THE PAPEZ MEMORABILIA - GALVESTON, TEXAS

NOVEMBER 5th, 1982

Glenn Russell

Mrs. Russell: - The little blue bodies - I didn't know what he was talking about!

K.E.L. - Did that come late in his career?

G.R. - Yes - that was late

K.E.L. - About '49?

G.R. - No, later than that. It didn't begin until - well yes, about '49 - he left in '51.

K.E.L. - What did you say to your niece about Papez? - to tell her something about him?

G.R. - It all begins really with the publication - that paper of 1937 would not be accepted by an editor of a major journal today - no editor would buy that paper today - they would reject it out of hand! Yet that paper has probably generated more publications than any other single paper. It created out of whole cloth a whole field of behavioral psychology. It wasn't until Paul MacLean read it and understood it - that was a decade later - I tell the students when I lecture - here is a paper that never would have been printed in this day. Yet, probably at least a third of the papers in behavioral psychology - will refer somewhere in their opening paragraphs to Papez, 1937.

So he created something out of whole cloth, out of his brilliant mind, realizing that something was wrong with prior theory on the rhinencephalon which was not consistent. He made a number of minor errors but he saw the problem. They were mostly of omission.

K.E.L. - Such as -

G.R. - Well, failure to include the amygdala.

K.E.L. - He stated simply that its function was unknown.

G.R. - Yes - at that time there wasn't anything known - it took this paper to open that door. The complexity of the system was simply not conceived at that time. When I was a graduate student the Limbic System was a much simpler place to be living than it is now, but the fundamental concept was that there was a behavioral mechanism interdigitating with the somatic mechanisms with a two way flow back and forth. It was an absolutely monumental conception, that should have been recognized before his death. He should have been a Nobel Laureate.

K.E.L. - That would only have been in retrospect.

G.R. - It could probably never have happened, because of his terrible antipathy and fights with Ranson. The bitter war that they fought because Ranson viewed the balance in such a dramatically different way. They had some very good and really bitter fights - the last really good fight in the American Association of Anatomists. And finally since Ranson had a "majority" taking his view, Papez ceased going to the meetings, which was unfortunate. But the other thing I remember about Jamie Boy - had he not been able to write - much as he hated to write - it was a painful thing for him, he would have been virtually illiterate. He could not talk or lecture except one on one. In public he was the worst lecturer I have ever heard. In groups he never spoke. He hated to write, but he sat down and literally "pained" himself to complete a paper. Of course we all know the "Proposed Mechanism of Emotion", but "The Brain of the Nine Banded " was one of the classical monuments in the neurological literature. The two papers on the thalamus of Macague were absolutely superb. There are a number of others which are just outstanding - major literary as well as scientific achievements. They read well. If you had never know the man, but just read his papers you would never believe that he was so totally illiterate as a speaker except one on one.

K.E.L. - The years that you were there?

G.R. - I met him first in 1942. The war came and I left - I came back in 1947, and immediately went back and was appointed as a teaching assistant - as his graduate student - so from '47 - '51.

K.E.L. - In '42 you took his course.

G.R. - I took the first course as a sophmore "comparative neuroanatomy" I admit it was not my most stellar performance. It was a course for graduate students. It was not an easy course - upper classmen took it by arrangement.

K.E.L. - What brought you back to work with him later?

G.R. - Just him - I came back because I was so fascinated by him - and neuro became a subject of great interest. I probably could have come back and gone to Medical School but I came back as a graduate student in Anatomy.

K.E.L. - But, you weren't impressed by him as a lecturer - did you have personal contact with him?

G.R. - Oh, yes, indeed - fortunately - there were only 7 in that class in 1942. The whole class was quite informal - we had lectures in the old amphitheater. It was a very steep old amphitheater - but a little tiny one. There are some pictures of it - you should have one. Bill Williamson.

You sat there and listened to this man expound. It was more than a one on one. He wandered and wandered.

K.E.L. - Did he have graduate assistants when you were there in '42. Stottler had been one earlier.

G.R. - Stottler had just gotten his degree in '41. I took the course in the Spring of '42. Bill Niemer was there. There was another one that was wierder than Bill.

K.E.L. - Where is Niemer now?

G.R. - He is dead. He was at Creighton - Bill Niemer was at Northwestern with Snyder and Magoun. He left there when that team broke up.

K.E.L. - When you were there you had Fred Johnson?

G.R. - Yes, he was one of the two graduate students working with Papez.

K.E.L. - I have just come from a visit with Jay Angevine.

G.R. - Yes, I taught Jay.

K.E.L. - Exactly. He said that you and Fred Johnson, provided the things that Papez didn't provide - you filled in the blanks. Papez would spin this sort of ethereal, mystical formula and you would come in and answer the questions Papez left dangling.

G.R. - Yes - right to the end of my graduate experience with Jamie he was remarkable. He had forgotten more neuroanatoly and morphology than I could ever learn and probably more than anyone else will ever know, for that matter. He could site it and go to his files and produce the slides and his drawings.

K.E.L. - We have about 300 of his sketches and drawings with his notes on them. That was in the material you sent us. It was the two cardboard boxes of his papers and notes that you cleared off his desk and lab benches - that formed the nucleus of the Papez collection.

G.R. - When Jamie boy retired and was leaving, I was still there. We were still working in the lab but he had closed it down. We were stuck there for the summer. I was kind of left without specific instructions but the idea was that I would clean up the lab and the office. And I talked to him about it - he didn't want any reprints, or any papers or anything. I tried to salvage what was there and might be useful - that looked like possible archival material - but I had no experience in that sort of thing.

K.E.L. - Was he mad at that time - with the Department or the University - was that why he left everything?

G.R. - No - he wasn't.

K.E.L. - I had visualized that he left in a fury - walked out and slammed the door.

G.R. - I had three years with him. I did my Ph.D. and M.A. together in three years, which is a little unusual - but I was older and already taken some or most of the required graduate courses. I was known and accepted in the Department. My major professor was in residence in Columbus, Ohio for four of the six terms in my graduate program. But as I was completing my B.A. I was actually taking graduate courses much of the time, so I had a lot of contact with Jamie Boy. But finishing my dissertation in those circumstances was not easy. Having your supervisor professor in Ohio ain't the easiest way to do it.

K.E.L. - During those summers was he at the cottage?

G.R. - No - he had become resident in Columbus.

K.E.L. - So you were there from '42 - '43 and then '47 - '51?

G.R. - Well from '40 - '43 and '47 - '51. '47 - '48 was when I completed my undergraduate degree. Cornell had refused to give us any credit for service - you came back and found you were hours short. I was something like 12 hours short of my degree requirements, so I decided to take the whole year to get back into the swing of things. I was four years in military service which is quite a gap, but during most of that year I was taking graduate level courses.

K.E.L. - Who came in then?

G.R. - Fred Johnson joined me then and we shared the office with Ellie Henschel. She was the German girl - a superb technician . The other graduate student in the office was Sam Leonard's student. I can't recall his name - Ernie? He is the new Dean of Medicine but was then the other occupant of our office - Oh - it was Ernie O'Neill. He was a physiologist - primarily a chemical physiologist - now "biochemist". I had had a course from him in my sophmore year. When I came back I finished my Bio-Zoology major.

One of my favorite stories - I was taking comparative anatomy in my sophmore year in the Spring. Perry stopped me leaving the lecture one day and said "I understand that you are taking comparative neurology with Dr. Papez. I have talked with Dr. Papez and I want you to give the lectures on the brain in this course!" I had never been approached before to teach anybody anything. Lecture to my own class? But he said "you have been taking Papez's course - you can do that better than I can". All of the comparative anatomists are terrified by the anatomy of the brain. So in my sophmore year I started teaching - to my own class! - my first teaching experience. I found I could do it.

K.E.L. - That must have had some influence on your coming back after the war.

G.R. - Yes - I was fascinated with anatomy then - I was fascinated with Papez as a person and his course in comparative neurology - for a few of us who had particular contact with him he was a superlative man. People who were his students were fascinated by this crazy man. We all revered him way out of proportion.

From my knowledge of the people I have met - the handful of his graduate students - (I have still never figured out how many students he had) most of them went on to rather successful careers. He attracted people who were quite different - not the standard approaches to life in general and academic careers etc. Bill Niemer who was a very strange man - very successful in many things. He could have been if he were more forceful, very succesful. But his name is remembered. In the Snyder Niemer Atlas and other things. Bill Stottler who was considered in the profession a sort - of waste land sort of guy - way out - never popular. Yet he was one of the nicest guys I ever knew. I was dearly fond of him and he made really significant contributions for which he had only limited recognition which came much too late.

Fred Johnson, I can say a few things about. In retrospect, having never been exposed to medical situations, I still realize that he was a paranoid schizophrenic of the worst kind. But when we were associated I didn't realize that this was what was wrong. His contribution however and his technical abilities were superb. He was a bright, bright guy.

K.E.L. - What was he interested in?

G.R. - He was one of the early workers on what became the Nauta method for transneuronal degeneration studies. Apparently Papez told him not to go down to Washington to the A.F.I.P. (Armed Forces Institute of Pathology) - no it was across the street - the research place. Instead he went out to Oregon to work with the cerebellar man - Olaff Larsell. Except Olaf took that year to go on leave. Then he worked with Archie Tunturi - Tunturi is brilliant but a psychopath in his own right, but it didn't work out well and he came on East to Ray Snyder's. He came apart and Ray couldn't save him. While he was there he just collapsed. Since that time he has been in and out of institutional care as a frank paranoid schizophrenic. I don't know if any of them survive intact. There was a momentary flare of genious and then it was gone.

Those were the ones I remember best. There were not so many because we were actually never close to each other. Orjanski (?) was one of Papez's students and I have no idea where he is. He was later Dean of Medicine somewhere. He was an original member of the Cajal club. But we were all separate. None of us had any arrangement or mechanism to communicate with each other.

K.E.L. - That is odd isn't it?

G.R. - Maybe it was a reflection of Jamie. He was so much isolated himself and he picked individuals and communicated with them on a one to one basis. He was a real "loner". My wife commented earlier that when the Medical School finally moved totally to New York City, he was not invited to go along.

I don't personally know that he was ever personally offended by that, however. I doubt that he would have been happy.

K.E.L. - What your wife implied was that Pearl didn't want to go.

G.R. - I may have been Pearl, but he was not invited himself. He told me he was not invited. I don't think he would have been happy in New York. It would not have been his wish to live there. He remained in the department of Anatomy. He had been extremely close to Professor Kingsbury and very close to Simon Henry Gage. In fact when I got there Simon Henry was still alive. I remember seeing Gage and Papez sitting together talking as old old friends. So I don't think he would have wanted to leave. He was comfortable in Ithica. New York City at that point in his life would have been disaster. He was a country boy.

K.E.L. - Yes, Paul Bucy said that when you first saw him, you would think he was a farmer rather than a scientist.

G.R. - Yes - he had no airs - he was just one of the people.

K.E.L. - There was something almost mystical about his relationship with people, it was very individualized - more than just one to one.

G.R. - He was great. Although I don't believe as an advisor to students he was worth a good God-damn. He usually wasn't advisor by his choice. He became mine informally. Officially my advisor was a crazy nut over in Chemistry - an absolute disaster, but by then I was dependent on Jamie and had already made up my mind. I knew where I was going. I would come back and work with him.

The policyat that time at Cornell was that you were accepted for the Graduate School by your Professor. He told me "you are in the Graduate School". In my total experience I think I went to the Graduate School Office about three times - usually to pick up paper or something like that.

It is the most delightful system of graduate education possible. It is a one on one preceptorship with the Professor. I gather it is still that way at Cornell.

K.E.L. - What about his relationship to the Faculty? To his Peers?

G.R. - Always great deference. He never tried to impress himself on them. He never made efforts to be a leader or to make inroads. But I think all the Faculty looked upon him with tremendous respect. It was not just his age - he was one of the older members when I was there. I think they looked upon him as a distinguised academician - a true man of science. He had a long history of productive work.

It was funny that it ended with "the bugs". We all agreed that we would not talk about it - it was one of the things we did not discuss. We set it aside by consensus. This interest in the bugs did not affect his sharpness in general at all. If you take a tough example like the basal forebrain bundle - he would describe it to you through the whole biological milieu - its evolution, embryology and everything and bring out all the papers to prove his points. His knowledge was tremendously organized. He would go to the slides and make drawings. He had a magnificent collection of material.

I don't know what is left. Mark Singer would be the last one to know about it. I would love to have stolen the whole thing, but the only part I know was the collection of papers I passed on to you.

K.E.L. - Who asked you to clean the place up?

G.R. - Dr. Adelman - clean it all up, throw it out, burn it etc. I chose not to. I called Jamie boy and asked him - "what am I supposed to do with all this material?" "I don't care". That was all.

I recall one of the great things I learned from Dr. Papez was on a visit to Columbus when I was working on my dissertation. We were sitting outside his office on the ground floor of the State Hospital - over beautiful greensward with lovely oak trees, a few patients wandering around. He looked at me and said "Glenn - you see the squirrels up in the oak tree?" - I said "yes, Dr. Papez". I never addressed him as anything but Dr. Papez. He said "you will notice there are two kinds of squirrels - describe them to me". "Well, they look like just squirrels". He said "well, watch them". I did for what must have been 10 or 15 minutes. He never said a word, and finally I said "you know - there are two kinds of squirrels up there". He smiled and said "you know, you are gaining in your ability to observe". "There are some that don't do anything - they sit close to the trunk of the tree" - - - "yes and observe what happens". He continued right on - "there are some squirrels in this world who get up in the tree but stay close to the trunk - the nuts are not plentiful or very good there but they stay close, but other squirrels go out to the end of the limb - they have a marvelous time, they ride up and down on the breeze and they find wonderful nuts". "They are the ones that have to look out for the kids on the ground throwing rocks".

I think I learned a lot from that - there it is - if you are afraid to go out on a limb and have rocks thron at you - you don't really belong in this business.

He was such an astute observer of people, but his quietness obscured his greatness for most people.

I regret that I wasn't there when he was fighting with Ranson. They were bitter battles. Ted Magoun talked about it with me, briefly. It was the 1937 paper. He had just refuted everything Ranson believed in. The hypothalamus. Freud's concepts were then extant. He actually fought in the anatomy sessions with Ranson, but his type would never be successful in a public fight against Ranson.

So he essentially ceased going to the Anatomy meetings at that point. That was in the late 30s and early 40s. I think the anatomists meetings were no longer a forum for that kind of discussion.

K.E.L. - Stanley Cobb asked him to do something at one of the meetings in that period but I don't think the session ever came off.

G.R. - That was the time of lipofuchsin, perhaps at one of the Cajal meetings. Jamie's bodies in the nucleoli, we thought were lipofuchsin granules. The President of the Cajal Club was designated the apical dendrite. Last year Howard Watson retired as the apical dendrite, and I succeeded him. So I made Howard a "neuro fibrillary tangle" - a senile plaque.

In a way I think the proudest moment in my career was when I was made a nucleolus in the Cajal Club. It is about 32 years old. Wendell Krieg in 1937 wrote "the pure and simple neuroanatomist". We met first at the Mount Royal Hotel in Montreal. It started out at first as primarily social - we were neuroanatomists who revered Cajal and carried over to modern neuroanatomy. Our history has been one of revering the memory and works of Cajal first of all, then the scientific program which has become a major feature of the anatomists meetings each year.

K.E.L. - How many members are there in the Cajal Club?

G.R. - Oh, about 400 or 450 world wide. All the neuroanatomists in the world are members. During my presidency some bright young brash people decided we ought to change the name - call it the Society for Neuroscience or some such thing. I appointed a committee to study that matter. It astounded me 3 months later to be asked to be a Charter Member of the Society for Neuroscience. I am known as <u>that</u> Dr. Russell - Godfather of the Society for Neuroscience, because I was not willing to let the Cajal Club be taken over. I agreed we needed a larger Society, but it would not be the Cajal Club. The rules and structures of the Cajal Club we carried in our hatband - the dues are still $5.00. We still put on a magnificent program which attracts 200 to 300 people the day before the anatomists meeting begins. They come on a Sunday to that program. We give prizes, we have had some of the most distinguished lectures ever given in that field. I am editor and publisher of the proceedings. We now are working on the second volume. They are pure proceedings - no crap added. I am very proud of that organization. I went to Spain that summer and had the red carpet rolled out for me. I got a cinder in my eye coming down from Paris to Puerto del Sol. A couple of days later it got out of hand. One of the neuroophthalmologists came in to examine me. It was his holiday - I asked what I owed him. He gave a speech about Cajal, the Club, the President from America etc. - it was an honor - no bill.

There is something I must not forget to find for you before you leave. I met one of the neurologists who had a copy of the record made by Cajal on his 80th birthday. It was on tape - Cajal's voice - that was a great experience. The youngster who brought it to me said "do you know who my grandfather was - Del Rio Hortega".

I gues it was the kind of education we were privileged to enjoy - in many ways it was unique. There are none of us left. We are the end of that line. That generation of Rassmussen, Papez and those. There will never be another era of generalist morphologists - they just don't exist anymore. I have a number of very dear friends whom I consider to be great neuroscientists - they know more about the connections of the basal ganglia than I do, or the thalamus or the reticular formation, but if you put the whole brain together, there is nobody left. In a way I feel it is a terrible shame. I think I am the last of the Papez line of general neuromorphologists, and it is sad that I can't generate one to follow that line - not one.

If you are going to deal with the nervous system you should know it all. It is a painful process. I have avoided trying to fight physiology, neurochemistry and so on, but tried to keep up with morphology. Jamie boy used to say it is a great idea to use what anatomy is known and you have to use it all. To build an idea do not propose something that isn'g known - build it on what is known, and use it all. That was his philosophy in devloping the 1937 paper - the paper that should be his monument.

He was a delight to sit with and talk to. Actually we didn't spend a great deal of time that way, but when we did it was all very rich - almost more than the normal gut can stand!

K.E.L. - How much did he talk with you about the 1937 paper?

G.R. - I don't think at that time he considered it very significant. I don't think he ever thought about it. He didn't mention it. Actually the one paper I remember him talking about was a paper on a case of "situs inversus". It was a clinical case he was rather proud of.

K.E.L. - Do you know Vonderahe? What was his relationship to Vonderahe?

G.R. - Friends - respected friends - who could talk to each other without any political or professional hostility.

K.E.L. - He got a lot of clinical feedback going both ways - from Papez to Vonderahe and from Vonderahe to Papez. In our collection the papers that are of greatest interest in addition to the body of material that you sent us are papers that Vonderahe saved. He died in 1978 but he had saved all the letters Papez had sent to him. He had them all laid out in order - together with many Papez reprints. His Son sent them on to us after he died. There were several loose-leaf collections that were of great interest.

G.R. - I know they were very close. There was none of political jealousy. I went out to Ted Magoun's lab when I was still a graduate student. Jamie boy arranged it all. Bill Niemer was there. The whole status of the brain stem reticular activating system was still being worked out. Two graduate students at Northwestern did a lot of the early work. I know their names - you can insert them later.

K.E.L. - Donald Lindsey - No? - Moruzzi? No?

G.R. - Moruzzi was not there then - he came later. These were two Ph.D.'s working with Ted. I was there for 6 weeks during that summer. That was all arranged by Dr. Papez.

That was the first time I had met Wendell Krieg. He was downstairs. Ted was very upset that they had no anatomic basis for the activating system. I said "oh - good God Dr. Magoun - I'll be back in five minutes". So I rushed down to the library and picked up a volume and went back with Bielshowski's 1885 paper on the ascending reticular activating system, which I knew all about. I tell you one thing about Papez - both as undergraduate and graduate students he made us read. We had to go back in the literature. I read the Anatomische Unsager from Volume I #1 - through the first world war. It was one of the things you did. We had time and it was important.

K.E.L. - You spoke about the 1937 paper as his monument.

G.R. - To those of us who know all of his contributions that paper is the public one for which he must be recognized, because he created a whole discipline behavioral psychology as we know it today - he created out of whole cloth with his patron and disciple, Paul MacLean. But there were better papers for pure neuroscience - the paper on the thalamus of the nine banded amadillo is absolutely a masterpiece.

K.E.L. - But in terms of your experience with Papez he didn't refer to the 1937 paper?

G.R. - No - he never referred to it. In my recollection I can never recall his referring to the Limbic System until Paul showed up with - - - - but even then I never saw the paper. I didn't know the paper - we never talked about it. It was later, after I had left that I learned about it.

K.E.L. - Paul MacLean's paper came out in 1948 or '49.

G.R. - I had a funny experience with Paul in Boston. It must have been in the '60s. He was invited to the Cajal Club. I wouldn't have recognized him, but he knew who I was and reminded me of our association in Ithica. It was rather embarassing. He was there for a week or maybe less.

K.E.L. - Was Bucy there when you were?

G.R. - Bucy I never new - but I met him. I was one of the invited guests at the B.R.I. (Brain Research Institute) when it opened. Ted Magoun and I became very close friends. In fact he offered me a job. We both decided that I would last about 6 weeks and then we would be ready to kill each other. I don't belong in that sort of tense atmosphere - which the Institute was. They are all my dear friends out there.

K.E.L. - Was Papez on that Program? I don't think so.

G.R. - No - Jamie-boy really - aside from giving a Cajal Club address - he gave one to the A.R.N.M.D. (Association for Research in Nervous and Mental Diseases) - other than those occasions I don't know of anything he was invited to, or attended. He was not a mixer by any means. He was an old Norwegian boy from Minnesota. He never mixed with faculty.

The faculty respected him but didn't communicate with him. Howard Adelman for whom I have high regard said that a few papers and a graduate student or two, hardly makes a great Professor, but it turned out that he was a great Professor. He was most erudite.

Every Thursday all the graduate students in Zoology went to Howard's house. It wasn't required - there was no penalty - we sat in his home and talked. He had 13 volumes of Vesalins in his own private library and we talked about the ancient anatomists. I spent many nights listening to him talk about von Baer. He had copies of the original volumes. It was a strange experience for a graduate student to sit with someone so erudite in the history of medicine. I don't know what went on at the administrative level because I was never there. At the level that the student would know about - I doubt that they spoke more than three words - two of them hello and goodbye.

Papez lived up in the corner of the third floor of Stinson Hall. He was private - an island to himself. The rest of the faculty didn't understand him. To have at the undergraduate level in the Department of Zoology - to have a neuroanatomist was most unusual. It was frightening. It was like what happened to me when I had to get up and lecture to my own class - because the Professor was terrified by neuroanatomy. They all are - you can go out and ask anyone. In these days showing interest in neuroscience - it has changed some, but in those days it was a terror.

K.E.L. - If you look back on the 15 years after the 1937 paper, what accounted for the fact that it was so neglected?

G.R. - It was not popular until after I had been out and he had gone.

K.E.L. - Why was that ?

G.R. - Well nobody realized what he had done until Paul MacLean began a project saying "here is an interesting article - lets see if it is good". It burgeoned. It only took it a few years into the 50s before it was at least recognized.

I don't know whether Jamie-boy should have the credit. I think he should because that paper generated Paul MacLean's thinking.

K.E.L. - Yes - but what other clinicians or neuroscientists really were committed to what Papez was talking about?

G.R. - Only with Paul MacLean's article.

K.E.L. - Yes, but I mean even after the MacLean article.

G.R. - Well Karl was in the bag (Pribran)

K.E.L. - Do you think Karl was a real advocate?

G.R. - Yes, and he put much more vigorous neuroscience to it. He was a very important person. I think Karl is one of the most vigorous Scientists I have ever known (Pribram). He did vigorous studies on the clinical basis alone which nobody knows about.

K.E.L. - How much do you think he was aware of Papez?

G.R. - Oh - he knew who Papez was, although I guess most of his attributions were to MacLean. But he knew who Papez was because I know Karl quite well. I was close to P. and S. I knew Malcolm Gardner. He is still one of the meanest guys on earth. He was with the armed services.

K.E.L. - How about Papez and Paul Yakovlev?

G.R. - I know nothing about that.

K.E.L. - Actually he knew Papez quite well.

G.R. - I'm sure he did, but I know nothing about it. Jay would be a better source of information (Angevine) because Jay was very close to Paul. I have known Dr. Yakovlev but only after he came down to A.F.I.P. (Armed Forces Institute of Pathology).

Ken Earl put his neck way out when he took Paul Yakovlev and all his collection into the A.F.I.P. Ken came here as a student in 1954, then he was asked to succeed Webb (Haymaker). How do you turn that down? He came from the Navy and had a distinguished career at A.F.I.P. About 4 years ago they went after him again - "why don't you come back on active duty - we will make you a nice Admiral and Director of the Institute". He turned it down - "once you get me back in those stripes you can ship me anywhere in the world. He would have been superb. I think Kenneth Martin Earle is the most distinguished Neuropathologist in the world today. He is one of the Deans of Medicine who I taught to be a Dean.

K.E.L. - You sort of brought him up.

G.R. - I taught him to be an academician.

K.E.L. - When do you think Papez concept had a really significant impact on clinical thinking?

G.R. - In the late '60s and not too much then.

K.E.L. - How many people think in terms of Papez concept now?

G.R. - Well, now I think the whole concept of the Limbic System is rather well established. In the late '60s and early '70s it was beginning to have an impact in Psychiatry. In Psychology it has made an impact, but they don't impact very effectively. I think Psychiatry has changed.

Psychiatrists who are the leaders in the mental heal area have suddenly become biologists. Biological Psychiatry is now having its day - "live better electrically".

We used to have a hospital here - the Crandall Pavilion. It was redesigned. It had been a Negro Hospital, but was redone as a Psychiatric Hospital. The medallion at the top of the elevation drawings was "live better electrically" - the Medallion Homes Sign. That was in the early '60s. We now have a Bob Rose here. Whether you believe in Bob Rose or not - he was the one who did the big project for defense when the air controllers went out. He was sort of the Chairman of the Meyerian Psychiatrists here. In fact this was the front from which Meyerian Psychiatry spread. That was in the '30s, '40s and '50s. It was a rremendously effective program. Now we are almost purely Biological Psychiatrists. Whether that helps anyone or not we can't tell.

At least the psychiatrists are at least aware that there is some real anatomic, physiological and biochemical substrate behind their profession.

K.E.L. - And are they talking in Papezian terms?

G.R. - Yes they are - at least they recognize that there is a measurable anatomic system.

K.E.L. - In that group of biological psychiatrists do you hear them talking about Papez?

G.R. - They all knew who he was. I think perhaps it is getting to the point where the first paragraph doesn't have to refer to the '37 paper anymore. It has become redundant. I have not gone back to look in the citation index for the last two years, but a few years ago I did go back and found this one of the largest citation indices for that one paper of Papez in 1937.

K.E.L. - How long ago was that?

G.R. - About 5 years ago.

G.R. - I have told my students it is probably the most cited paper in the world literature. The behavioral psychology literature is so vast, and at the same time I point out that it would not be accepted for publication by any major journal today. I know that if I were an Editor I wouldn't accept it. Would you accept that paper if it was handed to you as a draft?

K.E.L. - Well, I would, partly because I am biased in that direction.

G.R. - Do you think you would get it by your Board?

K.E.L. - It would depend on how many other Editors shared that bias.

G.R. - It is armchair science. Jamie boy in many ways was a very armchair man. I regret the passing of that sort of writing - where you can speculate without masses of data to go with it. You at least get your idea out in view. If I tell somebody a bright idea that I have thought about seriously and it turns out to be wrong - I am not offended by that. At least I have made somebody think for awhile. I think that is what Jamie boy is about.

K.E.L. - Did he write anything else that had that speculative approach? You were pretty familiar with all the writing he did.

G.R. - It is the only paper of his I know that is anything like this. I think I have read initially everything he has written. The rest were rather conventional science. Some were brilliant like the nine banded amadillo. That ranks right up with Lehorn Creighton's humming bird papers, as another great monument.

K.E.L. - And yet it dropped out of sight and was virtually ignored for such a long time.

G.R. - Well he was ahead of his time. The world wasn't ready for this guy.

K.E.L. - Well if you went back 50-60 years before Papez, Jackson and his people would have thought Papez was absolutely right on. It would have been so obvious to them that they wouldn't argue about it.

G.R. - In 1909, 1910, and 1913 a chap wrote 3 abstracts - (what was his name? - I'll get it) - talking about a system of central control of the nervous system based on 1885 papers that described the spinal reticular system.

K.E.L. - That was Bechterev?

G.R. - Yes - Bechterev. The original 188t paper dealt with the spinal reticular connections - the second order sensory systems which he didn't understand. Anyway these three abstracts appeared in the Ansage. Back in 1962 Sir Wilder (Penfield) was here - Bless his memory. I asked him "did you ever read- - - - - in the Ansage? - it is historic" - it was 1909, '10, and '11 or '10, '11, '13. He said "I have read all of those" and after a pause "I do remember those - they wer prophetic", and he admitted that probably he had come up with the centrencephalic concept based on a flash-back to something he had read years before in these three abstracts.

With the apprentice system - I think that is what graduate education is all about - one on one - the mentor system.

I may not have come up with what you wanted or expected but -

K.E.L. - You became his protege in a way. He was concerned with what -

G.R. - I don't think he was that concerned about what I would become or that sort of thing.

K.E.L. - But he was concerned about your opportunity to learn - wasn't he much concerned about that? That is why he would sit with you.

G.R. - I think back to my Ph.D. exams - my qualigying exams and the final. He said he wouldn't be here for that -

It was partly Jamie boy and partly Cornell - one of a few graduate institution. I count it as a privilege to have participated in that educational process. I turned out to be not a great research scientist. I suspect a lot of Jamie boy's graduate students turned out to be not great research scientists, but they were very perceptive members of academic.

I am not a compulsive research scientist who has to be out scrambling for grants.

K.E.L. - You got a lot of clinical oreientation in yor work with him?

G.R. - Yes - a tremendous amount. One thing that was embarrassing when I came down here to teach in the Medical School. This was a situation I really liked, and I immediately went out and started to learn neurology because I was now going to be teaching medical students. Starting in my second year here I taught junior year neurology for three years. Then I got involved in neuropathology.

Papez was the only man on the Ithica faculty who had any knowledge of clinical neurology.

K.E.L. - Did you see patients?

G.R. - Occasionally - he used to see a lot more - occasionally he would see a patient when I was there. The Chairman of the Department of Anatomy in the School of Veterinary Medicine. I have forgotten his name now - suddenly began to have foot-drop. Both legs became involved. He had progressive weakness. I was in the office when he came up to see Dr. Papez. Dr. Papez looked at him briefly and turned to me and said "do you want to examine him?" - "I think I know what he has Dr. Papez". He said "write it down on a piece of paper" - I wrote it down - I handed it to him and said "I concur - we will send him down to New York".

K.E.L. - What did he have?

G.R. - A midline saggital meningioma at the vertex. That was a classic neurologic syndrome - coming out first in the leg and foot area.

Jamie boy spotted it right away. We sent him down and they took the damned thing out. He came back and continued as Chairman.

As I said when I came here I was totally ignorant of clinical medicine. I hadn't been exposed to it - so I went to work to learn it. I was teaching clinical neurology and trying to keep a step ahead of them.

Then Dr. Earl came. I have cut over 6000 brains. I am a senior member of the American Association of Neuropathologists. I was the first associate member and was made a full member without having formal certification as a neuropathologist simply because of my experience - particularly with the gross - I am not so good at micro - though I can hold my own. In gross you can't touch me.

Then I have gotten involved in other things. I have published a number of papers, some very significant papers, in clinical areas - particularly bearing on neuropathology and neuroanatomy. Then I began to run out of ideas. I feel very strongly that just rinding out papers written just to generate money - so many of them are absolutely trivial - so I just kind of quit. It caused some problems here in my own faculty. I didn't get a lot of grant money by publishing paper, but I am a National Director of Sigma - X1 and on a number of major committees. I put the Society of Neuroscience together - I haven't been to the last couple of meetings - I keep the local chapter turning.

K.E.L. - It is too big now.

G.R. - Yes it is worse than the of frederation.

Then I have been involved in Nachting. I am an officer in the National Yacht Racing Association. I am President of the Bays Club. I spent 25 years working for the Republican Party.

K.E.L. - There weren't too many people of his contemporaries who had that sort of orientation, who were basic scientists and still had a foot in the clinical area and were talking about clinical pathology and clinical syndromes.

G.R. - Most of his students were involved in someway in clinical activities. A lot of people who came to him for advice were clinicians.

K.E.L. - Yes - Bucy came to talk to him about the temporal lobectomies in the monkey - the Kluver-Bucy Syndrome - and Webb Haymaker came to him to talk about the hypothalamus. Fred Mettler came.

G.R. - He stole ideas

K.E.L. - You say Fred was there before Bill Stottler?

G.R. - Fred got a lot of his anatomical ideas from Papez. His book is very Papez'y. He should have written it with Papez. I didn't get to know Fred well.

K.E.L. - You must have been there when Paul MacLean came to see him?

G.R. - I met Paul really quite sometime later. We had met at Ithica, but he introduced himself to met at one of the Cajal meetings. He knew who I was. Paul had a monumental memory for people and events. I was very fond of him.

K.E.L. - His career was certainly shaped by Papez from that first interview visit.

G.R. - and Karl's was too (Pribram).

K.E.L. - I don't have any clear picture of Karl's relationship with Papez. When was he there?

G.R. - Karl's background was tri-partite. He was with Paul Bucy and was very close to him. Then he came up to Yale and worked with Fulton and Yerkes. It is hard to figure out which was the dominant influence. He was with Papez in 1949 - I think in the summer, for about 6 weeks. He came up not specifically for Papez but to work out a major problem with Bill (Niemer), but he spent most of his time with me and with Papez. I had never met him before until I met him with Lidell. I was working out there then - to make a living. At that time he was conditioning behavior. They would separate one infant from the mother - one would be frustrated and one content. It was fascinating work. It was on a goat farm - all work in psychology. I learned you should never work with goats.

K.E.L. - This was like the primate work at Yale- Yerkes and his group, and John Fulton interested in the frontal lobes.

G.R. - And Bucy in the temporal lobe. Kluver got into all this. I think those of us who came with Dr. Papez were not run of the mill - we were all a little strange. We don't fit the standard pattern.

K.E.L. - You certainly weren't restricted in your perspective when you worked with Papez.

G.R. - Did I mention Ellie Henschell? Did you know how she got here?

K.E.L. - Yes - Bill Stottler told me she escaped from Germany.

G.R. - She was von Economa's chief technician. Before that she had been Bielchowski's technician. Bielchowski's silver inpreparation stain was developed by Ellie Henschell - not by Bielchowski. He knew nothing about the technology. When Bielchowski died she went to Constantine von Economa. When Constantine died in 1932 or 1935 his wife knew that she could not stay in Germany as a Jew. She would be dead. The only name she knew in America was James Papez because of long correspondence between her husband and Dr. Papez. So she called Dr. Papez and he sponsored her coming to the U.S. Ellie came with her. She spoke only fragments of English when she arrived. She was amazing. She never measured any chemicals. She couldn't be bothered using a set of scales or measuring containers of any kind. She was a real cook. She made her cresylviolet stain by holding the glass up to the light at a certain angle and adding to it until it was the right color. You are not allowed to measure. She taught me - you do it by feel. If in the basement of Simpson Hall somebody opened the bottle of acetic acid and then closed it instantly she would announce "someboty opened the acetic acid". She was an amazing lady. When Dr. Papez retired she must have 80. She went to the Brain Institute in Buffalo. The last I heard she had passed 100 and was still working actively, employed 40 hours a week as a lab technician. A remarkable woman. The most gifted lab technician I have ever known. She came in 1938, Mrs. Bielchowski arranged to get her out of Austria because she was Jewish. But one funny thing - she was never able to orient the section on the slide. She put the tissue on the slide at any angle, but she never learned a single bit of neuro-anatomy. Sections in the same tissue series would be mounted at all angles. She was not "smart" - she was "talented". She was tops.

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DISCUSSION WITH MRS. RUSSELL: (Joy Hillborn)

K.E.L. - When were you there?

J.R. - I never did - we were assigned advisors as pre-med students. He was assigned as my advisor, but in the same sense that he couldn't lecture - he was hopeless as an advisor unless you were especially "tuned" to him. He and Frank Freeman were teaching jointly the course in "Growth and Development". I ended up taking the course from Frank Freeman. He realized that Jamie boy was hopless for my purpose. So Frank ended up as my advisor in fact. It didn't take long for me to realize that medicine was not for me - but he was always my advisor.

K.E.L. - You had personal contact with him a few times?

J.R. - Oh, we talked a lot. He knew Glenn and I were going together and he adored Glenn. Glenn was a special person to him. Simpson Hall was not that large - we saw him a lot.

G.R. - You must go to Ithica and see Papez lab before they tear down Simpson Hall. It was on the third floor. A great big room. They certainly can't have changed it completely. The windows were up high on the wall. A big roll top desk in the middle of the room - it was a period piece. There were file cabinets and benches all around. It was cluttered - he was a cluttered man. He had an old wooden lean back chair with a high back. The desk was a disaster. I really regret that I don't have a picture of him sitting behind the desk - with papers everywhere. There must be a picture of the amphitheater where he lectured. The amphitheater was between his office and the end of the hall. On the other side of the hall was a huge space divided up into cubicles for graduate students,technicians etc. There was a door in the wall so you could go from the Professor's office to the lecture room.

K.E.L. - Apparently he would be lecturing and walk back into his office to pick up some slide or drawing he had thought of. He would keep on talking after he disappeared from sight and sound of the students. He would find what he was looking for an come back out into the amphitheater still talking but several paragraphs later.

G.R. - In all the time I was there I was in his house only two or three times and then really for business reasons. I was never invited there socially. I knew Pearl only very casually. She never came to the lab during my time - maybe earlier. She did all her work at home.

J.R. - I met Pearl several times but I don't recall in what context. They were very private people.

G.R. - They were very private - Minnesota, Norwegians. They didn't fit in the University Society. They had their own world. I don't think he ever joined the faculty club in all the years he was there. He lead his own life very quietly. He may have felt inferior in a way. He was just an M.D. in a world of Ph.D.'s. He never got a Ph.D. degree.

I remember when Professor Adelman wanted the old anatomy lab cleaned out after the medical school moved to New York. It was one of the more fascinating experiences - so Fred and I were down there with saw and knives. There were a lot of bodies in bits and pieces. The bodies are now all buried against the wall of the building. We cleaned out a huge refrigerator full of anatomical material.

In 1936 they began the transfer to New York. Jamie boy was not invited.

K.E.L. - I had had Hinsey as my Professor of Anatomy at Stanford. He left to become Professor of Anatomy and later Dean at Cornell. I talked with him about Papez, but he said he really had no contact with him.

G.R. - I could not imagine Papez getting up before a large class of Medical School students and lecturing. He would have been a disaster.

J.R. - He was there at the wrong time. In these days when papers are more important than teaching he would have been a gold mine, but then that wasn't the way to go.