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Paul Bucy

P.B. - Essentially the same ideas -
When this business of the symptomatology that develops when you remove both temporal lobes in the monkey - showed up, we wanted to see the extent of the extirpation - number one, and the degeneration which resulted from it - so we sectioned all the brains serially. We had them in both myelin stains and cellular stains. I had studied them and come to my conclusions as to what the changes were which are very extensive. So I got all of my material together and took it down to Ithica and went over the whole thing with Papez, and then if any questions came up he would get out his material - both human and animal. We would go over and correlate this, and finally came to a firm conclusion as to just what the changes were. He was a strange man in many ways. I think it would be fair to say that at first glance he impressed you as a farmer rather than a scientist. He was a loner. Although he had had people like Mettler who had worked in his laboratory, by and large he worked alone. He was very cordial and receptive. When I got in touch with him and asked if he would do this he said of course. I went there and spent a week with him, working on this material.

K.E.L. - These were the bilateral temporal lobe ablations?

P.B. - So although I had contact with him thereafter from time to time, there was nothing as detailed or prolonged as that first visit. It wasn't too long after that, he began to deteriorate mentally and got these funny ideas.

K.E.L. - The little intracellular bodies - the blue spots? Do you remember his talking to you at all about the 1937 paper on emotion?

(continued)

P.B. - Oh, yes. Of course that was the reason why I went there.

K.E.L. - That is peculiar, because as far as we can determine he made no reference to that paper in any of his writings from 1937 and '51 when he retired.

P.B. - He probably thought he had said it all.

K.E.L. - I think he was disappointed that he didn't get more response to the ideas in that paper. Bill Stottler who was with him from 1937-1941 said he was very disappointed. It came out at a time when there was concern about surgical intervention.

P.B. - Not very much in those early days.

K.E.L. - Yes, he was ahead of that. Moniz did his first publications in Europe in 1935 and 1936. For some reason people didn't pay attention to it - either because they weren't interested - or they disagreed with it.

P.B. - No, no, - neither. Some may have disagreed but they accepted it. The same thing was true of our monkeys when Kluver and I presented this to the faculty. By the faculty I mean the people at the University of Chicago. There was an informal group known as the Neurology Club - it had no organization - no officers - no news coverage. It consisted of all the people at the University of Chicago who were interested in the nervous system in one way or another. There was Ajax Carlsson, Gerard (Ralph), Lockhart, there was Herrick, there was Ben Kling, Bartholmetz, then the clinicians - Grinker, Bailey and myself, Earl Walker and what not.

K.E.L. - A really remarkable group.

(continued)

P.B. - Oh - that was the most stimulating group - each month we would meet and one of the group would present either his completed work or work in progress - and the rest would give it hell. And I recall that when we presented this material on bilaterial temporal lobectomy, Arnold Lockhart said "this is incredible - you have created psychopathic disease".

Well this was the trouble - we had. Of course the psychology people didn't like that and most of the psychiatrists didn't believe that emotional disease, psychiatric disorders had an organic basis. They weren't about to buy it either. This was absolutely contrary to their ideas and was just destroying whatever they thought, so there was no question about that. It was on a very emotional basis on their part, but it was there.

Well I'm sure that Papez experienced this very same thing. The only difference was that Papez had a hypothesis - a good, sound idea. But we had the lesion and the monkeys. And the change in the monkeys was reproducable over and over again. There was no question about that - there was just one difference. We were very fortunate with our first animal. This was an older female that had been in the laboratory of George Bartholmetz. She had become vicious. Oh, God she was the most vicious animal you ever laid eyes on. It was dangerous to get near her. If she didn't hurt you she would tear your clothes - just nasty. Well she was the first one we did. One afternoon about five o'clock I removed one temporal lobe. I think it was the left one but it really doesn't matter. We tried that out later. It made no difference which one you took out first.

The next morning I got to the hospital and -

Well maybe you know or perhaps you don't know - the reason we got to doing this. We weren't trying to find out what removal of the temporal lobe will do - or the change in behavior of the monkeys it would produce - that was all unexpected. Heinrick (Kluver) as you perhaps know -

K.E.L. - I never met him.

P.B. - Well, he was something. One of the most brilliant men and most knowledgeable that I ever knew. He had been experimenting with mescaline and had taken it himself, and had experienced hallucinations and had written a book - I think the title is - he called it "The Divine Plant". Well, then he gave the drug to his monkeys. He gave everything to his monkeys, even his lunch. He noticed that the monkeys acted as though they had paresthesias of the lips. They licked, bit and chewed their lips, so he came to me and said "maybe we can find out where mescaline has its action in the brain. So I said "o.k."
Well we started out by doing a sensory denervation of the face - we did but it didn't make a damn bit of difference. All right, we will do a motor denervation. That didn't make any difference either. Then we had to sit back and contemplate this - where next - and I said to Heinrick - "this business of the lips and the mouth is not unlike what you see in some cases of temporal lobe epilepsy - they chew and smack their lips and so forth - so lets take out the uncus". Well we could just as well take out the uncus and take out the whole lobe.
So I operated on this vicious monkey one afternoon about 5.00 o'clock, and of course following the operation the animal was still asleep and you couldn't see anything. But the next morning my phone was ringing like mad. It was Heinrick on the other end saying "Paul - what did you do to my monkey?" - I said - "what do you mean?" He says - "she is tame!" Subsequently in doing more ordinary monkeys that tameness with removal of one temporal lobe - that was never quite so striking. So that was one of those fortunate happenstances - that stimulated our getting the other temporal lobe out as soon as we could. Not immediately of course - we waited awhile to evaluate her. When we took out the other temporal lobe the whole picture blossomed.

K.E.L. - That was the Kluver-Bucy syndrome. It was a remarkable case of serendipity arising from Kluver's curiosity about mescaline.

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P.B. - Well, we never got back to the mescaline problem. This temporal lobe business was taking up so much of my time and Kluver's that we never bothered about the mescaline.

K.E.L. - Well now Papez must have been fascinated with that whole story.

P.B. - Yes, he was. He had lots of pathological material that we could refer to. He was so familiar with it that he would just go and pick out a slide that dealt with the specific pathway and the particular point we wanted to answer.

K.E.L. - He was interested particularly in the thalamus from his notes and drawings that we have and in the descending tract of the trigeminal nucleus and its connections.

P.B. - Yes - his interests were very broad as far as neuroanatomy was concerned. You know, he had that collection of brains of distinguished people.

K.E.L. - It was the Wilder collection?

P.B. - I don't know the name but these were brains of distinguished people - distinguished scientists, distinguished politicians - people who had shown some unusual intellectual capacity one way or another. The assumption was that it would be possible to find out why they were exceptional. But what was surprising was that all of them had some abnormality - a little bit of infarction here or there or evidence of old trauma or something else. But all of these were lesions which Papez could study carefully - all of them produced degenerations that he could trace and examine.

There is a break in the recording here. The discussion shifts to Percival Bailey's laboratory in Chicago.

P.B. - He was a good teacher.

K.E.L. - You say he came to work with Percival Bailey for a year or for sometime in Chicago.

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P.B. - Yes, and thats where I got to know him. Of course there were lots of people coming and going in Bailey's laboratory - it was almost like Grand Central station at times.

K.E.L. - Did you know Malone?

P.B. - No, by name only.

K.E.L. - He was also from Cincinnati apparently. Paul Yakovlev was interested in him because Malone had spoken very early about the concept of the reticular formation. He had done some work in Germany in Berlin.

P.B. - Well the first real look and understanding of the reticular formation, on the map - was Magoun, but his interest was activated by Ranson who was a very remarkable man.

K.E.L. - Yes, Magoun has recently written a paper on Ranson's laboratory. It is very interesting.

P.B. - Well you know Magoun is an example of how insitutions just don't have any sense. They had this remarkable laboratory and Neurological Institute at Northwestern which Ranson had created. I don't think it was heavily funded but it was there and alive. When Ranson died, Magoun was there to carry on, but did they offer him the position that Ranson had - with support to carry on his research? No, they didn't. He didn't have support there, so he came over to the University of Illinois and worked with Bailey and then of course, he got the offer to go out to Los Angeles - to U.C.L.A.

Another example of the same thing is Bailey. Fifty years ago when Bailey went to the University of Chicago he and I went there together in 1928. He had the concept of a Neurosciences Institute and there were the people there to do it. There was Herrick and there was Carlsson, and Lockhardt, Gerard, Roy Grinker, and the whole bit, but the God-damned University never could see this. And so after struggling with this for eleven years, Bailey finally threw up his hands and said "to hell with it". I went on several occasions before he finally quit to talk to the Dean, to talk to the President of the University who was that ignoramus Hutchins - Robert

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Maynard - he was terrible. And I got no place - just no place. Bailey had the capacity to do it, and would have created a first rate Neurological Institute.

K.E.L. - That was Bailey, but the principle also applied to Magoun?

P.B. - There was a difference. Bailey wanted a complete basic Science and Clinical Science Institute. Magoun's idea was Neuroanatomical and Neurophysiological. I have no fault to find with this - but in fact neither one of them got to carry on.

K.E.L. - I had tried to get Magoun to participate in that little symposium we held in 1976, but he opted out.

P.B. - I don't think that was his interest - the Limbic System.

K.E.L. - Yes, but the reticular system was the link. I think the Limbic System is the processor and analyser of all the things that go on in the reticular core.

P.B. - I wish to hell I knew what it was.

K.E.L. - I know that is going beyond the "hard" facts but it is useful perhaps to think of it that way - Magoun and Moruzzi's observations on the Reticular System changed our thinking about the whole problem of looking for specific lesions - specific clusters of neurones that did such and such - to the idea of complex circuits of millions of neurones, whole territories of activity that had general effects - tuned things up and tuned things down to modulate performance. I think that was a major step.

P.B. - Oh, yes. Magoun made, I think, one of the major contributions to neurological thinking, but I think he was also right to stick to his own machine

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and not try to branch out. It is funny but one of the most brilliant men I ever knew - very receptive to ideas - very thoughtful and very productive was Carlsson. Old Ajax. Well you know long before Magoun came out with this bit about the reticular formation. After my work with Fulton it became obvious to me that our concept of the function of the pyramidal tract was in error. It was obvious why we had made the mistake, but it was an error. Back in Chicago in the early 30's I presented this idea to the Neurology Club - that the pyramidal tract was not responsible for muscle tone, for control of the activity of the tendon reflexes and so on and so forth. Carlsson was furious - he didn't like that. Then Magoun comes out with a control system for muscular tone through the reticular formation - threw the pyramidal tract out. But that was obvious to me from observations made before I ever went with Fulton.

We were sitting down very much like this - maybe it was breakfast maybe it was some other meal. Anyway we were talking, and John said to me "you know there is something funny about these monkeys. He was of course concerned with the cortex. Some of these animals in which we removed the pre-central region - some of them have paralysis which is flaccid and others have a paralysis which is spastic. I can't understand it". Well in my naievete at the time I said "well that's easy" - he said "why?" I said well your spasticity is coming from area 6 and your flaccidity from area 4". Well this is not completely true - the idea is alright but the specification isn't that great. But that was the idea and when I went with Fulton we made the same observation but we didn't carry it on far enough to determine what it was that caused the spasticity. Magoun did that, but that whole thing was evolving and developing at the time.

K.E.L. - I remember you said earlier that the Papez formulation was anticipated by Herrick (Judson Herrick) and really formulated in general by Herrick.

P.B. - Oh, yes.

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K.E.L. - As a matter of fact almost the opening paragraph in Papez 1937 paper was a direct quotation from Herrick about the orientation of the medial and lateral walls of the hemisphere in relation to the thalamic nuclei.

P.B. - Oh, I wasn't saying that Papez hadn't acknowledged Herrick. He did of course.

K.E.L. - Why didn't Herrick's concept take hold?

P.B. - I don't know. Probably there were several reasons - one, the neurologists and psychiatrists simply refused to accept this as I have already mentioned, and as Herrick's idea was pure theory, based to be sure on his anatomical observations, but without having physiological or psychological support - they were not about to accept it. In the first place they did not know there was a brain. They certainly didn't know anything about neuroanatomy - so this didn't impress them, and as it was a theory rather than a fact that didn't impress them either. Then I think another reason is that this is not very widely publicized. Sure he published it, and that was it - there it is in print - go ahead and read it, but he never pushed it, then or in subsequent publications.

K.E.L. - Then it was in an anatomical journal which did not have much impact on the clinical world.

P.B. - Well I can't tell you now where it appeared, but probably the Journal of Comparative Neurology which did not have any wide circulation amongst clinicians or even amongst physiologists. It was an anatomical journal, and worse yet it was an animal anatomical journal.

By the way you knew didn't you that Herrick and his brother started that Journal? His brother really. He died of tuberculosis early in life. They were together there at Denison University in Ohio. His brother started this Journal of Comparative Neurology. After his brother's untimely death

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in his 30's Judson Herrick took over. He was associated with his brother in it from the start, but the brother was the prime mover according to Herrick. But Herrick continued it and made its name. You know the two brothers printed the first issues on a printing press in the basement of a University building. Then of course it was later taken over the the Wistar Institute and went on its way.

K.E.L. - If you go back even further in terms of what the Psychiatrists and Psychologists thought about this, you wonder where this great schism arose. Hughlings Jackson, and Ferrier and the leaders of Neurology in their era included psychiatry, psychology, behavior emotion and everything else as expressions of brain function, all contained in the apparatus.

P.B. - And Wier Mitchell.

K.E.L. - What broke up that rational view of the brain?

P.B. - Ignorance largely.

K.E.L. - Was it Freud?

P.B. - Oh, Freud was just the tail on the end of the donkey. To be sure his acceptance and popularity finally put the kybosh on the ideas that the brain had anything to do with behavior or emotion or anything else. Did you ever read Bailey's book on Freud? You must. Bailey finally made up his mind that he was going to bring down that bastard so he read in both German and English everything Freud had ever published, then he wrote his book and condemned Freud with quotations from his own writings.

This book of Bailey's exists both in English and in French. I keep getting the titles mixed - one is called "Sigmund the Unserene". The French is called "Sigmund----the word escapes me. They are the same book with one exception. The French edition has a preface that is written by a Frenchman - that is not in the English edition. You must read it completely.

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K.E.L. - I wonder whether Sherrington didn't have a lot to do with this schism between psychiatry, psychology and neurology?

P.B. - Why?

K.E.L. - Well you know he said "the mental process is not examinable as a form of energy".

P.B. - Oh, yes - these papers he wrote late in life - they were terrible.

K.E.L. - Yes, but they raised so many questions and he was such a revered figure - he was a God. And Penfield took the same position, and Eccles and so on. This discontinuity in concept of how the brain works was not present in the thinking of the leaders of the generation before that. It wouldn'thave made sense to them - was it because of our technological advances at that time? I have wondered it it was because of the expectations - for instance, when we got the cathoderay oscilloscope and could display and measure the nerve signal - we should be able to understand the whole damned thing, But that assumption did not pan out, so then they said well you can never decipher the mental process, or emotion and behavior. That sense of disillusionment about higher processes fit with Sherrington's position. Sherrington dominated the thinking of that time and the type of questions we were asking.

I had a funny story from Ebbe Hoff about that, when he was in Sherrington's laboratory. Ebbe had been reading some of Pavlov and in a relaxed moment in the laboratory spoke with Sherrington suggesting that he would be interested in knowing something more about Pavlov or even in going to pay him a visit. Shrrington said to Ebbe "well if it is just a matter of curiosity, that is o.k., but if you are doing it because you are interested in the physiology of the nervous system it is a waste of time!"

P.B. - Yes, if it wasn't a reflex it wasn't significant - no good.

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K.E.L. - I think that set us back terribly.

Some diversion about the Hoffs - Ebbe, Hebel and a sister Nebbe (?). They were all at Yale when I was there in 1933 - K.E.L. - my father was there then working on his book on the visceral nervous system. P.B. - yes I met your Dad there but at that time he was visiting, not working there.

P.B. - I was in Fulton's lab in 1933.

K.E.L. - Well you must have known the two famous Chimpanzees Becky and Lucey. They must not have been operated at that time because I think Fulton reported on the operation in 1935.

P.B. - Yes, he reported on them in London at the International Neurological Conference.

K.E.L. - Were you at that meeting?

P.B. - No.

K.E.L. - But you knew Becky and Lucy?

P.B. - Yes, oh yes.

K.E.L. - Had they become neurotic at that time - when you were there?

P.B. - Yes. Carlyle Jacobson was doing discrimination testing on them and the tests were getting more difficult all the time. The choices were getting more difficult until finally the Chimps broke down - they refused to go to the testing room - would lie down on the floor throw themselves around and have a temper tantrum just like a kid you know. They would bite and scratch and fuss to avoid going to the testing room. Then they took out their frontal lobes.

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K.E.L. - And their behavior smoothed out?

P.B. - But they were no better than they were before - they made the same or maybe even a little more mistakes - only now they didn't give a damn, and this is what attracted Egaz Morriz. He said "if you can do that with the Chimps why wouldn't it work on my patients?"

That was a thrilling period. You knew John and worked with him so you know.

K.E.L. - I knew him but didn't actually work with him. That was my brother Bob who was there in the lab. I was in Boston and we worked together on some frontal lobe stimulation studies on patients undergoing lobotomy in Boston at the Psychopathic Hospital.

P.B. - Well, John was a most remarkable man. He had one fault which Alexander Forbes commented on. He said to me "you know John is a brillian guy - he has ideas just tumbling out of his head, but I wish that someday he would sit down and think one through before he writes it up" - which was true, but he was a tremendously stimulating person. He had enormous influence but a greatly underrated man."

this is the end of this recording with Paul Bucy