myself and 5 others in histology and embrology, some from endocrinology and physiology and the two from comparative neurology, Russell and Johnson. We frequently ate together, had parties and saw each other all day long and most of the night. We shared a lot of our experiences in the different courses, family experiences, hopes and fears but were never able to penetrate what was going on with Fred and Glenn and Dr. Papez. Glenn taught in histology an embrology with me and he knew what was going on with our courses. I always had the feeling that they were under some kind of agreement that they wouldn't talk about what they did. Now, maybe that is just a marginal (?) feeling on my part. They were very specialized in neuroanatomy and Papez had a clinical anatomical approach that was totally out of step with the rest of the department. Most of the Professors around the building that I knew were grumbling, wondering why he was still around, why he hadn't gone down state to Cornell with the rest of them. For some reason either the nature of the subject, his approach to it, which was clinical, or maybe in someway because it was part and parcel of that certain vagueness that Paul MacLean described. We didn't know anything about what he and those two boys were doing on that third floor except as we found out in the context of

the course. Glenn and Fred gave a few lectures but Papez gave most of them. As I recall we followed pretty much a level approach rather than a systems approach. We worked our way slowly northward from the sacral cord into the basal ganglion. We did have some system treatment but they come only secondarily. We stopped to talk for awhile about the visual system. Since I think this reflected the organization of Mettler's textbook because if you go through Mettler's textbook you see the splendid detail which works its way upward to the brainstem and it only has short little segments of chapters devoted to the various sub-systems. It definitely was not a systemic approach.

K.E.L. - Where did Mettler's thing come in? That was obviously before you were there.

J.A. - Mettler's textbook was written in 1947 or '48 and I took the course in the academic year '49. I remember buying the book. I bought that book and Papez's comparative neuroanatomy. I have a copy of that here now. There is not too many of those around any more. I mentioned in this article some of my reminiscences. Will you draw up on those? There are a lot of things.

K.E.L. - Do you want to put that into the context of what you are saying? What did the students think about the course?

J.A. - We really couldn't figure him out (Papez). He was an enigma to us. Many of the things that he said were inaudible. He whispered and also he said things in conspiratorial tones. I put in my reminiscences here that when we got to the diencephalon he was always talking about the Bundle of Vicq d'Azere. We never spelled that name on the blackboard and we had no idea it was Vicq. I was convinced that it was some Italian person that was a graduate student or professor at some other university. I was intimidated by my colleagues and didn't dare put up my hand - "who is Vicq d'Azere? Does he live in Utica?"

K.E.L. - Was he rather formidable that people didn't ask him questions?

J.A. - Well, if you asked him questions he would seem not to hear for while. So those are some things that I remember but an important point is that we never got angry at him. We were so frustrated but never got angry, never lost our respect for him. One of the things right off the bat that kept us respectful was his gentleness because he was courteous. He would always speak to you in the hall and smile even if he didn't know your name. He had a really benevolent smile, sort of like Santa Claus and his name ^{Wenclaus} Wenclaus made everyone think of the Christmas Season. I used to think if we put a beard and a red suit on him he would be perfect because he was just the soul of ^{benevolence} benign outlook (?) I was aware from some things I heard professors say that generally he wasn't welcomed in the department. He wasn't part of the main stream of research on vertebrae or invertebrae or ecology. He really belonged with the doctors, and also there wasn't anybody else in the department that understood anything about neuroanatomy. One of the reasons I was attracted to neuroanatomy was that it was so challenging. There were so many things to learn and I felt it was a challenge and I liked to be able to tell people that I was a neuro-anatomist although I had not yet gotten to be one. It put me up a bit in the peck order among my friends. You people just look at the kidney or the liver but I look at the brain. So the lectures were probably organized but he never used notes that I can remember in front of the lectern. He showed lots of slides and in almost every lecture, he felt the material he was showing wasn't adequate in order to illustrate a certain point, so he would disappear into the next room, into his office and get another slide to make a point. What was so interesting about this is that he never stopped lecturing. He would go on talking and we would hear his voice fade out in the auditorium. It was one of those old auditoriums where the seats are naked (?) - they are very uncomfortable, bench-like seats. I found as a graduate student you could sleep under them when you were tired. It was made specifically to view pro (?) sections so even if you were at the very top you had a good view straight down to the body. So many times I sat in back and looked down at his bald head and hear him leave. He would put his wooden pointer of 3-4 foot on his shoulder like a soldier and march out. With that over his shoulder

you could hear him lecturing in the other room, you could just hear the voice. You couldn't make out any words, rather just the sound of slides being picked up and put down, drawers opening and closing and finally you would hear his voice get louder and louder, and he would come back in - miles down the road from his talk. There was a gap. The other thing that he used to do was that he would always innumerate the number of aspects to a discussion and then inevitably come up with one short or one too many, and he would say - there were 4 aspects (in a hushed tone). First you have, 2nd, 3rd - we would exchange knowing looks - he would either stop and run out of gas in which case we would try to suppress our laughter or he would go on to a 5th or 6th and often this was combined with the walking out. So you would have him walking out and we would try to guess what number - at what point he would come back on. The course went on like that. He made some drawings on the board but they were very difficult to figure out. He showed many slides - these were stained slides of the brain and there he had magnificent things to show - coronal, sagittal and horizontal sections. They weren't whole brain sections. In most cases they didn't have the technique to do that. He had a technician by the way devoted to him. Glenn can tell you her name. I used to know it but maybe I'll think of it later this afternoon - but she made all his material. I have never seen better material. The only thing that you can criticize about it is that she had to trim the blocks.

K.E.L. -

J.A. - I think she did.

K.E.L. - You know some of the drawings we have on just paper look as though they were drawn as explanations for something you saw on the slide.

J.A. - Yes - that is what he would do. For example, he would show you sagittal sections of the basal ganglion and talk to you about the -----and then he would hold up these very large charts. They were about 2-3 feet wide and 3-4 feet high and they were in 4 or 5 colors. There was one on the extrapyridimal system, the visual system. One on every major subsystem. I don't remember one on anything.

K.E.L. - Were they sort of crude drawings that he made?

J.A. - No - they were beautifully polished. They were ^{? Pearls} pearls. They were just as well done as the famous Caper series, the -----, the typical amp reptile and so forth. He had those hanging around. But these were magnificent. There was some sort of cardboard backing so that he could stand them up on the eraser track of the blackboard and point to them with his pointer and discuss them, but always these lectures left many questions unanswered which was tremendous concern on our part. We knew we had to cough this back up when the examination came around. He didn't spare anything. We were asked the most intricate detail of the extrapyramidal pathways as things were known then. He never did get into cell articulations, I don't think. The wiring diagrams of the cerebellar cortex. We knew there were----- cells and gradular cells or fibres but nobody had any kind of high resolution-----as we have today. But after each lecture we would then go into the lab - some days we looked at gross material, some days we looked at demonstrations. The demonstrations were marvelous, Russell and Johnson would set up all kinds of specialized preparations, to show features of neurons and glial cells. There were golgi preparations and silver one, modifications to show astrocytes, and then usually we would have a 45 minute-1 hour slide review and sometimes we did this en masse and we would all go back in the lecture hall. These were almost always run by Russell or Johnson. Papez would be in his office and sometimes came out to see that things were going on correctly but he usually left us to the graduate students. Sometimes we broke into two groups - Russell would take in one group and Johnson the other. We

tried to guess which one of the two we would learn more from. But in addition to teaching us more material, these slide reviews served as an important communication function. We just were not getting certain things from Papez. What is important? Why are we learning this? Where is the thalamus anyway? None of us could get a satisfactory answer from Papez. He would point to the mesial surface of the thalamus in a sagittally sectioned brain but he didn't seem to be able to give us any concept of where the lateral surface of it was so we would ask Johnson and Russell to explain these things for us. Very often, they were quite helpful. In addition, what I am trying to say is that there was an essential----- . I think our morale would have suffered greatly if we didn't have a chance to level and rap with these two graduate assistants. We would air our complaints - why do we have to learn this? Why did he show us those things? This is too much, we can't learn it. Glenn would try to appease us and re-interpret.

I think these two students did an excellent job in making it an effective course. That also explains a little but of the closed shop aspect. They knew that he had delegated heavy responsibilities to them and they were very loyal to him. They would have weekly meetings with him and go into his office and the doors would be shut. Never did they ever tell us anything about them.

K.E.L. - Apparently he didn't communicate with other peers in the faculty.

J.A. - Yes - some were quite rude to him. Wally ----- tells me years later that he went to Cornell to give an invited lecture in the neuro-biology program and he spoke about Papez and Papez's contribution. At the end of the lecture Professor Perry W. Gilbert, a comparative new anatomist at Cornell came up with tears streaming out of his eyes. He had never realized what a great man they had had among them. I knew Perry Gilbert very well and I don't think - I can't say for sure, Perry was a gentleman, did anything directly to Papez but he may have very well echoed the feelings of some of the more aggressive. He probably fell into line. I imagine he just didn't speak to Papez just to keep the company line. I know of no professor in the department that had anything to do with him. He was completely isolated on the third floor at the last end.

K.E.L. -

J.A. - No, because we couldn't hear him sometimes or couldn't understand him or when we could his answers would be just a can or worms. We would get more than we really wanted. I tried for a long time just to get the fields of Torel straight. These were always being defined in terms of the----- . I do the same thing myself with my students. I am a little more clear telling them why I am doing these other structures.

K.E.L. - A lot of that is not really definable, in overlapping areas.

J.A. - I mentioned in my references that he had no idea where many of these fibres went. These were just hunches based on a study of normal myelin sections or -----method. He made much of the-----method but he never had seen any of the more advanced techniques such as we have today. Heaven forbid that tritiated protein or HRP studies. I am sure he would have been very interested in that work.

K.E.L. - But he made a lot of-----He used his intuition.

J.A. - I mentioned in my little article for the symposium that one of the high points, one of the things we loved about the course was Papez's simulation of disease states. I remember he acted out strokes, Huntington's, Parkinsons. The Parkinson was extraordinary, and some of the really odd dyskinesia's. I don't know if he did the-----but he did things of that level.

K.E.L. - That was a lot of clinical insight.

J.A. - Oh, yes, nobody could come near that. This was great - it was very exciting and it made us really love the course even though we didn't understand everything. It was something to look forward to, but then we would always giggle or laugh. The state that he simulated would persist for a long time. He could go into it almost immediately. He would say - so now you have a person with poverty of movement - then

he would fall into the shuffle. She is a Parkinson - that is the way they are. But he didn't pop out of it - he just kept on that way and he stuck in it for the rest of the lecture and I wrote in my reminiscences that I didn't (television show hadn't appeared by that time). Looking back now I would want to say and now will the real or normal Dr. Papez please stand up. But he would have us in hysterics and sometimes it would seem to last the rest of the day or even the rest of the week. We would see him shuffling around. He would get caught in one of his teachings, so that was a lot of fun.

K.E.L. - Did you have any one to one with him?

J.A. - Well, after the course was over, I got an "A", but I know that these were just not automatic "A's", there was a pretty good bell shaped curve. Perhaps Russell and Johnson saw to that. But then after the summer, not long after the course ended, we took six months of dissection - human. I was really one of the three of the last students ever to work with him. Dr. Nicolas J. Gerold, was a graduate student in histology and for many years he was head of Biology at Hamilton College in Clinton, New York. He is still there but retired. So Nick Gerold and myself and the 3rd person was John Robert Troyer who is now Professor and Chairman of Anatomy at Temple University. So Bob is someone you ought to talk to. The three of us, Gerold, Troyer and myself dissected a human cadaver for six months with Papez coming up usually once a day. We dissected in the ^{Burr} Bert Wilder Collection Room. It is impossible to describe where that room was, down into the basement through the animal room and stockroom. There was a little door and if you went in there, there were drums of formaldehyde and all kinds of things of that sort. There was a small door and then a ladder that looked as if it led up to the sides of a ship. You climbed up the steel vertical ladder to this little room where there were all sorts of jars filled with brain and that was the Wilder Collection. Papez frequently went down to check on the collection - making sure the specimens didn't dry out and properly capped and so forth. So we had a cadaver which was well preserved but terribly emaciated. It was one

of the cadavers that Cornell University Medical College didn't see fit to transport. We did some searching among the few cadavers that they had but they were all bad and we picked the least unsuitable. It was a difficult body to dissect. It was all air and the brachial and lumbar sacral plexus were perhaps easier to figure out because the muscles were so atrophied, but we dissected that body. We did the whole body. I don't recall the recommended text but he just told us we could use what we wanted. I suspect we used Gray but I don't really remember. We tried not to take our textbooks up there because they would spoil by getting grease or oil on them. He had some typewritten notes of things we were to look for but mostly it was just verbal. Perhaps on Monday he would tell us what he wanted us to do and then keep coming up during the week - 3 or 4 times to see how we were getting along and he always would pitch in and dissect with us and I remember one time he would get very enthusiastic about it, this same business of this ? vagueness and the conspiratorial tone would come out and occasionally to make a point, he would draw in the grease. This was amazing because even though it was a thin body it was very fatty. He would draw relationships on the table top in grease. I will never forget this, to the horror of my friend Nick Gerold, who had a brand new clean white lab coat, Dr. Papez drew with a -----encrusted forefinger some relationship on Nick's breast. The rest of us were sitting there trying to stuff whatever rag into our mouths to keep from laughing. I remember we did an awful lot of laughing behind his back and I'm sorry for that now. I was only 21 years old and wasn't grown up. Yet even then, I knew I was in the presence of a great man. There is no question about it. We looked forward to his visits. He gave so generously of his time to us I can remember it as clearly today as then. It was very memorable. The content of it has slipped away now but the affect of it remains. He really cared about us, but he would never tell you that. It all came out secondarily. We realized that this man was putting in a lot of time - teaching us and we were all so anxious not to disappoint him. When he came down, we made him think it was all a lark and we were just trying to satisfy our course requirements, but it was very unusual.

K.E.L. - Did he ever talk to you about what he thought he might do or what your interests were?

J.A. - No - I don't really think so. We seemed to be more or less unlocked in time. A kind of here and now. This is what we are going to do today. He was always anxious to get right to the material and he would get lost in that. I don't remember any long range views about career development or any looking back - like, where did you come from young man and where did you have this before? Never any criticism - why haven't you done this very well? I don't remember a single word of criticism. These were powerful----- . We felt that he was above all that and at least by George we better be above all that too for now. We could do our laughing and giggling later on. The Professors that did ride herd on us that way, we responded in kind. So we had other people we could take our aggressions out on.

K.E.L. - Did you have any impression as to what he had to say about emotion? Did he ever mention that?

J.A. - I don't recall any mention in the course about emotion substrates. It was only later that I discovered about the 1937 paper. I wasn't aware of any. At the time we took that course the emphasis was definitely on the basal ganglia - the fields of Forel and probably the reason that I understand them (even though it sounds arrogant) at least in whole range serial sections, I won't take a backseat to anyone to being able to get up stone cold with a pointer and say well this is the----- coming through here and the oblique section looks a little funny but that is where it is. I can explain my way through that, and I got that from the review of the slides and from Pearl Papez's pictures, which showed that hairpin turn and how some of the fibres of the vicious (?) came in and joined it. Some of the problems we had were due to Medler's terminology not quite matching Papez. Medler, for example, used the term ansular vic-----for all the-----, whether they hooled round the-----or not or the capsule of ----- and that caused dreadful confusion. I remember the basal----- and the bend of -----, these things were discussed in one lecture

and there was tremendous confusion as to the epi and synonymy.

K.E.L. - There is still a log of confusion?

J.A. - Yes, I think there still is but of course today, a lot of progress has been made - transmitter signatures of afferents, to these areas or efferents from them. For example, the nuclear-----which Papez taught us as a septal (?) nucleus. I heard him say many times ----- and he would never really explain that but, it meant it was lying up against the septum. Now we know from the neuro-transmitter studies but also tracing techniques, that it is better considered part of the strai-----but the emphasis, I don't remember the limbic system being discussed but I do remember this tremendous emphasis on motor - extra pyramidal system was the one he talked about most. And then the next year when we were taking the gross anatomy-----section, at that time he had gotten into -----bodies. I knew very little about that work except that people in the department were openly laughing at him. Several professors and graduate students working for those professors came back from the anatomy meetings, held at Brown University, Rhode Island, and were extremely - they displayed great merriment in discussing that Papez had been ridiculed at the meeting. They thought he was senile. I saw a few of them, they were set up on demonstration the year before as part of one of our neuro---, one of the last exercises in the course. Then sometime in the late spring '51, he and Mrs. Papez left and there was, I am trying to remember - I had suddenly fallen in love with my present wife. I thought mostly about her at a time when I could really have been watching, making more observations. It was spring time! But I do remember going to some party that was given for Papez on his retirement. I know that Glenn and Fred were there and some faculty members came. I can't remember where this was held. I believe it was held at one of the faculties.

K.E.L. - You don't know about anything that went on between him and the trustees?

- J.A. - No - he kept all that to himself. He may have shared some of that with Glenn and Fred in those frequent closed door meetings. We were never able to learn what went on in there, and that was in such marked contrast to us. We were always telling Glenn and Fred what went on with our professors. Sometimes they knew it already because Glenn was teaching in another course. A very big activity that went on up there was the teaching of the female students in Home Economics. Papez had a course called Human Growth and Development. After he left, M. Singer started teaching it and when Marcus Singer was suspended from teaching in the McCarthy era (mid '50s). McCarthy's counterpart in the Senate, Harold Belding got Marcus Singer up in Congress and then Singer talked about himself but refused to talk about other people. He had been in a Marxist discussion group and cited for contempt of Congress and convicted. That is another story, but I suddenly ended up teaching Human Growth and Development with the same enrollment about 350 girls. Several things about the course showed what an ambitious offering it was - it took and also it was an audience in which one had to be very careful. I used slides that Papez had made of the different body systems. I taught this course for 2 to 3 years. I never taught in it when Papez was giving it. I just know that he was very familiar with it. I believe that is what he did in the Fall (Papez).
- K.E.L. - How many sections to that course?
- J.A. - There were 2 sections, as he taught it. We did it all in one group. We had lab sections, I think.
- K.E.L. - What was Singer's relationship to Papez?
- J.A. - He just replaced him. He had no actual contact with him, as far as I know. I don't think he ever met him (Papez). You could ask him. Mark until recently was Chairman of Anatomy at Case Western Reserve. He had a stroke about 2 years ago. A very serious right hemispheric stroke. I've had letters from Mark since then, so that cleared and he has now got some residual problem, but it was devastating at the beginning, but I think it is still possible for you to talk to Mark. To the best of my

knowledge Mark Singer came from Harvard. He was a very successful young associate of a professor at Harvard who failed to get tenure there and the students demonstrated that he was an extremely competent teacher and they wanted to have him at Cornell or keep him at Harvard. Their efforts had simply aided in getting him out sooner. You can't tell the President or Fellows at Harvard what to do. So Mark ended up at Cornell and he was very respectable of Papez. One of the first things we did was to collect the reprints, put them in orderly files. They were mostly up on the top floor of Simpson Hall. If Simpson Hall still exists - it's been completely renovated. It was really an old building with beautiful wooden floors, waxed ones that we could put lemon oil on the floors, beautiful furniture in the building. But Mark never threw anything of Papez's away - contrary, he felt very proud of Papez. He understood what Papez was all about. He knew about his clinical interests and he knew about his greatness when during his course we did become aware of the limbic system concept. Mark started teaching there in '50. I left in '56 and he went on and left Cornell sometime in the early '60s. I'm not sure but he went to Case Western Reserve. Mark wasn't very much appreciated either. Two things happened, being hauled up in front of Congress as an alleged "Red". There were people at Cornell that didn't want to have anything to do with those bastards. Mark was a victim, a tragic victim just as so many others. But the other strike Mark had on him was that he was another one of these neuro-anatomists. He was out of step in a predominantly broad spectrum college zoology department. So those were 2 of the things. I guess another thing was that the Chairman of the Department was German and I respected him enormously as a scholar. He taught me many many things but I think he had a strong current of anti-semitism. This was expressed toward Mark. He just didn't like Jewish people so Mark had a hard time. That was the Chairman who felt that way. I want to be very careful about this. I have some other information about that. H. Donovan was a bachelor. He hard lined it as far as his graduate students were concerned. They weren't supposed to have girl friends or families,

outside the department. You were expected to live in the department. If you took time out of the department, that is for a vacation, you had better leave town because we were told that if you were on vacation that was grounds for immediate dismissal. In other words you either had to get very very far away from your graduate studies or be right on it. There was no middle ground. You had to go to Boston. That was one thing, but just to take 5-6 days off you couldn't do that. You could perhaps go to see a movie now and then but that was frowned on. You could go and have an ice cream with the boss. We used to do that except he drove an old rattle trap car and we were afraid for our lives when we went out with him. But I mention this because I think it is relevant to the man who was Chairman of the department when Papez was there. It was probably the man who participated in making life miserable for him and this is sad.----- was briefly Chairman Bill Stottler said.

K.E.L. - Did Singer occupy Papez's territory?

J.A. - Yes - his office, that pro section amphitheatre, the outer offices where Papez's technician had been. Singer put his technician in there. I got the office that Fred Johnson had I believe and Mark even used Papez's filing cabinet. He had some beautiful microscopes there. They were all the older-----and lights microscopes with the brass barrels and beautiful machined fittings. One could put the ocular in and would flow quietly with a hiss of escape of air. A number of curious objectives in focal lens. I guess that he used those for inspection of thick tissues, Colgi preparations. He had some unusual objectives.

K.E.L. - Did Papez leave in the Spring?

J.A. - He left in the Spring, in May I believe. As I said it coincided with my falling in love with a student which was also frowned on. I was not only interested in the young lady I was also interested in Professor Otterman (?) and my other superiors not to find out that I was dating

one of his students. This explains why----- had that not been going on I think I would have----- . I always felt that Dr. Papez was a nice man and would like to know him better. I always wished him well and was kind of sorry when gross dissection ended because we didn't have anything to do with him anymore.

K.E.L. - Did everybody know he was retiring?

J.A. - Yes - I think we all knew that.

K.E.L. - Was that because he was 65?

J.A. - We knew pretty much that he was retiring. We were told that there would not be anymore comparative neuroanatomy.

K.E.L. - Did other people that retired managed the same way? There was a real cut off.

J.A. - No one else did retire at that time. I am sure that the kinds of checks and balances that one has today, attention of society. I was the Chairman of the College for tenure promotion the last two years. About 10 years ago I was Chairman of all the University Committee on academic privilege and tenure which dealt with the abridgements of tenure by the University and I don't think there was anything of that sort then at Cornell. I really think it comes down to this - if the boss is mad at you and had the support of the trustees and the administration - too bad. I know of several other professors who didn't really agree with the Chairman but kind of played along because your head would roll if you didn't. A kind of benevolent despot - dictatorship situation. We felt the same as graduate students - we were given heavy responsibilities but we weren't really given much in the way of redress and dialogue. We didn't really toss back much. I don't want to put down my training at Cornell because I think I went through an outstanding laboratory and even some of the people who were very critical of Papez were in themselves outstanding scholars and this

was looked upon as one of the last golden moments of American Morphology. I was in a department that had Simon Henry Gage, Bert Green Wilder, B.F. Kingsbury and Sutherland Simpson was there in the physiology. There was some towering figures in Cornell and some of them were still there when I came, William -----, an outstanding comparative histologist. Sam Leonard, a very fine endocrinologist. Perry Gilbert, as I was winding up my graduate training there, Perry Gilbert was having publish or perish problems and so they sent him off somewhere and to everybody's pleasure, he became the world's expert on sharks. He worked on the mating of sharks. The Navy has used his consultant services. But again I mention that because that was a person that didn't really hurt Papez that much but he didn't extend a helping hand - later repented, as I told you in his conversations with me.

K.E.L. - He had no cohorts of support? that were visible - he was isolated?

J.A. - That is right. When you write about this, you will have these people suitably masked so they don't----- . I don't want to hurt any of my teachers because I think they are all still alive. Sutherland Simpson is not, but Wimson (?) definitely is, the last I heard.

K.E.L. - They are all pretty old.

J.A. - Yes, that is true.

K.E.L. - Well, that was an interesting year. Did-----

- J.A. - Singer taught the course in neuroanatomy. I took his course. Let's see, how did that happen? I also went down to Harvard Medical School on Singer's advice and took a course from Russell Barnett who was Singer's replacement at Harvard or who took over teaching neuro there. That was in '52-'53. Somewhere in there Singer got suspended. After he was suspended I taught neuro-anatomy, as a graduate student - I taught Papez's human growth and development but Singer's neuroanatomy course - but he of course was behind the scenes telling me what to teach and I learned very effectively this way. I didn't teach the course by myself. I had Marion Diamond, now an outstanding neurobiologist at U.C. Berkley. But I don't think Marion ever knew Papez. But Marion and I taught the neuroanatomy, and I know we had discussion of the 1937 paper in there - from where it came from, I don't know. Singer was very respectful of the memorabilia that is fiscal and intellectual that was around the place. He knew he was filling the shoes of a very intellectual person.
- K.E.L. - Were you aware of much memorabilia that was left there?
- J.A. - The microscopes, some of the charts, the big charts, the capper's (?) charts were all there. These really were a marvellous collection. I know it wasn't complete but I would say 3/4 of Papez's collection - reptilian and amphilian and bird brains were there. You could to to these cabinets and pull out slides of the brains of turtles. I remember looking at the mesencephallis nucleus of the 5th nerve and I use the word kind of with tongue and cheek because it is really an intermedullary ganglion cell. The mesencephallic cells are really dorsal to the ganglion cells living in the central nervous system and I was interested in that so I considered doing a dissertation problem on that. I thought it was a very attractive subject for a possible Ph.D. dissertation. I ultimately selected something else. I went to the slides of Papez's comparative neuro collection and I was able to look at that nucleus in several different reptiles and amphibia. He had slides, I think, of salmon, whitefish. He had some shark material.

- K.E.L. - Did he have any armadillo?
- J.A. - Yes - I think there were slides of that.
- K.E.L. - That is one of the things Paul Yakovlev and his family, when Papez visited him - his children were fascinated with Papez talking about the Armadillo.
- J.A. - Some of this material was discussed in the course. He used to talk to you but the course was called comparative neurology. We did have lectures on the forebrains of various major vertebrae classes. I guess an impression that came through and Singer was guilty of this too - that one vertebrae led into another. If a frog kept at it long enough its life would get bigger and better and it would turn into a reptile. I got the picture that what they used to call the -----genetic scale that somehow each animal leads to another. The lower is at the left hand and the higher at the other. We never did get the picture you know that all of the brains we see are the brains of-----both the differentiation of stem forms. We had no concept of philo (?) genetic trees. Now whether Papez did - I'm sure he did but he never bothered to give that. He had some models there - the usual clay (?) Adam models - the salmon and frog, turtle brain and brain of the pigeon and the brain of the mammal. He also had one of the Dr.-----models----- of the cerebrum. These are models that were invented in the late 19th century - some in France. They are still marketed today. They are intricate models - paper mache with hooks and eyes and a certain sequence of assembly and disassembly, but those things were still there.
- K.E.L. - There was quite a bit of stuff - he was interested in the trigeminal descending tract because Paul Yakovlev talked about that.

J.A. - Well another thing I remember is that we were asked to. I don't know whether Papez started this or not (whether Glenn Russell did). We were asked sometimes to come out of the audience up to the screen and take the pointer and point out things, and that really brought a lot of respect and fun. You could sit there and laugh until somebody got up with a pointer, then you realized that it was quite difficult. I remember the first time pointing to something dark and being told by Glenn Russell that this was the myelin staining erythrocytes in the basal arteries and I better rise my sites a little and get into the brain stem so the next time I pointed to was also a black dot and I hoped it was the------. It was one of the few things I learned right away. No, he said it was a similar vessel. I think the third time I got on to it. I picked a huge thing. But that gave us (although Papez didn't make us do that) some respect for what we could do.

K.E.L. - What did you do after you graduated?

J.A. - In 1956 I completed a dissertation on neural pathological effects in which I found out that-----disease inhibited regeneration by arresting mitosis. He thought (Simmons) that there was some effect on the nerve so he asked me if I didn't want to work on that. I did that and found out-----was profound - neuropathological effect on peripheral nerve. I also found that disease had a general toxic effect on the animals. The golden hamster, had an LO 50 very much like that of the rat. We didn't know about the blood brain idea of what I wanted to do next. I was just handed off by Mark Singer to his friends in Boston at Harvard Medical School. Paul Yakovlev had worked with Singer and even as long as 2 or 3 years before I got my Ph.D. I had been the native runner carrying photographs to Boston. So when I would go to Boston I would take pictures with me and with Yakovlev and say my mentor Dr. Singer has labelled these and wants to know what you think. If you have something to take back I will do that. So I called myself the courier of ------. So when I got my Ph.D. Marck sent me down to Paul Yakovlev hoping I would get appointed

to the Department of Anatomy. (Something about a Professor with a terminal illness) I think it would be best for your development to throw in your work for the department of neurology. You could work with Paul Yakovlev in the mornings and teach. Neurology wasn't even a department then. That is a complex story.

It was the department of neurology and psychiatry and what ----- wanted me to go into was neuropathology and neurpathology did not have a department. At any rate my first academic appointment was research assistant in neuroanatomy in the division of neuropathology in neurology and psychiatry which is one of the longest titles that I ever heard and behooved that the length of the title is in inverse proportion to the height of one's ----- But that was my title. I got to work very closely with Paul Yakovlev and assigned to the Warren Museum or from '56-'59 or maybe a bit longer. Sometime around 1960, Harvard had selected Don Fossel to come and assume the leadership of anatomy in the interim Roy Green became acting chairman. So when Fossel was selected as chairman of anatomy I moved back. I have to mention Raymond Adams. He was absolute crucial in my development when----- told me he was about to die. He died of prostatic Ca. - there were metastasis. I remember one of the first things I did at Harvard. Dr. Singer and I and Paul Yakovlev all went to his office. I know I went to his funeral. I can't remember if it was with Yakovlev or Singer, but at any rate Ray Adams (?) had to create a -----for me - formalized to my relationship with -----and was regarded as a kind of assistant to the curator.

K.E.L. - Was he chairman of neurology then?

J.A. - No - there again it was so complicated. The department was neurology and psychiatry and in neurology there were these two great men - Adams and Denny Brown. Ray at M.G.H. and Denny at the City. There was a great rivalry, and Denny had his own group of disciples in his sphere of influence. You were either one of Ray's boys or Denny's boys and I prided myself that I sat at a certain happy point in which both those circles intersected. I maintained all the way through good cordial

relationships with both of these men. I learned greatly from them and I don't think I did a bit to solve any of the problems - that wasn't my role. I was just a lowly post-doctorate - not an unusual post doctorate but Ray made that possible and I was very loyal to him but I felt personally silly that there should not be these----- in the department. I didn't get into that. I just refused to become part of either camp, but nevertheless I felt more of Ray's camp than Denny's. It was pretty well accepted that the Museum was Ray's turf. Denny came sometimes to see Paul or to teach a course. Denny would call me up sometimes to see what we taught the students. I would have to tell him what happens when you transect the brain stem behind the -----There was a lot of cellular physiology but they were not getting systemic neurophysiology. In the absence of teaching systemic neurophysiology the class had Sherrington (?) type. It was a kind of a happy accident because that brought me in contact with Denny Brown. Well I think maybe I had better give them a review of what happens when you transect the tracts at various levels. That was a very good idea and by the way I will be there too. I've only read about it in books. So I was working in that neuropathology division under Ray but teaching in the department of anatomy.

K.E.L. - But Ray was specifically neuropathology.

J.A. - Yes and Brown was neurology. They both taught in the second year course.

K.E.L. - Now Ray was in the City?

J.A. - No - that was earlier I believe. The whole time I was there Ray was at the MGH - a very good friend of mine. Then Stanley Cobb (?) was in retirement but I saw a great deal of him. He was thrilled with comparative -----of neurology. He had some surprising grandchildren with air rifles. He had a log of bird brains, some legally obtained and others not. He had turkey and pigeon brains, hawk brains and a number of reptile brains. I would frequently go down to the General to look at nuclear with Dr. Cobb. I would help him identify. I remember one

time I was able to find a facial motor nucleus and it was a very difficult thing to identify. Dr. Cobb had not gotten used to the idea that something like the facial nucleus could be practically visible on the surface. He was looking too deeply. Very practical things of this sort, I enjoyed very much. He was just the most delightful person. He treated me as an equal. I realized that he was having fun and so was I. Then we would go over to the Museum of Comparative Zoology and he would talk to his dear friend-----she was a paleoneurologist. She studied the fossils, cranial remains of prehistoric animals. It is sort of a question whether you think the gas tank is half full or half empty. We would find ourselves pretty frustrated because he would see the cranial-----or imprint of----
----- . But I remember first to bring us back to Papez I learned more than I thought in comparative neuro. I had not learned very much but I had a point of view. I had learned that all these structures - in different positions sometimes - the mesencephalic nucleus in fish brains was a unitary structure as Paul Yakovlev would say. It was right in the midline. This suggested to me that maybe you could split it up in two pieces and unite it, but I had this point of view and I know that I got that from Papez and to some extent from Singer but Singer I think expound the point of view as one of his specific ways of expressing respect for Papez or maybe it was just because the college catalogue said - look this has got to be comparative vertebrae neurology. So we find out at the end of the year that it was all that human stuff. I don't know why Mark did that. I recall that he seemed to like it. Mark had a lot of clinical orientation. He could put it across in a more vivid way. He had more impact on the students.

K.E.L. - He (Papez) had a sort of mystically-----huddle?

- J.A. - Yes, he (Mark) had much more charisma. If not the greatest teacher, Mark was certainly one of the greatest.
- K.E.L. - Did you get anything from Stanley Cobb's - that is in perception?
- J.A. - Well, I did get from him----- . He was a no-nonsense kind of person. Sometimes in 4 letter words - exactly what he thought of something. He did tell me that-----we were talking one time about-----book on the tiger salamander and Cobb remarked that those structures (mentioned in this book) were baloney! I really enjoyed that - his sensible outlook on things. I think that is one of the first times I ever allowed myself to question something a great deal. It was the labelling that he didn't like - not his work. He didn't like the terms which he felt polarized. What I got from Stanley Cobb's - was a willingness to challenge ideas no matter who came up with them. Even if it is a great complex concept there ought to be some 25 or 50/cent terms one could explain it in. I didn't think that one would have to use-----terminology. I wouldn't say he was the only one that had that effect on me but Cobbs and Singer were the important ones more so than Paul Yakovlev. Dr. Yakovlev was a hell of a man conceptually. His writings were full of very long words - convoluted passages. He knows about the shading of synonym and antonym, but Yakovlev never wrote in a very concise earthy style and Singer and Cobb did which I liked, because Ernest Hemingway was one of my heroes and I liked short pithy words.
- K.E.L. - Cobbs came on target very quickly. Cobbs was an imperialist (?)
- J.A. - He would come and give lectues. I worked behind the scenes making sure the slides were on correctly. At that time anybody who was somebody in neurology was either in Boston or passed through Boston. All the big New York people - Penfield, etc. I had the honor of taking Dr. Penfield around - a tour of the Museum talking about the thalamus. Also R. M. Norman----- . I took all these people through the Museum and Webb Haymaker came frequently. He was another person that was right-----

K.E.L. - During that time Ray Adams was directing the Museum?

J.A. - Yes - pretty much so. When my 11 years at Harvard was up Dr. Fossel my chairman had advised me that I probably wasn't strong enough to be considered for tenure from a research point of view. I think I knew that and never contested it. It was also though evident to me that my teaching efforts were very much appreciated by Harvard. I thought that I was a pretty good teacher. I never had much opportunity for service. I don't think I even identified in that sector of University life (?) I was pretty much held down in the department of anatomy and committee functions. The only time the department met was at a seminar or perhaps-----but anyway Adams was running the Museum then. I am not sure how it got started. Dr. Fossel came up with the idea that I would be curator of the Museum as a way to keep me at Harvard and that was one of the greatest honors I've ever had. After all, the man that I replaced was Paul Yakovlev. I was very tempted by this - I loved the Warren Museum. I was aware of all the specimens - the wet and dry specimens, the skulls. If you go to the Warren Museum today you will see the white letters on the -----skulls. This is the-----
I was the person along with Harry Fallon who painted the whole bar white and let it sit for 5 minutes and then wiped it off with a rag. We were tired of explaining to the millions of visitors who had their hands on the bars which they though was a guard rail, asked where the crow bars were - I would say - that is it - you are holding it. So that is just a little example of the Warren Museum. It was really home to me even though Dr. Fossel gave me an office in the building, hardly a day went by that I didn't return to the Warren Museum to see my friends or my projects. During the end of my period at Harvard-----

K.E.L. - This must have been '66?

J.A. - This was in 1966 and so this offer was made. Two things influenced me to leave Harvard and go to Arizona. One was the Warrn Museum was a very attractive prize in the area of spoils in what had now become a hot war for territory in the University. Ray Adams wanted it as a

kind of retreat where he could go and write papers. Dr. Fossel succeeded in having it divided up into six teaching laboratories. George Erikson was down at one end and wanted to branch out and have more room for his mammal collection. The people downstairs wanted to use it as secretary space and that is just about what has become of it. It has wall to wall terminals. The Museum today is confined to about 1/10 of its original size. Harvard never had any respect for this pavilion. They never put a guard on the stairs - things were stolen - people didn't take care of the specimens and they didn't get the support they needed and Dick Syndeman (?) came and was now building a marvellous new research enterprise of which I was a part and had a deep impact on my life. Dick changed me from being a handmaiden to the clinical neurology department to having my own research career. I got into work Dick was doing. Here I was, Ken, in the situation of a curator - of having to say no to all my dearest friends.

I was told by Fossel then the dean, not to listen to any of them and make a new Museum which would deliver its contents with the contents of other Museums to the public of the Boston area. In other words I would be local interface for the public. Perhaps have an exhibit on leukemia for 3 months and move it around Boston - take it out on tour. Well I thought I really don't want to do that I don't want to stand in the way of any of my friends. I had my own feelings - I wanted the Museum to stay more or less what it was. I wanted to see the brain collection kept there. Finally, Paul almost single handedly moved it in a rented truck to A.F.I.B. The other reason was at that time once in a lifetime opportunity to go out to a frontier situation occurred - that is the University of Arizona. I think if I kept on at Harvard I would have made an outstanding independent investigator. I don't apologize for my life - it is just that I got more pleasure out of the Medical School Program. I feel very satisfied with that.

- K.E.L. - That is wonderful to be able to say that.
- J.A. - I am just sorry there is not a new Medical College being built on some frontier. I couldn't go back to a City.
- K.E.L. - The crisis period for the Museum was a very interesting period. I have the feeling from Paul that he is now very disappointed with the A.F.I.B.
- J.A. - Yes - I worked with him. I was one of the A.F.I.B. panel of so-called scientific experts that would meet once a year and discuss the future planning. We made some recommendations about the animal aspects - to build a more comparative cataloguing.
- K.E.L. - Paul thinks they are closing or shutting down.
- J.A. - The technician he had was a real servant - very loyal.
- K.E.L. - What was the conflict involved in the termination of Paul's-----
- J.A. - He eventually reached retirement age and we were all about to have a party when Al Pope came in. He was a very reserved fellow, a very fine teacher and very unlikely to tell you a joke. Al came in one day very animated and laughing - I asked him what was going on and he said - It is Paul - he has misdated his age. He thought he was 75 but they did some computing and realized he was a year younger than he thought and had another year to go.
- K.E.L. - How great!
- J.A. - But eventually he was just retired because of his age.

- K.E.L. - You can go along there - way past 65. I went to Toronto in 1967 and spent a weekend with Paul in Brooklyn because I wanted to particularly get some stuff from his library. His house is extraordinary. It is like a fortress. The whole walls were books - it was all screened from the outer world and you couldn't see out. He is a night person and stays up very late to two or three in the morning. I catalogued a lot in his library and counted 82 volumes that I would like to have for myself. That was when he was negotiating with A.F.I.B. Suddenly I got some of those things - some of the letters of Papez I was interested in but suddenly that disappeared because he was committed to A.F.I.B. and then he said afterwards that he didn't think A.F.I.B. was interested in laying claim to that stuff and a lot of it will disappear.
- J.A. - When these things were going on I had already left - the Spring of '67. I was actually committed to -----a year before that.