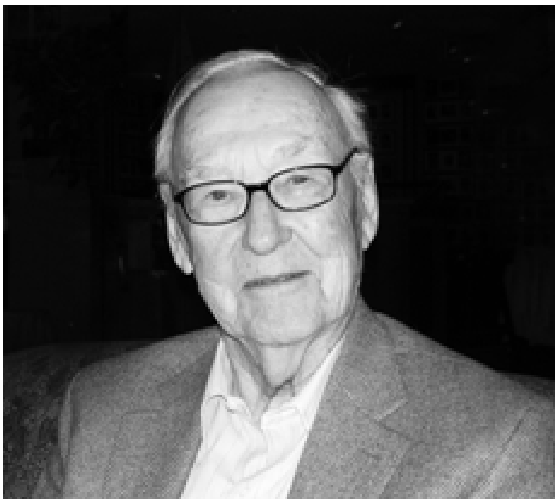
James Crow



Personal Details	
Name	James Crow
Dates	Born 1916
Place of Birth	United States (Philadelphia)
Main work places	Madison, Wisconsin
Principal field of work	Theoretical, population genetics
Short biography	See below

Interview	
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Personal Scientific Records	
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Biography

James F. Crow was born on January 18, 1916 in a Philadelphia suburb. At the age of two he moved to Wichita, Kansas, where he received the A.B in 1937. He then went to graduate school at the University of Texas, receiving the Ph.D. in 1941. He taught at Dartmouth College from 1941 until 1948 and since that time has been at the University of Wisconsin. He has worked in both theoretical and experimental population genetics. He is now Professor Emeritus, having retired in 1986.

INTERVIEW WITH DR JAMES CROW, 24th OCTOBER, 2005

PSH. It's Monday 24 October 2005 and I'm talking with Dr James Crow in his office in the new Genetics Building at University of Wisconsin, Madison, USA. If I might Jim, I know you have done several interviews before so I don't want to duplicate things, but because I've not been brought up in the American system it would be very helpful just for me to perhaps start near the beginning. So could I ask first of all, when and where were you born and brought up?

JC. I was born in a Philadelphia suburb in 1916. Actually I'll tell you more. I was due in January 1916 and that's also the year the Journal, *Genetics*, was due. I arrived on time and the Journal was a few months late. But we both date back to 1916 and I was, as I said, born in a Philadelphia suburb. Then at the age of two, I moved to Wichita, Kansas, where my father was a biology teacher and I participated in the 1918 flu epidemic and barely survived. I grew up in Kansas and went to a small Quaker College in Wichita, and then got a Fellowship to the University of Texas for graduate work.

PSH. This was Austin?

JC. Austin, yes. And then my first job, my only other job, was at Dartmouth College. I started graduate school in 1937, finished in '41. I had ambitions then to get a Fellowship with Sewall Wright, but then this was the beginning of our participation in the war and it seemed unreasonable to try to do fellowships at that time, so I took this teaching job at Dartmouth and stayed there for seven years. I came here in '48 and have been here ever since.

PSH. Can I just ask you about your time in Texas? I mean the years when you were at Austin, which years were those?

JC. 1937 to 1941.

PSH. And was Painter head of Zoology then, or was he President of the University by that time?

JC. Neither by that time. He was in the department of course, and it was a small department. I saw him every day but it was shortly after I left that he became President.

PSH. And was he still then, may I ask, actively involved in chromosome work or had he moved on?

JC. No, he was involved in chromosome work and regularly came to the laboratory. He was doing salivary chromosomes and was interested in the structure of these chromosomes, the cable like structure. That's what I remember best about what he was doing. He no longer had any interest in human chromosomes as far as I know; I did see some of his slides, I told you that last night.

PSH. One other person who comments on those slides; in T C Hsu's book, he mentions he saw the slides and that his reaction was that he was amazed that anybody could draw any conclusions from them because the chromosomes were so tangled.

JC. I could say the same thing. The only clear thing I saw from those slides was what I said last night. With this precocious separation of the X and Y, and although nobody has ever told me this, I think it's the most plausible explanation of why everybody was so sure that the number was 48. Because this was the haploid number of 24; if you count the X and Y as separate, as tetrads.

PSH. Yes. Do you think in Painter's work, was he mainly looking at spermatocytes or spermatogonia? Do you have any sense as to which he would do?

JC. Yes I do. He decided, sensibly, that by studying meiotic chromosomes he'd only have half as many, so he spent more attention to testis cells than anything else. And he got testis material, he talked about it even after I got there, from some of the patients at the mental hospital there. They were castrated for probably the wrong reasons, but anyhow he got the material that way.

PSH. Coming to when you came to Madison, who was here at Madison when you arrived in 1941?

JC. '48.

PSH. '48, beg your pardon.

JC. The most influential person for me and the main reason I was especially happy to come here, was Joshua Lederberg. He and I were very close friends, still are, and we saw each other every day. There was a 10 year period when he was still here before he went to Stanford. L.J. Cole died before I came here so I never met him. I only inherited his desk, but the Chairman of the Department at that time was R A Brink. Irwin was in the department and it was several years before Sewall Wright came. Irwin, Brink and of course Lederberg were my principal influences.

PSH. Was there any work at all on human genetics going on at the time you came?

JC. No none.

PSH. And was there any encouragement that was given to you to take it in that direction or did it just happen?

JC. I didn't get encouragement, or discouragement either. The foundation of the Department of Medical Genetics was really Joshua Lederberg's doing. We had a new Dean who came during that period who was interested in genetics. His name was Bowers and he got Joshua interested or Joshua got him interested, probably, mutual, in starting a department of Medical Genetics. So they did, but one of the things about this department is, that it was Lederberg's view that Medical Genetics should not only include the human genetics but the genetics of human parasites. So microbial genetics was definitely a part of it. To back up just a little bit though, although I wasn't especially interested in human genetics, and nobody was pushing it, one of my first students, Newton Morton, was genuinely interested in human genetics and wanted to go into that field. He worked with me for a year, maybe two years, on a Drosophila problem. But since I knew he was interested in human genetics I got him a position with Jim Neel at Hiroshima. There was a second reason for thinking this was a good idea. His wife was Japanese and I thought it would be just the right thing. Actually he didn't stay there very long. He intended to stay two years and was back in a few months. It turned out that he and Neel didn't get along. My hypothesis, and I think it is probably correct, is that Neel had all sorts of plans for many years ahead as to how to do this Japanese study and Morton was full of ideas, then and now, and didn't hesitate to say them. I'm sure he suggested all sorts of ways of redesigning this that Neel rejected. In any case he didn't stay there very long. But from my standpoint one of the most important things he did while he was there was to discover Kimura, and he sent me some reprints from Kimura. I was most amazed by the depth and the mathematical details which I couldn't understand, but I could appreciate.

PSH Was Kimura involved at that time internationally, because Japan must have been in some ways still very isolated after the Second World War?

JC. He certainly had no reputation outside Japan, or in Japan either for that matter. He graduated from high school during the war and was able to enter the University of Kyoto. He had a very outstanding record and probably could have gone wherever he wanted to. He went to the University of Kyoto and on the advice of Kihara he went into the Botany department. I might have thought he would work for Kihara, but the reason was purely survival. The students in Botany were deferred until the end of the war or until the end of their education, so that kept him out of the Army. He was very interested in Sewall Wright, Fisher also, but especially Wright, and on his own he read Wright's papers. Something that would be fun for you to see actually are his copies of Wright's papers. They didn't have copying machines of course in those days, so he copied them by hand and put in his own interpolations into it. In some ways he improved on what Wright did. It was really a joy for me to view that when I saw it.

PSH. Are those records all archived, do you reckon, here?

JC. They aren't here. Some may be in Michigan and I suppose they are archived. I hope they are. I've never enquired into it.

PSH. So then Newton Morton came back from Japan and then came back to Madison, am I right?

JC. That's right. He and Kimura were students at the same time.

PSH. So then Kimura came over for a spell?

JC. A little later. Morton came back, probably in 1952/53 and then, I believe it was 1953, I ran into a Japanese person in our Union here. The Genetics

Society was meeting in Madison that year, and he had lost his way. I asked if I could help and he said his name was Kimura. I then asked him if he was the Kimura who had written these articles and he was. I'm probably one of, there can't have been more than one or two other people in the United States, who even knew about him. He was on his way to Iowa State. He had applied to work with Sewall Wright and Wright said he was soon retiring. He hadn't quite decided whether to come to Madison yet, but anyhow that was in the offing, and so he recommended that Kimura go to Iowa State where Lush, who was a Wright disciple, was teaching. It turned out Kimura really didn't like Iowa State. He didn't like the direction of the research there, which was mostly analysis of epistatic components of variance, and he really wanted to work on stochastic processes. So about midway through the year he wrote and asked if he could come as my student. I was hesitant about this, because I couldn't teach him anything about mathematics; I knew that then and it has been reinforced since. But I also I knew by that time that Sewall Wright was coming here, so I thought that would be a perfect opportunity for him to come here, so I accepted him as a student

PSH. So am I right then, by that point, Newton Morton was also back here and Sewall Wright had moved over to here and then you had Kimura here.

JC. Quite a power house I would say.

PSH. Did all of them interact or did they kind of work pretty independently?

JC. They interacted in the conversation sense, among all four of us, or any sub set of them. But the work was quite independent. Kimura, although he admired Wright very much, worked in his own way and so therefore made very little use of Wright's work. They would talk of course, but I don't think Wright made much of a contribution to Kimura's work other than what he had already written long before. Morton was interested primarily in human genetics by this time and again, everybody used Wright's inbreeding coefficients and that sort of thing, but he didn't profit very much by Wright. Nor for that matter did I. For a while I had lunch with Wright every day and we talked about everything, but we never did write an article together, partly because Wright was very independent in his own work, self-sufficient.

PSH. Is it your feeling that most people in mathematical genetics and mathematics generally tend to think inside their own head and be rather independent in that way?

JC. I think that's true, yes. Certainly Morton and Kimura never wrote anything together and each did his own work. They were in somewhat separate fields. As to my own relationships with mathematicians, because I've had some very good mathematical students, and I've worked very well with them starting with Kimura and Morton and later on with Nagylaki and other graduate students. The working relationship really was that I would think of the biological aspects of the problem and then they would do the mathematics. And that makes a very nice team, but I don't think there was ever any collaboration in a strictly mathematical sense. When I was working with Morton, trying to devise ways of measuring Wright's effective population number from census data, Morton and I, and mostly myself in this case, had worked out the methods for doing this. But we weren't sure it was correct and Kimura I remember helped us by independently deriving the same thing.

PSH. What year was it then that a specific medical genetics department was formed here?

JC. It would probably be 1958, could be '57. There was a symposium organised at the time this happened, which was published in the Journal of Medical Education.

PSH. Who did you say organised it?

JC. Joshua Lederberg.

PSH. So I am intrigued that people who were so non-medical in many ways in their outlook should have wanted to start a department of medical genetics. How did that happen?

JC. Well it's the philosophy, well let me say a little bit about the organisation. It started with a two person department, Lederberg and Morton, and that lasted for a year, not much longer. Then Lederberg went to Stanford and so I was appointed to take his place and by that time we had evolved a philosophy, which was that this would be a basic science department. We would try to get joint appointments, collaborations with the clinical departments and that was only partially successful. John Opitz is the conspicuous success, but we never were able to get the kind of joint appointments with medicine or any of the other clinical departments.

PSH. Do you think you were ahead of your time?

JC. I don't know whether we were ahead, but it didn't work. It certainly didn't work out and though it's been a good department all along. it hasn't been clinical particularly. Then genetic counselling came in more and more, at first with Opitz and then with Joan Burns and Renata [Laxova]. Although these were department members, they functioned quite independently with the rest of the department, both geographically and intellectually.

PSH. So thinking of the main people who have been involved with you here, first of all John Opitz, who I haven't had a chance to talk to myself yet. When did he come here, roughly?

JC. Early sixties I would say. I don't think he was a faculty member, I think he was a post doctoral fellow. He was in paediatrics in any case, and it was clear that his interest was in genetics and so we had some sort of a joint arrangement, I have forgotten all the details about this. But he essentially started the work in morphological human genetics and as you know he knew, then and now, an enormous amount about rare genetic diseases. He is a bit of a McKusick type in knowing all these things.

PSH. Did he interact much with Dave Smith when Dave Smith was here?

JC. I believe David Smith had gone by the time he came here. I may be wrong about that.

PSH. Right.

JC. The interaction with David Smith was almost entirely with Patau. I believe by the time Opitz came he had been gone. That could be checked but that's my memory.

PSH. Am I right that David Smith himself never really had any formal genetics training?

JC. He was doing what Patau told him to do. Of course he was a good clinician. I'm sure he was the one who identified these two trisomies by finding them at the hospital, because I think he's the one who went to the hospital. Whether Patau actually went there or not I don't know.

PSH. Coming onto Klaus Patau, and you've already given me a lot of information, but you told me he started off in pathology. Was he working on cytogenetics already while he was in pathology, or did he then switch back to it when he came to genetics?

JC. Well he was working on cytogenetics but not necessarily human. He was interested in DNA measurements, by optical methods, and that's where his main interest was. It wasn't until this trisomy possibility turned up, and that I brought it up with him, that he switched to human genetics.

PSH. So the Department of Pathology, was Patau's role there just basic research or was this a clinical pathology department?

JC. It was more a basic department. It was the laboratory of pathology largely, and I don't think Patau had much to do with the workings of the Department. He carried on his own research programme and I don't think he did any teaching. If he did it wasn't very much. And during that time, he and I knew each other. He came here in the Botany Department first, as I probably mentioned earlier, and then switched to Pathology and I'm not sure what the reasons were. This all happened without my knowing anything about it, but I of course saw him regularly whatever his nature of his appointment and then as soon as we got news of this human genetics problem, then I arranged it so that he joined the Genetics Department, so he switched from pathology to genetics after the trisomy discoveries.

PSH. Was that then the beginning of what you might call a Clinical Cytogenetics Service? Did Patau develop diagnostic cytogenetics on a wide basis or was it just his research interests in areas like the trisomy?

JC. Well it was a pretty narrow basis. He had some hope for trisomy mapping for example, for finding partial trisomies and identifying the traits. He was also very interested and involved in these discussions that went on as to how much and how well you could identify individual human chromosomes. He attended several meetings on this subject, for it was a hot subject.

PSH. I have read his paper, which I think is 1960, on that and am I right he was pretty upset not being asked to go to the Denver conference?

JC. I don't know whether he was upset or not. He did mention the fact that he wasn't there, but I don't know why. I don't know the story here. Patau was quite self sufficient so I don't suppose it made that much difference to him, but I don't know this.

PSH. The story I heard was that the people who were invited were one's who had already published a human karyotype by the beginning of 1960, so maybe he . . .

JC. Maybe he hadn't done it. He did go to a lot of meetings though, although not that particular one.

PSH. I mean his classification in terms of grouping of chromosomes and his paper on how you can and, at that stage, how you couldn't, clearly separate individual chromosomes, I think is very valuable.

JC. I think so too and he was ahead of the field on that particular subject at that time; because he was more influential than anyone else in saying that within a group, you really couldn't tell which one was which, and that people who were claiming that were probably engaged in wishful thinking.

PSH. What about his subsequent work? Did he stay strongly in the human field or did he go back and continue with his more basic work?

JC. He stayed in the human field and he worked with Eeva [Therman]. They were going to do what they called a blind study, which was just taking people as they came in and they tried to look for a non selected sample. The numbers were never very large. I don't think an awful lot came out of this but much of the latter part of his stay here was devoted to that. And then he died prematurely. I don't remember what year it was but he developed a prostate cancer, it metastasized to the bone, and he didn't live very much longer. He was never one to publish very much. He published quite a bit during the period when there was great excitement about human chromosomes, but before that and after that he was reluctant to publish.

PSH. I suppose in those days publication wasn't quite the absolute essential that it is now.

JC. I think that's right, although there would be some criticism of him for not publishing more, and Cotterman even more. We had two non-publishers in the Department.

PSH. So if I come back again to Cotterman, who, before you gave your talk last year, I was terribly ignorant about. When did he come then, to Madison?

JC. It would probably be in the early sixties.

PSH. So already there was a Medical Genetics Department?

JC. We already had a Medical Genetics Department and at that time - a little more about the history of the Department. Newton Morton and I were the Department for a while and I think the first appointment was Patau's shift into the Department and then the next one we got was Oliver Smithies. I already knew of his work and I thought he would be a great one to recruit so I worked very hard trying to persuade him and it turned out I didn't need to. He wanted to come here anyhow so it was a very short conversation, and then the next one to come . . .

PSH. Where was Oliver Smithies working then before?

JC. He was in Canada at that time.

PSH. He was in Canada having moved from Britain to Canada?

JC. Yes. He even had spent some time in Wisconsin, but I didn't know him at that time, in the Physical Chemistry Department here at an earlier time. Before that, with respect to his move from Britain to Canada, I don't know.

PSH. Was he involved, when he came here then, I always associate him with very basic biochemical genetics rather than specifically human studies but maybe I'm wrong with that.

JC. Well I think you are right about what his viewpoint was, but he always was interested in medical applications, even though he was not clinically trained himself. He, probably more than anyone else in the department continually emphasised the possibility of medical applications. It was his discovery of the starch gel method and then the useful thing to come from that was the haptoglobins and that immediately thrust him into human genetics. So after he came here he worked on haptoglobins for a while and then into haemoglobins and other kinds of globins, so he was entirely in human genetics during the time he was here.

PSH. And am I right, he is still living? Is that correct?

JC. Yes, he had left here, much to my regret, maybe ten years ago. He is in North Carolina now. The reasons for his leaving were the kind of thing that often happens. He had this lady friend, who was a very good scientist in her own right, but there was never a faculty position for her here, so he went to a place where they could both have faculty positions. I think it was a mistake on Wisconsin's part not to cook up something for her.   And I will tell you a curious anecdote. One of Smithies' students, Walter Nance.

PSH. I know Walter.

JC. You know him. Well Walter told me one time that he thought if I had still been Chairman when Smithies left, I would have found a way, probably illegal, for keeping him here. But I certainly would have made a big effort to do it.

PSH. I'm sure you would have been able to make it legal.

JC. I would have found some way. I think the Genetics Department didn't show very much leadership in trying to keep him here. Anyhow the next appointment after Smithies was Demars because I thought, we all thought, that tissue culture was the coming field, so we got him to develop that area. And then I guess Cotterman was the next appointment and that sort of finished the growth. About that time Opitz switched allegiance, not necessarily full-time. He retained a membership in Paediatrics.

PSH. So altogether then by, I suppose, the mid sixties, you had quite a large and broad team?

JC. It happened very rapidly because we had a programme project grant from the National Institutes of Health that would pay some salaries and I was able to, what they would say nowadays, leverage, this money with the administration to get these appointments, so we grew from two people to six people in just two or three years.

PSH. Coming back to Cotterman, was he medically trained?

JC. No, not at all. He was a PhD and his thesis was in combinatorics. Very clever, very basic and never published, as was characteristic of him. It was probably the most widely cited, unpublished thesis, of Ohio State. And Snyder was the human geneticist at Ohio State and I don't think he had much influence on Cotterman, except to recognise how talented he was, and encourage him to do what he wanted to do. Then Cotterman moved to Michigan and started this Human Genetics clinic there.

PSH. That's interesting. So he was able to start that even though he wasn't medical?

JC. That's right and I think, there's a name I couldn't remember last night and I can't remember now either. It's a man who worked with rodents, Peromyscus, especially, who was at the University of Michigan who once started a heredity programme and I think he's the one who brought Cotterman there. I'll think of the name sometime and send you a note. (It is Lee R. Dice)

PSH. I'm interested because that links back with a phase when most of the genetic counselling was being done by basic geneticists.

JC. That's true. I even did some myself and then I realised, and I think everybody in this business realised, that often the genetics is not the major problem, it's the clinical knowledge that really matters. So I don't think I counselled more than three or four people before I realised this is not the way for PhDs to behave. I had a few that were just clean-cut genetic diseases where there was no doubt, Huntington's for example.

PSH. So when Cotterman came to Madison, what were his main areas of interest that he was developing?

JC. Well my main interest in wanting him to come here and Newton's too, was to develop immunogenetics, and at that time he was doing such studies, so he came here from Dallas. He had been in the blood lab there and so we equipped a laboratory in immunogenetics for him to work in and then he never did much as far as that was concerned. He carried out a few experiments but mostly he went back to what was really his great love, mathematical combinatorial genetics.

PSH. Was that around the same time as he began as first editor of American Journal of Human Genetics?

JC. That was a little earlier. He was still at Michigan at the time when he edited the Journal. Well, he moved from Michigan to California for a while and I believe that happened during his period while he was editing the Journal.

PSH. From what you said in your talk it was, and I can understand this, it was something that rather got on top of him and the administrative side just overtook everything and he couldn't quite cope.

JC. Exactly.

PSH. Do you think that affected his research as well, probably?

JC. I don't know. He was a free spirit and it is hard to say what affected him. As I think I have said, this was perhaps the most carefully edited journal that ever existed during the first three or four issues and then he gradually lost interest. Then it got further and further behind and then he was finally replaced by Strandskof who could take care of the details, which Cotterman never would.

PSH. When was it that you were able to get Joan Burns and her genetic counselling programme going? I'm sure she told me that just now but I've forgotten already.

JC. She would remember the dates better than I would, so I can't really say. She was a student of mine in Drosophila genetics earlier and then as I said, went out and had a family and then came back with an interest in genetic counselling, but I'm vague about the time.

PSH. One of the questions I asked her was whether she felt having had a primary genetics degree first, helped her to get her programme more accepted by the professional geneticists. She said she thought it did.

JC. I think so too. She was already known to most people in the department anyhow.

PSH. The other person, and again I will be seeing her later, what year was it Renata [Laxova] came over to Madison?

JC. Again I don't know what year it was.

PSH. I shouldn't be asking you what year, but roughly when.

JC. Well I'll tell you the circumstances. It was that John Opitz, she essentially replaced him, and he was getting increasingly dissatisfied and there was some reverse dissatisfaction with him too. He finally decided to seek this position in Idaho. So that left a vacancy and though I do remember the circumstances, I can't tell you what year it was. Then we hired Renata right away to fill that niche. I thought, and I think it's correct, that for a role in genetic counselling she was far better than John Opitz was. She had a real feel for nosology and there was some criticism of John for being more of a memory artist than a deep thinker. He did have an astonishing memory, especially for syndromes and diseases and of course I would like, if he was still here, he and Renata both, because they were quite different in their contribution to the University. But the development of clinical and genetic counselling owes a lot to both Joan Burns and Renata and of course she brought the clinical expertise which was needed in such a programme. At least the paper qualifications which are necessary for this.

PSH. One thing that I have been asking everybody that I have seen, has been, who has been the person that's most influenced them in their career and life in genetics and is there anyone that especially stands out in terms of you and your formative years?

JC. In my own case, at the University of Texas, although my major professor was Patterson, the person who most influenced me was Stone, Wilson Stone. This was because he and I shared an interest in evolution and in Sewall Wright's work. He was not at all mathematical and I think as often is the case, he thought mathematics was more useful than it really is. He knew I liked math. So he encouraged me to take courses in that and to read Wright's papers. So in graduate school he was the major influence. Both Patterson and Painter were there and of course I saw both of them.

PSH. The other thing I've been asking everybody, again, it's not a fair question but I've been asking people can they identify one piece or area of work which they feel most identified with. If you had to have one piece of what you have done remembered, what would it be?

JC. Well among the things I have done, the cutest, it's by far from the most profound, or it isn't profound at all, but this was the idea of using surnames as a way of studying population structure. I think I enjoyed doing that as much as anything I have ever done. The paper with Morton and Muller was a big influence in my life and I enjoyed doing that. The history of that paper is interesting. I had served on the BEAR [Biological Effects of Atomic Radiation] Committee. I already knew Muller from Amherst days and I knew Wright. This committee reached an impasse between Wright and Muller. It had nothing to do with the eventual conclusions of the committee, but it had a lot to do with how we would explain them. Muller wanted to use his genetic load concept and Wright thought that was too simplified and wanted to bring in all the complications, including random effects, and Muller was having none of it. The Chairman of this committee, wisely chosen, was Warren Weaver of the Rockefeller Foundation, a mathematician actually. I played a significant role with him because I understood both Muller and Wright's work and perhaps I was the only person on the committee for which this was true. So I sort of tutored Warren Weaver. Meanwhile he was a Talleyrand. He managed to get these two warring spirits to agree to a report and it was quite a piece of work. But it was in the course of these meetings that it started out with Newton

Morton sending back some data from Japan on consanguineous marriages. Then I worked out a little theory how to relate that to the mutation rate, and Morton played a role in this too and collectively we did this. And then I talked to Muller about it and he had almost the same thought, so he said let's write a paper together, at one of these BEAR meetings I think it was, some meeting where we were together. I didn't want to have a paper written by Muller because he sometimes exaggerated, and it wasn't the style in which I wanted to participate so I went home and immediately wrote the paper and sent it to Muller. Newton was here too, so I showed it to both of them. Muller had several revisions to suggest, but they didn't change the nature of the paper, so the paper is almost entirely my writing and I enjoyed this because two or three people, L C Dunn for one, congratulated me on writing a paper with Muller that was intelligible to everybody. Some such words as that. So I did get pleasure out of this and it was Muller's suggestion that we make Morton first author. In the first place he was as much responsible for it as any of us, but also he had a budding career and Muller thought that would help him.

PSH. Are there any other special things Jim, because we must break and perhaps go for a bite of lunch. Coming from outside I am very conscious that there are big areas I'm just not familiar with which somebody working around here, it would be second nature to. Thinking in terms of how human and medical genetics developed here are there any big areas or major players I haven't asked about at all?

JC. I don't believe so. I think we've covered most of them. There is of course far more genetics now in the clinical departments than there used to be and the genetics programme itself has grown, and there are some medical aspects within the department but largely this is the basic department and the way genetics has permeated the medical schools has been through the clinical departments or sometimes basic science departments.

PSH. Well thank you again. It has been fascinating.

End of recording.