Trefor Jenkins

Personal Details

Name	Trefor Jenkins
Dates	Born 1932
Place of Birth	Wales (Merthyr Vale)
Main work places	Johannesburg
Principal field of work	Anthropological genetics, medical genetics
Short biography	See below

Interview

Recorded interview made	Yes
Interviewer	Peter Harper
Date of Interview	10/10/2007
Edited transcript available	See below

Personal Scientific Records

Significant Record set exists	
Records catalogued	
Permanent place of archive	
Summary of archive	

Biography

Trefor Jenkins was born in Merthyr Vale, South Wales on 24 July, 1932 and qualified MB, at London University (King's College, and Westminster Hospital) 1956. After military service ('57-'59), he obtained the D Obst (RCOG), and became an MO at Wankie Colliery Hospital, Zimbabwe, 1961-62, before moving to the University of the Witwatersrand, (Wits), Johannesburg, 1963-1998 - first in the Anatomy Department and then in Haematology, School of Pathology (Wits and the South African Institute for Medical Research (SAIMR)). An MD(Lond) thesis on "Genetic polymorphisms of Man in Southern Africa" (1972) and a US Public Health Service International Post-Doctoral Research Fellowship at Case Western Reserve University (1968) proceeded his appointment in 1975 as the first professor of human genetics at Wits University – from which he retired in 1998. He was honorary lecturer in medical ethics from 1980-2001 and Interim Director of the newly established Institute for Human Evolution at Wits (2004-2009). He is the co-author of three books: "Health and the Hunter-Gatherer", OUP (with GT Nurse), 1977; "The peoples of Southern Africa and their Affinities", Clarendon Press, Oxford (with JS Weiner and GT Nurse) 1985; and "A Search for origins", Wits University Press (with P Bonner and A Esterhuizen), 2007.

INTERVIEW WITH PROFESSOR TREFOR JENKINS, 10th OCTOBER, 2007 (PART ONE)

PSH. I am talking with Professor Trefor Jenkins in Cardiff and it's 10 October 2007. Trefor, might I start and ask when were you born and where.

TJ. I was born in 1932, in Merthyr Vale in South Wales.

PSH. For people who don't know South Wales, can you say a little bit more about where Merthyr Vale is.

TJ. Merthyr Vale is in one of the valleys, namely the Taff valley, which runs from the Brecon Beacons to Cardiff, about I guess 30 miles long, and the valley itself is a narrow one in the lower part in which I was born, but it's better known perhaps because the borough in which Merthyr Vale is situated is called Merthyr Tydfil and I guess 30 miles from Cardiff would be a reasonable point on a map.

PSH. Tell me a little bit about your early years in Merthyr Vale.

TJ. Yes, I was born into a working-class family, coal mining antecedents and went to the grammar school for the district. It was then called secondary school. The full name was Quaker's Yard Secondary School, in Quaker's Yard Grammar School, which was 3 or 4 miles down the valley from the village in which I was born, and I started there in 1943, I guess it might have been 1944 and had my secondary school education, culminating in a higher school certificate accomplishment which enabled me to go to university in London.

PSH. What was the educational system like in and around Merthyr in those days.

TJ. They were village schools, so the numbers were relatively small. They were, I think, of a very high quality with good teachers, committed teachers, and I don't think in retrospect we were deprived in any way, although the area was a deprived area. Coal mining was the industry and it was before the National Coal Board of course, and so conditions for the mining of coal were not optimum, I imagine and there were traditional strikes, and also accidents, but it was a warm Welsh village, I think pretty typical for the time. A large proportion, yes I think it's fair to say that a large proportion of the children did go on to secondary school education and produced a lot of people who became university students and educated. It was, and I believe it still is, an exporter of educated young people, because having gone to university very few returned to those areas to work, but amazingly it still went on producing good quality students, with the Welsh as the Scottish, known for a definite emphasis on good education and a lot of the budget, I recall, of the council, the county council, it was a borough county council. A large proportion of the budget went into education.

PSH. Were your family immediately involved in mining?

TJ. Oh yes yes.

PSH. And which was the mine that was mainly

TJ. Merthyr Vale which is situated between the village of Merthyr Vale on the east side of the valley and Aberfan on the west side. But technically the mine was in Merthyr Vale even though both villages converged on the same mine for working.

PSH. Had any of your family been to university before you?

TJ. No, I think that only one first cousin of mine had been to a teachers' training college, Carmarthen, completing his teachers' qualification during the war and then being conscripted into the Royal Air Force and became trained navigator and it was his return to the valleys in 1945, when he became a school teacher, that was the stimulus I think to my realising my potential as it were, because I don't think without him my family would have been fully aware of the opportunities that were open to working class children at the time. So I think that he was the only person who had education after school and after high school, grammar school. So I think I was the first in my family. I would think in the village there were very few who went to university. But I have to say that there were some and I can rattle off the names, they are not relevant really, of people who became Head of Botany in University of Sussex and then . . .

PSH. Who was that?

TJ. Bernard John, whose name is well known. He was a very fine geneticist and went to the Australian National University as Professor of Population Genetics. And Sir Ronald Mason went to the same school, a year or two ahead of me, and he became Professor of Chemistry in Sheffield and Scientific Adviser to the Prime Minister. And there were others. It's invidious to name just two; there were others and subsequent to my time there were many more who were successful after university education.

PSH. What do you think it was that made you go in the direction of medicine and science rather than say the arts or humanities side for university?

TJ. That's interesting and it may just be imagination in some ways, but I do remember being very impressed by the local general practitioner, a Dr Ferguson, a Scotsman, who visited me at home when I was sick, and I wasn't a sickly child, when I had tonsillitis or something. And I remember him very clearly and the prescriptions he wrote. I was interested in the process of a doctor's visit, a prescription and my mother taking it to the pharmacy and so on. I can even remember some of the items on the prescription but I won't bore you with that. But I think that must have stimulated my interest, but since I am in Cardiff, the other incident I can remember is being taken to a paediatrician in Merthyr hospital who was the Professor of Paediatrics in Cardiff and I think his name was Strachan. And my mother took me to see him because I was having pains in my legs and joints. We called them growing pains and I remember, and I must have been well under 10, the paediatrician examining me and saying "Yes well probably all you need to do is give him aspirin" and I remember being rather affronted by that and saying

"Only aspirin? Well anybody can prescribe aspirin!" Anyhow, it showed it wasn't anything very serious, but even then I might have had a bit of an enquiring mind and questioned the doctor's prescription.

PSH. Were there any particular teachers who really fired you up.

TJ. At school, oh yes, there were undoubtedly a very inspiring headmaster and a number of teachers who were inspiring, including the very young teacher of zoology, a biologist who came, who was appointed, I remember it must have been in the middle years of the war. And she was an inspiration, still alive and still an inspiration at age 86.

PSH. So when you left school, was there any particular reason why you went to London rather than anywhere else for university?

TJ. Well that's an interesting story in itself. I failed to get a place in Cardiff, and I was too late to apply to any other medical school. The following year I did another subject or two in Higher School Certificate and the following year I applied in good time to many medical schools and the first one that interviewed me was King's College Hospital London, and they offered me a place. So when subsequently someone said 'but why aren't you going to Cardiff?' I said well King's College, whatever that is, I didn't know what it was, but I had an interview, I had an interview in London and was accepted so that was the first to accept me and so that's where I went to from school, but because of that extra year when I took botany and zoology separately, instead of biology, and then I think added physics to it in someway, when I got to King's College in the Strand to do the pre clinical course, I was over-qualified and they wouldn't take me into the first year, and they tried to get King's College to take me a year earlier than they had accepted me for. They couldn't do that so, since King's College in the Strand served King's College Hospital, Denmark Hill, the Westminster Hospital and George's, the secretary phoned Westminster and they accepted me. So I did my pre-clinical training at King's College, London and my clinical training at the Westminster Hospital.

PSH. In terms of your medical school training, was there, particularly in the pre-clinical side, was there much of great interest in the science side there, or was it all very much geared to clinical work from the beginning?

TJ. Yes it was geared to getting it out of the way to go on to do the clinical work, I would have to say. A minority of students went on to do BSc's and so on but I wasn't in that category at all. But my goal was the five terms that we did and then on to the clinical. But it was a fine education at King's College and culturally very enriching, with a good choir that I joined and there were debating societies and so on, so it was a fine education. And then Westminster was a delightful small medical school, where we were a class of about 24, so one knew all the teachers. There was a common refectory for staff and students, so I was fortunate I think to go to Westminster.

PSH. What years was it that you were at the Westminster?

TJ. '53 to '56.

PSH. I can't but help thinking that at King's College, at that time, there were all the developments in molecular biology with Maurice Wilkins and Rosalind Franklin.

TJ. Indeed, yes, in the basement

PSH. Did that impinge on you at all?

TJ. No it didn't. We went into the building, Somerset House, from the embankment, having got off the tube and we entered through the basement and I knew we were going through the physics department, but I didn't know that Randall was working there and Wilkins and Rosalind Franklin. No, I didn't know they were there at the time, but I read with interest subsequently, of course, what was going on in the same building.

PSH. Did genetics figure at all in the undergraduate teaching.

TJ. No, I can honestly say, there was no stimulus, either at Kings or at the Westminster. I don't think the Westminster was a strong scientific institution then. There were a number of part-time consultants. I believe that a few years later, I heard from a friend, that the University had threatened to close Westminster Hospital because they didn't have enough academic posts and had gone along the private practice path too far. But it was a very collegial place to be and a good choir to sing in and again cultural societies, famous speakers came, as they did in fact at Kings. We would have Alexander Fleming and John Cockcroft, and all these famous people came to give a lecture and so I suppose all of that was enriching.

PSH. When you qualified, what happened next?

TJ. Well, I did my house jobs at St Stephen's Hospital in Fulham, Fulham Road I think it was, which was an old hospital, with many of the consultants from the Westminster also on the staff.. So I went and I did my first job for Dudley Hart, who was a well-known physician. I didn't get a job in the Westminster, but this was like the second best and that was an interesting experience. Many stories about that I could relay. And I did a surgery job of a man called Daryl Waters who was not on any teaching hospital staff, but it was interesting, partly because he was Enid Blyton's husband and therefore we could call him Noddy behind his back and see many of the characteristics of Noddy coming out in Mr Daryl Waters. So there was plenty of fun and, in fact, he invited his Registrar and me to his house for dinner. So I had the pleasure of meeting Enid Blyton, his wife! He was not a very impressive character, partly because he was stone deaf and didn't have his hearing aid on when he operated, or the battery failed when he was operating and somebody had to go along to the pharmacy to get a national health service battery for his hearing [aid]. Anyhow they were very good registrars from whom one learned a great deal. After the two house jobs at St Stevens, I did my National Service, two years, in fact..,I got married in between the first job and the second job of the housemanship.

PSH. And am I right that you have known Ada since schooldays?

TJ. Yes, from sixth form days at school. And as I say we had a long courtship and married in '56.

PSH. So with National Service did you go into the medical corps?

TJ. Yes, oh yes.

PSH. Was it Army or . . . ?

TJ. Army, and they were short of doctors, so we had a very abbreviated basic training and I didn't distinguish myself too well on drills and parade. I was excused the passing out parade because I was such a bad marcher and the corporal training us said, after he gave us a break, oh he was very obnoxious on the parade ground and made insulting comments, but when we went for a smoke break, he said, and he called us 'sir'. "Sir I have only encountered one worse marcher than you". And I said who was that? He said "That was Roger Bannister", who was in the previous year's intake, apparently. I said why should Roger Bannister be a worse marcher than I am? He said "Well he had a good excuse. His chest was so big he couldn't swing his arms properly." But I had no excuse except I didn't concentrate, when he'd say right turn I would turn left and that sort of thing. And when we went on shooting practice with our pistols, the man training us was shouting at me saying "aim higher sir. Aim higher". I had to turn to him and say "Look, I'm really a pacifist and I'm trying to hit the target in the legs, not in the heart". He thought I was mad of course. But it was true. I did try to deliberately hit the "man" in the legs, hit the target in the legs. So it was quite an interesting experience. Training was cut short and we were sent to hospitals, and I was stationed in Canterbury with the Buffs regiment and that was the only active service I saw, which enabled me to learn to drive on an Army vehicle and do locums for the local GP in a little village called Sturry. So I had a taste of general practice. After my Army sick parade, I would go home change out of my uniform into a suit and do a clinic for Dr Mackintosh But it was a very good year, newly married, we had a baby and then, at the end of military service, we moved to Wales, for the only time I ever worked in Wales, and I went to the obstetrics hospital in Swansea, which was called Mount Pleasant, and did six months there and got a lot of experience and then I did, not quite six months, but almost, in Morriston Hospital doing anaesthetics, because by now I was convinced I would want to go to Africa to work and I felt I needed to know how to deliver babies and how to give an anaesthetic. And so that was preparatory to me getting an interview with Anglo American, a big mining company, where my wife had a cousin, a first cousin who was a doctor working in Rhodesia as it was, Southern Rhodesia, in the Central African Federation, and he was the Chief Medical Officer there, so it was fairly automatic to go to a job without really applying. And that's how I landed up in Africa.

PSH. If I can go back a tiny bit and ask, what was it that made you want to go to Africa in the first place?

TJ. Well I have to confess that I had some idea of becoming a missionary doctor and I wrote to the Baptist Missionary Society for details and I explained that I would like to go for two or three years to see if I liked it. And they wrote back and said ‘No we can’t consider you under those conditions. You have to make a commitment for life’. It really was very stringent, ‘because we will be sending you to Belgium for a year to learn French, because we need people in the Belgian Congo’. And I must have indicated that I wanted to go to Africa I suppose, and so I said well I wasn’t prepared to commit myself to a lifetime of missionary work and so to go to a mining hospital, a mines hospital in Southern Rhodesia, was an alternative to that. This was 1960, before independence in the Belgian Congo, as it was, and I was even liaising with a friend of mine from schooldays who was teaching in Nigeria, that we would both travel, with our families, we were both married, to the Belgian Congo and meet up half way. After a few months, Belgium very suddenly gave independence to the Belgian Congo and, of course, there was chaos there. Hamarskjold died in an aeroplane crash and nuns became refugees and there was mayhem, which we all know about. So that the meeting in the Congo never materialised and I did my eighteen months stint in Wankie Colliery Hospital. There was no shortage of doctors in Wankie and since I was the newest, the youngest and the smallest of the doctors, I was given the paediatrics ward to look after! But they were very considerate about this. They arranged for me, during the first week that I was there, to go to Bulawayo to do ward rounds with a couple of fine paediatricians (they went on to become very famous doctors) and they taught me how to do scalp vein infusions and some other procedures, and so I became a paediatrician in a week and went back to Wankie, where I had the paediatrics ward to look after. That was the changing experience, my life changing experience, because there I saw sickle cell anaemia! I had never seen, or even heard of it before, and I became fascinated by it and discovered that I enjoyed research, which had never been stimulated in me at the Westminster or Kings, and I realised that I wanted to do research. I gave expression to that by contacting the director of the Museum in Livingstone just across the Zambesi River, at the Victoria Falls, to ask him a few questions, like why are these people in Wankie called Bantu? What does that mean? I had no idea and told him I was interested in the people and I had asked all the local people and nobody seemed to know why they were called Bantu. So Desmond Clark who became the world famous Africanist wrote a very courteous letter back to me explaining the term and what it meant and so on, and also encouraging me to carry on my research. Sickle cell anaemia is a very important disease with anthropological significance, he told me. And encouraged me to carry on with it.

PSH. That was quite far-sighted at the time.

TJ. It was ’61. He was just about to leave Livingstone to take a chair at Berkeley, where he remained as a distinguished Professor for the next forty years. He died a few years ago after retirement, but he carried on his research in Africa and trained a whole generation of Africanist anthropologists and I met him subsequently. I met him soon after that actually, when I went to Zambia to do research work. But he came back for many years working in the Zambezi valley and in the northern part of the country. So that was also a fortuitous and fortunate experience..

PSH. Did you have any facilities, in terms of a lab where you could do basic, simple things?

TJ. There was a technologist there from South Africa, very good male technologist who taught me how to do the sickle cell screening test on a slide. But I soon discovered, reading the journals, believe it or not, the journals that I read were sent to me by train from Johannesburg. There was a colleague at the hospital who was a Wits university graduate and he told me about this university, that I had never heard of, in Johannesburg, the University of the Witwatersrand, and he said if you need a journal, write to the librarian there at the medical school and she will help you. I did that and they would send up annual bound copies of the BMJ for example, wrapped in brown paper, on the train and they never charged for postage. I guess I had to pay for it to go back! I was exposed to recent literature in that way, and the older literature as well, because I knew nothing about sickle cell anaemia. But I read around the subject and did field surveys in the Zambezi valley. I would exchange our family car for the Methodist missionary's Land Rover and drive down to the valley at weekends, holding clinics and collecting samples of blood and urine, because I also did a Bilharzia surveys, as I recall. But it was really sickle cell anaemia and blood grouping which, back in the lab, this technologist taught me to do. Then it became apparent from the literature that I was way behind and needed to do electrophoresis, and there wasn't any apparatus for doing that. So I requested the apparatus and the Chief Medical Officer, who was my cousin by marriage, approved it but the manager of the mine, a very fine man, called me in and said "You have come here to do medicine, not research. We can't approve the £30 or whatever it was for the electrophoresis apparatus" I think, I had already thought of leaving, but that convinced me that I needed to move on. Because I had served only eighteen months of the three year contract, I was liable for our fares back to the UK. So that helped us decide that we would go to South Africa on the way back, as it were. I was obviously politically aware about what was going on in South Africa in '61, the end of '61. I got a job in Durban, in surgery, because I felt if I was going to continue in this sort of work I needed to improve my surgical skills, and went to Durban to work for a friend of a friend, who was professor of surgery there. But I was unable to get free from research! The week before I was due to leave Wankie, I met at the hospital, Denis Burkitt on his famous tumour safari; he showed us photographs of children with this new tumour/lymphoma that he had described. When he heard I was going to Durban, he said something like, "Well, if this tumour occurs anywhere in South Africa it will occur in Durban", because his hypothesis was that it occurred in the tropical area: and he tied it in some way with malaria but he didn't know how, and Durban would represent the coastal tail of the southern distribution of the mosquito and all the year round malaria. And so when I went to Durban, I was presented, on my first ward round there with a case of Burkitt lymphoma, which I recognised from Burkitt's photographs which he had shown me in Wankie. That project kept me busy for the year, finding and following up on Burkitt lymphoma patients, leading to a paper and again the research pull was there. And because in Durban I met a fellow houseman, called Errol Friedberg who had been a Wits student and had done a BSc at Wits whilst doing his medical training under Philip Tobias, when he heard my account of my research efforts in the Zambezi valley, from Wankie, he recalled that Philip Tobias had done a lot of anthropological research in the Zambezi valley and so when he was planning to go to Johannesburg to interview for a pathology registrar's post, he took me along with him. I contacted Tobias, met him on that weekend visit and Tobias said yes he could give me a little job and it was a sort of slave labour job. A table doctor.

PSH. In the anatomy department?

TJ. In the anatomy department, and the freedom to continue my research. So it was an amazing lot of coincidences that led me to Wits and I worked in that department for two and half years.

PSH. Am I right Trefor that you had already published the child with sickle cell anaemia before Burkitt's paper?

TJ. Yes. From Wankie, with the encouragement of Michael Gelfand, who was a great physician in Salisbury as it was then, Harare now. His classic book, "The Sick African" was available at Wankie of course and he edited the Central African Medical Journal, which he had founded , I think, about eight or nine years before. So he was very supportive of my first manuscript. He knocked it into shape and continued to be supportive, I published, I think, two or three papers in the Central African Medical Journal, and then the Burkitt lymphoma study we published in a journal, now defunct, called Medical Proceedings, because the head of surgery that I was working under was a friend of the editor of that journal. It was interesting because it did define the southern limit of the distribution of the Burkitt lymphoma.

PSH. When you went to work in anatomy, did you still keep clinical work going at the same time or were you then a sort of full-time anatomist?

TJ. Yes I was a full time anatomist, but soon discovered that next door to the medical school was the so-called Non-European Hospital, which was part of the Wits teaching hospital and gave Wits the opportunity of training black doctors who were not allowed into the white hospital at that time, and that persisted for many years. So the Non European Hospital as it was called, which was right next door to the medical school, which itself was right next door to the Institute for Medical Research where I had contact as well. . . What was the line of what I was trying to develop then?

PSH. Well I was wondering if you were able to combine clinical with the anatomy?

TJ. Oh yes. So because of that hospital's proximity, I would attend ward rounds there every Wednesday morning with an outstanding physician called Harry Seftel, who had specialised in the diseases of African people, having worked in Baragwanath for some years before he moved to the Non-European Hospital. And those ward rounds were incredibly interesting, because I saw a spectrum of medical diseases which I had never heard of, and he was an entertaining and erudite clinician. So I learnt a great deal from him Then, in addition to that, in order to supplement my meagre income from the demonstratorship in Anatomy, I would work sessions at the Non-European Hospital at weekends, a night shift or two, and that enabled us to survive financially, my wife having to go out to teach, and that's how we survived. And then Tobias, in fairness to him, did realise through the year or so that I might be worth keeping, not just for one year, that I wasn't really committed to surgery and had already started doing field work from there. He gave me a lectureship, so that gave me more pay. And I continued, incredibly fortunate because Tobias had given up doing fieldwork himself and he knew the importance of the new anthropology, which was genetics, even though he hadn't himself got involved. He used to collect samples and send them to London to Nigel Barnicott at University College, and to a local blood transfusion service for red cell antigen typing but I set up lab facilities in his department, with his support of course, but the samples that I had collected in 1963 from the Kalahari Bushmen, and from the Zambezi valley Tonga, Bantu-speakers, were sent to Nigel Barnicott in University College London. I visited Nigel in the December of 1963. We took our first home leave, as it were, at Christmas 1963, and I went to University College to his lab and I watched him doing starch gel electrophoresis, which I had never seen before. When I saw him in his suit and waistcoat and a curly pipe puffing away (and the dribble dropping onto his waistcoat), as he sliced the gels. This is a very appropriate recollection because Oliver Smithies was awarded the Nobel prize yesterday or the day before, and he had devised starch gel electrophoresis, which was a tremendous advance and wasn't, as far as I am aware, referred to in the newspapers this week.

PSH. True.

TJ. I saw Barnicott doing it and I said "Well, I'll be blowed, if an anthropologist in his suit and smoking a pipe can do it, I'm sure I can do it!" I asked him whether he thought it was feasible for me to do it and he said "Of course. Would you like to do it?" and I said "Yes", and he said "Well, when you go back start it up and if you are in any difficulty, give me a ring". And I did that. I phoned him once or twice from Johannesburg at the anatomy department; I had forgotten how he had got the top layer off and so on, in spite of watching him do this for a few days. He didn't give me a chance to do it because they were precious specimens. But that was also inspiring, to go to him and take it back and start the technique in the anatomy department, which I did.

PSH. It sounds almost to me Trefor as if you had developed a kind of anthropological interest by the time you had arrived in the anatomy department, or separate from your job in the anatomy department.

TJ. Oh yes . Well from my work in Wankie there were the surveys. They happened to be on the distribution of bilharzia and sickle cell trait. But you are right, I was interested in the populations and I enjoyed driving down to the Zambezi valley and interacting with the Tonga people, they were called, joining in the dancing at night and so on. I bored friends for many years with my photographs! I was interested in the people. So you are right, I was yearning for, I think, how can I put it, to gain an understanding of different populations.

PSH. When you got your lab with Phillip Tobias, what were the first projects that you actually used it for?

TJ. Well sickle cell was of interest of course, G6PD deficiency using the screening technique described by Motulsky and Campbell-Kraut. And with the encouragement of biochemists at the South African Institute for Medical Research, I was able to access those techniques and it was the start of the polymorphism explosion of the time and so the red cell enzymes from the Galton laboratory were being described and the techniques and starch gels were needed for that, so it was exciting technologies that were available at the time, coupled with the excitement of expeditions, I suppose. I guess I might have been an explorer if I had been born a hundred and fifty years earlier! So it was all of that excitement, I think, and the influence of my mentor, Arthur Steinberg, whom I met when he visited South Africa. He came to study diabetes in the Natal Indian or Asiatic population and his collaborators in Durban were physicians who, he thought, were completely undisciplined and didn't take the project seriously. He phoned me, one day, from Durban and said "Can you come down here? These two colleagues are driving me mad!". I had never done anything like that. I said how do I get down there? He said "Get a flight. I'll pay". And so I got on a plane and went to Durban just to metaphorically hold his hand, working amongst two clinicians, who had no idea of field work, if you like, Arthur had done a lot of his Hutterite work by then. He used a lot of polymorphisms, not only his imunoglobulin (Gm and Inv) systems. So I went to rescue him and that led to a friendship and an NIH international post doctoral fellowship, which took me to the States for the calendar year of 1968. But by then I had moved from the anatomy department, I should make this clear, after two and half years when I realised that my natural home was in a biochemistry laboratory, rather than in an old fashioned, anatomy department, and so I moved across into haematology. Believe it or not, there was no advert. I went to see the Director of the SAIMR, who was interested in African peoples, and he thought it would be fun to have someone working on the genetics of African peoples, created a job for me in the blood transfusion service of the Institute, which then enabled me to have access to thousands of samples from the gold miners who came from all of sub Saharan Africa, really, to work in the gold mines, and those who were blood donors would give, or perhaps I should say, we would take (no consent was specifically given in those days!); we would take an extra 2 or 3 mls of blood at the end of collecting a pint for transfusion purposes and that gave me access to thousands of samples from dozens of tribal groups. My early work was carrying out sickle cell and protein and red cell enzyme polymorphisms on those samples, interspersed with field work in the Kalahari amongst the San (Bushmen) people.

PSH. When did you first start getting particularly interested in what one used to call the Bushmen but now San.

TJ. Well, my first trip was in that very first year in Tobias's department, 1963. He had some money left over from a grant in about September or October and he sent my colleague Stan Blecher, who was a year or two senior to me in the anatomy department, and me on a Kalahari trip. It was absolute luxury because Tobias was able to afford a chartered plane to fly us to Maun on the Okavango delta, where we engaged some white hunters, because it was the off season, who took us in their very efficient vehicles, with ice making machines and so on, to both east and west sides of the Okavango delta to San people, and so our first field work was ten days . . . Oh not only for the studies I've already mentioned, but Tobias also wanted to do chromosome studies on one or two San people. Because, believe it or not, in 1963, which is the year we are talking about, there were still murmurs and rumblings in the German literature that the San people were a different species from *H. sapiens*. So Tobias wanted to confirm that they had 46 chromosomes! At the end of our Kalahari trip we collected blood from a few San people on a runway on the western side of the Okavango delta, waiting for the plane to land to fly us back to Johannesburg, within two hours, or whatever it was, because we thought you needed very fresh samples to do chromosome analysis. And so we did it. I don't think they ever published the results of the chromosomes of the San at that stage. It would have been a bit insulting in a way. But Tobias, remember, in 1956 I think it was, when the International Human Genetics meeting took place in Rome or Copenhagen.

PSH. Or Copenhagen?

TJ. No. Was '56 Copenhagen?

PSH. I think '56 was Copenhagen and '61 was Rome.

TJ. Well, it was at the Rome meeting in '61 that Tobias read a paper on Bush-Caucasoid, or Bush-Caucasoid hybrids, and there is a picture in that article, I think, of a man whose name was Jimmy Morris who was married to a San woman and they had four or five children and the picture is showing that the German anthropologists, mainly working in South West Africa, as it was, were wrong in claiming that the San were a different species. Tobias was now keen to have chromosome proof as well; I mean it's a bit of overkill, really, wasn't it? Nevertheless that was probably why he funded the trip! Stan Blecher was doing cytogenetics and he got very nice cultures on this very fresh blood carried from the Kalahari! And that was an important year, 1963. At the place where the airstrip was situated, was called Nokaneng, on our way into the Kalahari, we met Richard Lee. He was the leader of an expedition from Harvard starting a year or so later or so, but he was there doing a reconnoitre and we met him and he directed us to the Kung San who live at Dobe on the west side of the delta. And that was my first exposure to the San. Later in the year, in December, on our way back to the UK for our holiday, I carried out some more fieldwork in Zambia. and collected blood again for the Tobias type of work which was to be sent to Barnicott. I didn't do detailed genetic studies on those samples, but I did sickle cell and G6PD screening tests in the field, yes in a hotel bedroom!

PSH. Can I ask, at that time when you were going back to see Barnicott at University College, did you have contact with the Galton people in a wider sense?

TJ. No. he talked about the Galton and Penrose was still head, because I think Harry Harris took over '65 or thereabouts. And so I knew about the Galton and they were very close geographically, but I didn't actually go there at that time.

PSH. And the other person I was thinking, had you had any contact in terms of your sickle cell studies with Allison?

TJ. Yes. I visited Tony Allison at Mill Hill in 1963, because he had done the famous work in East Africa, but also because he was a Wits graduate. He had done a masters at Wits under Dart. He had been born in Kenya, of British parents, and he came to Onderstepoort, the veterinary school in Pretoria but the veterinary people found him so bright that they 'phoned up Raymond Dart at Wits (I think this is authentic), and said to Dart, "We have a chap here who is far too bright for us vets. Would you be willing to take him to do science?" And Dart took him and he did his Masters at Wits on the brain of elephantulus (elephant-shrew) or some animal.   He was extremely bright. Philip Tobias remembered him from those days.   And from there he went to Oxford and he started his medical course there, clinical work I presume, and from there went to Kenya visiting his parents and doing his famous field studies at the same time.   So I went to Mill Hill, met him, talked about Wits and so on, and he told me what he was doing and how his research was going and so on.   He was very welcoming. He came back after that to Johannesburg on a couple of visits and gave us some lectures. I don't know whether he is still alive. I know that he went to the States.

PSH. I think he is still alive, because I met his son, who actually is a doctor in North Wales.

TJ. Oh!

PSH.   And that was two or three years ago when we were talking, but I haven't heard since then that he's died.   And indeed he wrote a sort of retrospective review of his research which I have, which wasn't very long ago at all, which you can look up.

TJ.   For you he wrote it?

PSH. No not for me. He published it and I think that was only a short while ago. Trefor, remind me of the year you went to America again.

TJ. '68, the calendar year '68.

PSH.   So you were there for one year?

TJ. Yes.

PSH.   And where were you based?

TJ.   Case Western Reserve University in Cleveland, Ohio.

PSH. With Arthur Steinberg?

TJ. Yes.

PSH. And did you go there with a specific project in mind, or more to learn the techniques?

TJ.   Well I took samples for immunoglobulin typing, so I learned to do that there. He had samples from us earlier on and I joined in the lab work. I can't say it was a specific project, other than analysing the computer programmes, and we had to punch in cards in those days. I attended his statistics lectures and some of his introductory biology classes as well, and we wrote a long paper. So I wasn't, I suppose, a typical post doc, but I did learn a lot from him and the other people in the lab. They were very good people. So it was consolidating and educational, I suppose, which I had missed out on because I had never done any formal courses in genetics. He was a bit disparaging about medical geneticists in those days and made no secret of it. He thought that we were, as doctors, not achieving very much. He did some genetic counselling himself, by the way. He ran a clinic at the Rainbow hospital I think it was called, the teaching hospital of Case Western Reserve, and there is, in fact, in Kevles' book, an interview with him in which he relates, how frustrating it was to do genetic counselling. Now he wasn't a physician but he did counselling and expressed it this way in Kevles' book. He said "When I went home in the evening after doing my counselling clinic my wife, Edith, would say "You been doing counselling again?", because I was so depressed at being able to give them, only, a 1 in 4 or 1 in 2 risk". So as a scientist he felt very frustrated in doing that. He couldn't see the importance of counselling. I refer to it, I think in his obituary, or I might have deleted it.

PSH. I have got the obituary of Steinberg upstairs.

TJ. It's in Kevles anyhow. I returned to Wits in January '69, advised by Steinberg not to "waste my time" on genetic counselling, so I didn't do anything about genetic counselling. I wasn't doing it before going and I didn't come back to it. But in fairness to Arthur, and I must check the dates, within a year, I think, he wrote to say, 'Get back into clinical work. We can now, do so much practical good to help patients with genetic diseases. So I've changed my mind. Get back there: you are very lucky to be a medical graduate.' - that sort of thing.

PSH. It interests me that his original attitude to compare it say with, Sheldon Reed, who can really, I suppose, started off what was the centre of very, very early genetic counselling in America and I have been re-reading his book and some things he wrote, and I guess it must be a very different personality, but he said he found it intensely rewarding even though he couldn't actually do anything and I think probably he was a very sort of empathic . . .

TJ. Person

PSH. Person who enjoyed the communication process.

TJ. Was he an MD?

PSH. No he wasn't. He started off in Drosophila research, but my gut feeling would be it would depend very much on your personality and on what you felt was most important. Because most . . .

TJ. Arthur did change his mind.

PSH. Fair enough, but only when there were practical things that could be done, whereas I think a lot of us involved in genetic counselling in the early years, when there wasn't much you could do, certainly I always felt that families were very grateful just for being listened to, even though you hadn't done very much, it was still rewarding despite that.

TJ. That's what I found as well.

PSH. When did you actually then start anything in the way of genetic counselling.

TJ. Well it must have been at the end of '69, early '70, although I had done a little bit of counselling I suppose, even earlier than that, with respect to some chromosome disorders. I had been exposed to that a little bit. But anyhow, let's say it was '70 or maybe it might be a year later, but what I did was set up, in the Institute where I worked, a weekly clinic and I visited Cedric Carter about that time. When I came back I motivated for a post of social worker to come and work with me, into a research post actually. Jennifer Kromberg. a social worker, came and she did her PhD on the psychosocial aspects of albinism, taking a number of years to do it; and sat in on every counselling case that I did in the Medical Research Institute. But I also did a clinic at the children's hospital, where a psychiatrist colleague sat in with me, and sometimes Jennifer might be there as well. And we always had "autopsies" on the session afterwards and I remember particularly the psychiatrist occasionally saying, "Oooh I wouldn't have pursued that line" or something like that. But she came to appreciate some of my 'foibles' perhaps, and was very encouraging and complimentary about my sensitivities and that sort of thing. Jennifer Kromberg never got that. She didn't like it if the patient cried with me, as it were, over a case. But the psychiatrist, who left and went to Australia, I can't remember her name, was very encouraging, and introduced the "autopsy" after the session, and I guess that was a good way to learn in retrospect.

PSH. I'm sure of it.

TJ. She would be critical but also encouraging.

PSH. Because at that stage, the medical model of genetic counselling was very predominant and I guess that most people in medical genetics hadn't given an awful lot of thought to the more psychological side. But at the same time I think a lot of us did manage to do quite a bit even though we didn't know the name for what we were doing.

TJ. I'm sure that was the case, and I think that some people have a natural empathy which some don't have.

PSH. At that time Trefor, what sort of families would come to your genetic counselling clinic? Were they African families who came or not?

TJ. No. Well we tried. Jennifer Kromberg and I would hold clinics at Baragwanath and at other government hospitals and what they became, is interesting, because we would collaborate with the paediatrician there usually, and the paediatricians, I'm thinking of two hospitals in particular, would think that we were interested in collecting cases, rare cases and so on. So the cases they showed to us weren't altogether appropriate. We would see a very selective group of rare diseases or un-diagnosable conditions and we had to say, look we are not coming for that reason. So it took again a number of years before we got the clinicians attuned to what we were trying to do, and I don't know whether that was your experience. But clinicians are primarily concerned with getting a diagnosis and they thought we would be interested in the same. Now it changed over time of course, and by the end of my tenure, I think, we were better trained. Perhaps we learnt a lot on the job as it were, and we had clinicians coming to work in my department and they brought their own interests and qualities and so on. So it did build up, but we were hampered in our genetic counselling endeavours by the lack of posts, because I was employed by a private organisation, in effect, called the South African Institute for Medical Research, which really provided the laboratory services for the whole or nearly the whole country, and employed and trained pathologists. I specialised in haematology, that was my discipline, didn't know much haematology, except perhaps for red cells a bit. I was comfortable with red cells, looking down the microscope, but not much else. And in those days we were learning on the job, but of course, there were also literature which we had access to, including your book of course, but also I remember buying a Warkany early on and the fascination with congenital malformations was something; and we had a visit from David Smith, but he must have died a long time ago.

PSH. He died quite young.

TJ. Yes and he came and gave the most incredible embryology lectures to the anatomy department, as well as to us geneticists. So I think we were in contact with clinical geneticists, although I would be the first to admit that I wasn't properly trained. But we did have the one advantage, that we actually had to do the investigations in our labs, so we had to set up techniques and so on and we had a biochemist on the staff and he was very good on lysosomal enzymes and we had a large Jewish population in South Africa, so we were early into Tay Sachs disease and pre-natal diagnosis.

PSH. I noticed that you published quite a few things early on, on that.

TJ. Yes on Tay Sachs. We had a visit from Michael Kaback early on and another interesting aspect to our work there was just being in South Africa and we would have race complications, race classification cases and I have a series of about 25 records of patients who came, for what you could broadly call racial classification. I anonymised those files and a sociologist working in London, has incorporated those in her thesis. She has done a thesis on race classification and she found them interesting, because some were legal cases, at least in the sense that the lawyer brought them for classification, because they were being deprived of some privileges because they were classified black and not white. And many were of tremendous emotional interest, and concern, because family disruptions were involved. So I didn't shy away. Some of my friends wouldn't get involved; in fact Steinberg said that he didn't think this was genetics, this was perverted sociology, which of course, it was. But we were in a situation where we could help people negotiate some legal hurdle. Like someone, a young man, whose mother died, brought up in a white area and the house was going to be transferred from her name into his, and he was told by the lawyer, "Sorry but your mother was 'coloured', and therefore she had no legal right to a house in this suburb so you might not be able to inherit the house." Now that case I remember well, testing him and doing the polymorphisms, the range of polymorphisms we had available and, as in all the cases, I would write, "Given the genotypes these data are compatible with Mr so and so being classified as white, in the South African context" and that apparently helped the lawyers, although I must confess that the one advocate who brought most cases told me that the courts would not admit my evidence because they knew race classification was not a scientific thing. But we went through the ritual of doing it in the hope that it might influence the outcomes and avoid tragic cases, because we certainly saw the most terrible cases there. Anyhow that series may be publishable some time, to show what it really was like and how scientists and doctors in particular were caught up in this "perverted sociology", as Steinberg would call it.

PSH. Would it be reasonable to say, Trefor, that that was one of the factors that particularly started focusing you in involvement in the ethics of the system? I thought this was something we should perhaps take up in more detail after the break so to speak, tomorrow.

TJ. Yes ethics yes. Sure.

PSH. It would seem that this was a genetic aspect which will almost lead into these issues.

TJ. Yes, you are right, because I got involved in being invited to talk at ethics symposia through a church connection. There was a theologian in Natal who put on an annual symposium on ethics, and he would ask me to go and talk on ethical implications of mental retardation and things like that. I'm trying to think what they were. Oh, euthanasia, you know the classical ethical problems. But race would feature in many of those, so when the Dean asked me, oh and there were those symposia I went to, and there are volumes, and my papers are published in those. They were very naïve, looking back on them, because I had no training but in the Institute where I worked, there were a group of us, all claiming to be Christians who studied a book, I don't know whether I introduced it or what, by Joseph Fletcher, titled "Situation Ethics". He is now credited in the History of Ethics as being a pioneer in the founding of the modern era of medical ethics. Now his "Situation Ethics" has been frowned on by ethicists as being not a system at all but in fact Helga Couhse, the German-born ethicist working in Melbourne, now retired, in her history credits Fletcher as having been the stimulus to the development of interest in medical ethics. So I did get interested in that I must say and what I did was, when the Dean asked me in 1980, early in 1980, if I would give the statutory lecture required to be given to the final year medical students, and which the Dean always gave. I had obviously expressed an interest, nothing formal in the medical school. So I thought it over and I said yes, I would give that lecture on condition that he give me four additional lecture slots. And he came back and said, "You can have the five!" And so I got five lectures for the medical curriculum and I hope used them to good effect.

PSH. All five on ethics?

TJ. On ethics. I was already giving some lectures on medical ethics. The head of Medicine, Tom Bothwell, very early on, long before I was doing anything very much in ethics, asked me to talk on genetics, medical genetics. Alan Emery visited in about 1973, and I got him to give a lecture to our medical students. So I was interested in ethics and I thought it made a good complement to genetics, because we had very clear-cut ethical issues, abortion and prenatal diagnosis and all the things associated with the discipline. So it fitted very snugly and I'm glad to say that although I applied, tried repeatedly to get a post, it didn't mature until the late eighties, but I've got files of motivations for a post and then we got a very good ethicist, Udo Schuklenk from Melbourne. So that's another of my interests.

PSH. I think we should break soon, Trefor, but just before we break and come back to ethics a bit tomorrow, I'm trying to get a feel as to when your unit in the Institute of Medical Research crystallised as a genetics unit, rather than just being blood groups, anthropology and that side of things.

TJ. Well I was in the blood transfusion before going to the States, that would have been 1965, the last half of the year, ' 65, '66, '67, I worked from the blood transfusion lab, with a lot of Kalahari work. Then I spent the year, '68 with Steinberg. I came back to the SAIMR and was now placed in haematology, where Jack Metz, was head. He wanted to see the Bushmen and I took him to the Kalahari in 1969 and, under the starlight sky, sleeping in the open, we talked; he was now Deputy Director of the SAIMR. He was surprised when I told him that I was going to share the samples of that field trip with colleagues in Cape Town and asked "Why aren't you doing all the testing on them yourself?". I explained that with only one technologist helping me, I had to share the samples. He said 'if you had another technologist helping would you be able to do them?' I said 'yes I think so; it's largely just a matter of hands'. He said 'Well when you get back you will have another technologist.' And he was such a support that with his help, we grew, and there were even more technologists. Some were graduate technologists, some were very good and moved on to other things, but the main change in my career probably was when I had a medical colleague come to work with me, and that was George Nurse. George was a genius, in many respects, never settling down to do anything with a clear focus. He was authoritative in many subjects, including linguistics, music and anthropology. He completed his PhD in anthropology under Tobias soon after joining me. He really a polymath, there's no doubt about that, and he loved field work and possessed great skills. Between us we did an enormous amount of field work in South West Africa (Namibia) as well as Botswana and South Africa. Of course we were still denied access to countries further north because of the apartheid policy in South Africa. I did fieldwork in Zambia up until 1966 and then it got a bit too uncomfortable. I had to say lies, say that I had come from Wales, and then someone would say, have you ever been to South Africa? So I decided I wasn't going to do that any more but and it was very profitable work at that time. George Nurse came and gave a new impetus to the anthropology. He was an MD. Not a good bedside manner. I don't think he was a good counsellor, so he saw very few patients but was good at so many things, including writing and so I would have to say that his stimulus in the window dressing via publications got a tremendous boost from his presence and as you know we wrote a couple of books together.

PSH. Just remind me of the titles of those books.

TJ. The first one was on the the San. It was it called ‘Health and the Hunter Gatherer, published in 1977 by Karger in their series Monographs in Human Genetics’?

PSH. I think it was.

TJ. And then after he’d left, he left after five or six years and went to Papua New Guinea, we (Nurse, Weiner and Jenkins) wrote ‘The Peoples of Southern Africa and their Affinities’ 1985 and so that was a good summary of the work up until his leaving, and it was really a summary of the work preceding the advent of molecular technology that came in during the late ‘70s and George left in ’78. It was the third volume in the Series of Research Monographs in Human Population Biology, following on Bob Kirk’s “Aboriginal Man adapting” and Arthur Mourant and colleagues “The Genetics of the Jews”.

PSH. So when was it that your unit became defined in genetics?

TJ. The Department of Human Genetics at Wits, was established in 1975, the University of the Witwatersrand, largely through the promptings of Tobias, who had been committed for twenty years to getting human genetics established at the university and had not had much success, eventually in ’75, with the financial support of the SAIMR established the University chair within the School of Pathology, and it was advertised and I was appointed. So that was the turning point. We now had a Department of Human Genetics. About 5 years later the MRC established a Research Unit under my directorship. The unit was called the Human Ecogenetics Research Unit. I think George thought up the title. And we got good support from the MRC, which was an interesting paradox because the management posts in the MRC were occupied by “government types”, Afrikaans people, conservative, and yet they supported us, I think quite generously and didn’t seem to object to our joining in discussions on the racial implications of our work. My inaugural lecture was about race and race discrimination. So it is interesting that they were government supporters and yet I suppose they thought, it’s alright, having a tame Jenkins it didn’t matter, but we certainly said what we believed in, and they seemed to tolerate it.

PSH. Can I just ask, when did you get cytogenetics going as part of the

TJ. It was there at the Institute before I got there. There was a woman called Sarah Klempman and another woman called Eugene Wilton was appointed as her assistant. Sarah was a pathologist and Dr Wilton a married doctor who came back to the SAIMR and together they got Cytogenetics going in about 1960.

PSH. Really early.

TJ. It was in 1961 I moved to Durban from Wankie, Southern Rhodesia. I had a Senior House Officer post in surgery for the year 1962 and Errol Friedberg was a Houseman in the same department. And his wife called Sylvia was working in the physiology department of the University of Natal and we became friends, the Friedbergs and the Jenkins's. One of my patients in the surgical ward was a man, query Klinefelter's syndrome, and I found in the literature that you could diagnose that chromosomally, and so I discussed it with Sylvia, because she had done the BSc in Tobias's department and had learned cytogenetics from Dr Klempman and Wilton. Oh yes, this was the point, this man hadn't had chromosome studies, because there was nobody in Natal or Durban doing chromosome studies, so I felt a bit frustrated at not getting a diagnosis and the poor man had, I think had had a testicular biopsy with an inconclusive result or something, but I asked him, phoned him up, could he come in again for us to do chromosome studies, because Sylvia Friedberg had done them as a student at Wits the year before and she said she thought she could do it in the physiology department at Natal. Well he couldn't get permission from his employer to come in again, so I said we'll come and collect the blood from you during your lunch hour. So I drove my car, Sylvia with a bundle of tricks and the man got into the back seat of the car outside the shop where he was working, and I collected blood and Sylvia cultured the chromosomes and confirmed the Klinefelter diagnosis. And I don't know how early that was, compared with what the people at the Institute had done, but it was probably one of the early cases in South Africa of a chromosomal diagnosis. Hymie Gordon I might say, in Cape Town, you remember Hymie?

PSH. I do.

TJ. He was doing genetics then and we collaborated in the latter part of the sixties. He had been with McKusick for a year and another South African, Ingram Anderson, had been with McKusick for a year. You didn't overlap with them?

PSH. I didn't, no.

TJ. So there were two. Hymie came back. Hymie went to McKusick to study cardiomyopathies. I think, and got interested in genetics. Ingram Anderson was interested in genetics and went to McKusick and didn't get turned on sufficiently. He came back and soon went into private specialist practice. But Hymie stayed in Cape Town and worked in medicine establishing a genetics laboratory and I collaborated with him. He would be the person in Cape Town that I would send half the sample to, for him to do some of the enzyme polymorphisms, because he was a frequent visitor to the Galton laboratory and brought techniques from them to Cape Town. But then he left the country in 1969, virtually closed his lab, because Cape Town didn't have anyone to take it over, and it was in '69 when I went down to Hymie to learn the techniques he had learned and I was deploying there, that the Dean at UCT asked to see me and asked if I was interested in applying for the Chair that they were planning to establish. And I went back to the Institute, I spoke to our Director. It was James Murray, Deputy Director who ran the show at that time, and he thought that it wouldn't be long before there would a chair at Wits, because the Institute was now combined with the University and would get the chair established and so I decided not to pursue the Cape Town job. Then some months later, Alan Emery pitched up. I mentioned that earlier. He had been to Cape Town to advise on the Chair. Peter Beighton was already in Johannesburg; I was collaborating with him on the Tristan Da Cunha work and did polymorphisms studies on his cases of arthritis and so on. I didn't apply for the Cape Town chair; he got the chair and I stayed put. I felt a loyalty to the Institute by that stage, and it worked out very well because we soon had two departments then. I didn't have a full department until 1975. I stayed at Wits/SAIMR, and that's how Peter and I headed the two departments for over 20 years. I retired in 1999 and Peter a year or two later.

PSH. Trefor thank you very much, I think you have worked pretty hard this past hour or so.
Let me now turn the machine off and we can resume tomorrow.

INTERVIEW WITH PROFESSOR TREFOR JENKINS, 11th OCTOBER, 2007 (PART TWO)

PSH. It's Thursday 11 October, 2007, and I am continuing the conversation with Professor Trefor Jenkins from Johannesburg. Trefor, yesterday we started to touch on ethical areas, but I said we would leave that for today. Before we come back to them, I would like to just continue a little bit with the scientific side, and we had got to the point where your Institute was established and we'd just begun to talk about how molecular genetics came into it. Can you tell me how that happened?

TJ. Yes. The first I heard of the molecular breakthrough, as it were, was when I visited Stanford University, in 1978 I think it was. I had a friend there, in addition to meeting Cavalli Sforza, I had a friend, a South African, Errol Friedberg was his name, and I visited and stayed in his home, and he drew my attention to a recent paper, by Kan I think it was, who did the sickle cell molecular diagnosis and so on and said, you need to get into this. This is where genetics is going. I remember that very clearly. So when I got back to Johannesburg I tried to stimulate some interest in that, but we didn't have anybody in the department with that sort of background. But later that year or early the following year, we had a visit from Malcolm Ferguson-Smith, who was at Cambridge and he came to us as the von Wielligh lecturer, which you indeed came to deliver many years later, and a fellow guest at their home that evening was Jennifer Thompson, who was a microbiologist-geneticist and was already getting involved in molecular work and it was very interesting to hear them discuss the "molecular revolution". I didn't realise the full significance at the time, but she and Malcolm tended to go off in a huddle to talk. What they were talking about was the great developments in molecular genetics. I remember that as a sort of turning point, and I don't think Malcolm talked much about it in his lectures. If he did, I didn't appreciate it, and off he went. But it wasn't really until 1980, when I went to Boston, really primarily visiting an ethicist, Ruth Pertillo, that I went to the University of Massachusetts, where her husband was a surgeon and there I met, he had arranged for me to meet, Ray White. So off I went to Ray White's lab and this charming man showed me the X-ray plates with the restriction fragment length polymorphisms displayed there, and he showed me a draft of the paper, this was in March, the paper that was due to appear in May in the American Journal of Human Genetics, and that's the famous Botstein et al paper which I think many people would say was the turning point. I phoned from Ray White's lab, Cavalli Sforza in California, because he was coming to Wits in May, as the guest lecturer for our students congress and he commended the research as very exciting and so on; "Look forward to seeing you in Johannesburg in May". I think I went on to a few other places. It was quite a hectic visit, but the day that Cavalli Sforza arrived in Johannesburg, which was I think the 9th of May, I got my myocardial infarction, so I did not participate in the students congress as I was supposed to. But he visited me in hospital and he had the paper that appeared in the American Journal of Human Genetics, and it was then that I fully realised the implications and I remember him saying "You'd better get well, because now there is going to be really exciting work happening." So there was a very dramatic but historic period in my life, so then I was determined.

We had a very sympathetic director at the South African Institute for Medical Research and we were able to get the necessary equipment and so on very easily and, with Jennifer Thompson's collaboration, we enrolled two PhD students, jointly supervising them. They did theses, one on haemoglobins and the other using restriction fragment polymorphisms to study population affinities. I am happy to say that one of those was Anne Bowcock, who went on to be well established in the States after completing her PhD, and the other one was Michele Ramsay. So that was how we got in to Molecular Genetics. It may have been a bit slow compared with a lot of places around the world, but it wasn't too far behind.

PSH. It wasn't at all slow actually.

TJ. And we got launched that way. Then we really started from scratch. We didn't get any established molecular geneticists coming into the lab, so we grew our own timber, as it were, and then never looked back after that. So it was an interesting collection of coincidences perhaps, and good luck and we were away.

PSH. At what point did Michele Ramsay go over to work with Bob Williamson?

TJ. She completed her PhD and went to Bob's laboratory in about 1988. Anne Bowcock had finished her PhD sometime earlier and went off to the States because she was to marry Errol Friedberg at Stanford and she ran Cavalli's lab for a number of years before having her own department with a lab also in the States. Michele Ramsay completed her PhD at a more leisurely pace and then went off to Bob Williamson, by which time we had many students coming through then and doing their honours and higher degrees in molecular genetics.

PSH. The other development which I would just like to touch on a bit, what point was it that Himla Soodyall came into the department?

TJ. She and another very good student, Mandy Spurdle, came a few years later, I think perhaps after Anne Bowcock had left, so Michele would have been the main on-the-spot interacting supervisor with those students, and of course Himla went off to do her post doc with Mark Stoneking. Well she went to Penn State to work with somebody else, but when she was there, she met Mark Stoneking and went back to do a full post doc with him. She was very well trained in population genetics and molecular techniques. Stoneking's paper was '87 I think, so she was there then. Funnily enough, I recall meeting Mark Stoneking in '84, before the mitochondrial DNA polymorphism work.. When he and Becky Cann were travelling around the world collecting samples, they popped in on the meeting in Frascati, outside Rome, of the European Society of Human Biology. Derek Roberts was involved in that and I remember giving a paper there and meeting Mark and Becky. So that also had an impact on us, of course. Himla is still with us and carrying on her mtDNA and Y-chromosome work. I suppose she would be the closest to my successor, as far as that interest was concerned, and she has continued and expanded enormously the field work of population genetics and is the sub-Saharan Principal Researcher for the National Genographic Project, and she is going great guns.

PSH. One of the things which intrigues me, is that you must be one of really the very few people who bridged, not exactly the gap, but the development between what you might call classic anthropological human genetics and modern molecular human genetics, and can I ask, had you been continuing your population based work right through this period, or had you more shifted over for a while to the medical genetics side?

TJ. No I hadn't. It was very much anthropologically biased for those few years, and I don't know whether I told you how during my year with Steinberg, in 1968, he encouraged me, or tried to encourage me not to go into the medical genetics side, because there was so little that he felt we could offer. I didn't agree with him even then, but he was such a mentor for me that I did try to follow that. I went back to South Africa and didn't start clinics or anything. I concentrated on the laboratory and field work, all with a strongly anthropological slant, and I think I understand, I don't regret that, because there was so little going on in South Africa in genetical anthropology, if you can call it that, that there was a gap there too. Yes, what you've got to realise that in South Africa there were so few people in the field. Philip Tobias was a great personality and international figure, but he didn't really train students in the modern physical anthropology, biological anthropology. He was still measuring and I remember my first meeting in London in Barnicott's laboratory with Ronald Singer, a South African anthropologist, who had left South Africa before I got there, having been beaten by Philip Tobias to the chair, Dart's chair, and when Singer met me for the first time in London, he said "How's Philip and I said "very well thank you." And he said "Still measuring?" Which was true, and in fact I helped Philip measure on Saturday afternoons. We would go to the mine compounds, where Africans from all over Southern Africa came to work. We would "measure" them; it was very traditional physical anthropology. It was the old fashioned anthropology. But he was doing so many other things as well.

PSH. Did at that time, the South African political situation cause difficulties for you in terms of getting samples from the rest of Africa, and links of that kind?

TJ. Oh yes it did. I did my field work back in the territory of the Zambezi valley from Johannesburg, and went even further north to a Leprosarium I remember near Lusaka, near Broken Hill and I collected samples in that vicinity, with the co-operation of a Welsh pathologist there, Griffiths, Glyn Griffiths, and I did a few trips to that area until it became rather embarrassing, because he would introduce me as coming from Wales, because he was a bit embarrassed or nervous to introduce me as coming from South Africa, and I remember going in a land rover doing field work and got on very well with the Zambian driver and helpers and so on, and when one day one of them said to me, “Have you ever been to South Africa?” I had to take a deep breath and say ‘Yes’. And he said what’s it like there? And I gave him my honest opinion about it, but I never confessed to him that I was really working and living there; it was then that I decided I couldn’t do that anymore and so I stopped trying to work in Zambia.

I didn’t do much work in Rhodesia, as it was, after that time, but I expanded the work in Namibia; of course,. South Africa controlled Nambia, and that enabled me to expand our work on the San and also all the other tribal populations there. In fact Herman Lehmann accompanied us on one of those field trips. That would have been about 1973 I guess, and with his German background it was most entertaining. Because Windhoek is a very Germanic town, or was then, and Herman had the most marvellous time there you see. He could have imagined he was in Germany and spoke German to everybody, and we sampled the delicatessen food that we were going to take, which sausage should we choose and which salami, stocking up before going into the field and roughing it! It was marvellous doing work with him, and you might remember a paper that we wrote together in the BMJ on sensory polymorphisms. Because he always liked to have one up on Weatherall, as you know, and to have a paper describing field work like that was something he felt David Weatherall didn’t do! So Herman is a co-author of that paper with a fascinating title I might say, which is so long but for a good reason! It reads: “Public health and genetic constitution of the San ("Bushmen"): carbohydrate metabolism and acetylator status of the !Kung of Tsumkwe in the North-Western Kalahari.

PSH. One of the things which I find amazing, Trefor, is the extent of your international links and collaborations over all this time, because it seems really that you were tied in both on the technological and on the broader fronts with very, very good people in other countries, and yet it must have been quite difficult, with your base in South Africa. How did you manage to develop all these links?

TJ. Well I think, without being too cynical, I think it’s a very interesting area of the world to visit and so although some people, you being one of them, wouldn’t come to South Africa because of the political situation, others didn’t feel that an issue at all, and others wanted to disguise it when they came, and Herman was one of them. He was a consultant for WHO, so he didn’t want it to come out really that he was breaking the embargo on South Africa. So he did have some anxiety about it but he overcame that somehow and had a great time, of course. His wife, Benignia, came with him. She told me later that it was a wonderful visit, because he hadn't done any field trips for a long time; they had had a family tragedy and that this was a sort of "coming out" for him. He was relaxed and had a great time. So I think it wasn't me. I didn't do anything for him much. It was the attractiveness of the location where I happened to be working. With a lot of conscience, my own of course, I didn't feel I could stay in South Africa if I were exploiting the situation and so, I also, when doing field work, was conscious very early on of the accusation that I might be exploiting people who didn't have the autonomy to give proper informed consent. But I overcame that somehow, and I think that one just has to say, yes I may have been selfish and not being supportive of the campaign if you like, the campaign for change in South Africa, but I do believe there was room for internal change to take place, and so, without looking at other people and what their motives for coming might have been; I did look carefully at my own.

PSH. During this time, your department was virtually the only other department in South Africa involved with medical and human genetics. How did things work on a countrywide basis, in terms of links with the other elements that were in the field in South Africa.?

TJ. Well there weren't any human genetics departments in the Afrikaans universities but Peter Beighton of course came to South Africa in the early seventies, already established as an authority on a group of diseases. What are they called?

PSH. Bone dysplasias?

TJ. Yes. Well even narrower than that. He wrote a book on Ehlers Danlos.

PSH. Ehlers Danlos yes, connective tissue disorders.

TJ. Yes, so he had, I think he also worked with McKusick.

PSH. I think he did.

TJ. And he came and worked in the Orthopaedic Department in Wits on I think a Fellowship of some sort, and he was an enthusiastic field worker, so he did lots of surveys into bone dysplasias and made contact with me straight away, In fact I think we did some typing with him, in collaboration with him, on polymorphisms in many of the cases. And he didn't take a permanent job in Johannesburg, but a job came up in Cape Town, the Chair, which was three years before Wits created a Chair, so Peter took that job. We did some things together, for example, each of us went to Tristan da Cunha. He, I think a year or two before I did, but we shared samples we collected there, and he also, I think went to the Kalahari. He was a great traveller of course, which he had started before coming to South Africa, crossed the Sahara on a camel with one other person. So he was in Cape Town and we had a very positive relationship. Our interests were so different that we didn't collaborate very much, but he certainly developed genetic counselling services in South Africa and he chose to do his outreach, let's call it that, by holding clinics in many centres and rural areas, which I didn't do. I gave expression to my sense of adventure perhaps by going to the Kalahari and doing anthropological field work, but he went collecting cases of rare diseases and so on, and many of his students who did PhDs with him went on to become very well established in the field internationally.

PSH. I'm interested that there was nothing in the Afrikaans universities.

TJ. Yes.

PSH. Was that chance, do you think, or do you think it kind of reflected a sort of inwards look.

TJ. I think it did. I think that whatever we might have felt at the time, in retrospect the Afrikaans universities were isolating themselves, even from the English universities in South Africa, let alone the rest of the world, so I think that it would be fair to say that apartheid had a negative effect on the Afrikaans universities. That's changing of course, and now they take black students, of course, which they never did pre-1990, and they have more English than the ones who, say, were most narrow in their outlook and only read Afrikaans literature, were depriving themselves, obviously. But I think that's changed and had been changing and I never had any problems relating to the Medical Research Council, which was strongly influenced by Afrikaans colleagues. We from English universities were included in delegations that they sent to Israel. There weren't many countries that they could send delegations to, but Israel was one of them. I remember going there, together with Tom Bothwell from our own university, Stewart Saunders from Cape Town, and it was apparent on that trip, which was in about 1979 or so, that the Afrikaans-speaking people and the English-speaking people didn't integrate too well and I naively said to my English-speaking colleagues, "Shouldn't we sit on the same table?" at breakfast, and they said "No, relax, relax" and I felt uncomfortable with that. Anyhow I think I did relate to them. It caused a bit of a problem when we had a protest against apartheid in Tel Aviv, as we left a lecture theatre one morning. There were Israeli students outside carrying placards saying 'Down with apartheid' and, in smaller type, 'In Israel as well as South Africa' and I asked if I could have a photograph with them, and so some of my Afrikaans colleagues didn't like that. And, in fact, yes I remember, I was excluded from the members of the group who gave a press conference when we got back. Yes, at the airport there was a press conference and I was not included in that.

PSH. Trefor, you went to South Africa in '63?

TJ. No, to South Africa at the end of November 1961.

PSH. At what point did you start really getting, really feeling that you had problems with the system there, because over many years you have shown your opposition to apartheid, but how did it develop?

TJ. Well I agree, it was there from the beginning, in fact since reading "Cry the Beloved Country" in the late forties, as a young schoolboy almost, I knew there were problems in South Africa. So I knew they were there and I didn't go to Africa with a view to going to South Africa, I went to Rhodesia as it was, and due to circumstances, fortuitous ones, I thought we would come back to the UK *via* South Africa, just to visit there to confirm how bad it really was. It never entered my head that I would ever continue to live there, and become a citizen of course. So when we went to Durban after eighteen months in Rhodesia, I still wasn't thinking in terms of staying, but it was the attraction, well fortuitous friendship with Errol Friedberg in Durban, he was the newly qualified houseman and I was senior houseman, and we became friends, and it was his going to Wits, back to Wits where he graduated, to do his post graduate training, that I went with him. He went up for an interview one Saturday and I went with him to meet Tobias. Because he knew that Tobias had done field work in the same area where I had done serological work, in the Zambezi valley, he on the north side and I on the south bank of the Zambezi. I went and met him and then he offered me a job and so at the end of the year the Friedbergs, Erol and Sylvia went to Johannesburg, and Ada and I went to Johannesburg with our three children. And again, I still hadn't thought I would stay, but I got caught up in the excitement of genetics, and particularly population genetics and the fieldwork that went with it.

PSH. So when did it start having a practical impingement, the two things couldn't just be kept in watertight compartments?

TJ. No. I think that I gave expression to some of my political unease, if you like, through the church, the Methodist church to which we belonged, because there were always really great leaders in that church, including African leaders, who were strongly against apartheid. A lot of the members of the church were not, of course, and that was another story where I found myself in conflict with some of the conservative white members of that church. But I don't know, hope springs eternal they say, and I can't honestly say I thought change would come, but it was interesting and in a sense, exciting, to be part of the change. So I did all the right things, Institute for Race Relations membership and actually I put on a symposia for our local church on 'Race' and got authorities to speak on that from the church, and I would feed in a bit of biology and so on. And Tobias himself, you should remember, was a great champion of opposition to the science on which apartheid was supposed to be based. So he and I were very close when it came to protest. He had been doing it for many years, but I certainly identified with his leadership and followed. So there were always people of like mind which made it easier. I can't say I was particularly brave Only once was I visited by the security police and they denied it was to investigate me but somebody else, an American anthropologist whom I knew. So I don't know whether it sounds hollow, that sort of explanation. Perhaps you could say I was selfish and it was an opportunity for interesting fieldwork, and I therefore compromised on opposition, but I don't think so. There aren't many Biko's or real martyrs and it was very interesting and I think worthwhile.

PSH. At what point did the Steve Biko affair appear and how did you get involved there.

TJ. Yes, well I had never met him, and I have to confess I hadn't heard of him but when I went back on things I had read, after his death had become big news, I realised that he was well-known in student circles and also there is a National Geographic article of about '75 or so, in which he is shown as one of the leaders. Now in South Africa this was concealed in a sense from us, with censorship of the press and all the rest of it. We didn't know, many of us, what someone like Biko was doing. Anyhow, we all knew, of course, when he died and I remember straight away, or as soon as it became apparent that the medical profession was not taking any action with regard to the conduct of the doctors who had been responsible for his care, when that became apparent, I phoned up a forensic consultant in Johannesburg, Hillel Shapiro, and said 'Well what shall be do as doctors about this? Shouldn't we lay a complaint against them?' He reassured me that the Medical Association or the Medical Council itself of which he was a member, would be taking action sometime and he said "Relax Trefor", that sort of thing, it will be done. But of course, it dragged on and on for various reasons, legal reasons. And then it became apparent that nothing was going to be done, because the medical authorities, the Medical Council had decided there was no case to be answered by the doctors, so it started off with large numbers and mass meetings and so on; when it came down to 'well what are we going to do about it?' and 'who is prepared to give some money towards it'. We ended up with two groups of three doctors. So there were six of us that saw the case right through to the appeal court.

PSH. What did that involve? It's something so familiar to you, but I think that for most of us outside, we know that you and your colleagues were involved, but not exactly what happened.

TJ. Yes. Well the inquest into the death of Biko, which was required by law, found that he had died from a head injury inflicted or sustained during a scuffle with the security police, and no one or persons was responsible for that. And that was how the case ended, except that the presiding magistrate was of the opinion that the conduct of the doctors, not of the security police, might need to be investigated. So he sent what was a de facto complaint to the South African Medical and Dental Council, (i.e. the relevant pages of the report of the inquest), and that went to the Medical and Dental Council as a complaint against a few doctors, two in particular. And the council set up a preliminary committee of enquiry, which looked at the complaint, and they came to the conclusion that there was no complaint to be answered and therefore they would not set up a full committee of enquiry, and that was the end of the matter. And so we had a mass meeting and we decided that there was a complaint to answer, but we should take legal opinion, so we chipped in the money to pay a top advocate, a lawyer who briefed an advocate named Sydney Kentridge, a great lawyer, and he gave us an opinion, that if we wanted to appeal the decision of the Medical Council, which was a decision regarding the complaint from the magistrate embodied in the proceedings of the inquest, we would have no 'locus standi'. We needed to lodge our own complaint and then we would have locus standi. So we now had to engage a lawyer, and we had a wonderful human rights, lawyer, Richard Rosenthal, son of Eric Rosenthal, a wonderful man. We briefed him for the case and he produced a document, some hundreds of pages long, as a complaint to submit to the Medical and Dental Council. We went through various attrition processes, starting with eight doctors and gradually cut down to three. Some fell by the wayside, when it became apparent what financial implications there were. Anyhow the three of us remained. There was Frances Ames, who was the Cape Town neurologist and psychiatrist, and Philip Tobias and me.

We lodged our complaint, and then another group of three doctors, (one African, one of Indian origin and one white) felt they wanted to lodge their own complaint - understandably, and we were sympathetic; but it was a separate complaint. They relied heavily on our lawyer's work in the first place, but they had their own lawyer to formulate the complaint. To our surprise, the Medical and Dental Council ruled that we did not have a serious complaint and they declined to set up a committee of inquiry. Now we had the locus standi to go to the Supreme Court and ask them to rule and to direct that the Medical and Dental Council should hold an inquiry, which they had never done, and that was quite interesting and exciting because we went with our lawyers, Sydney Kentridge was our advocate representing us; the other three colleagues had Ishmael Mohammed who became the first Chief Justice in the new dispensation of South Africa in 1994, to present them. And then another advocate, Dawid de Villiers, who was a friend of Frances Ames, volunteered to act for us as well. He and Kentridge were certainly doing it pro amico, pro deo, We went to the court and the judge, who was a very conservative judge who had a record of sending people, I think including Biko in fact, to jail, a very pro-government sort of judge, and he after about four days of hearing, we gave no verbal evidence, it was all in the documents, ruled (with another judge concurring) that there was a case to answer and that the Medical and Dental Council was required to hold an inquiry. Now that put them on the spot and they had to hold the inquiry, which duly took place. We saw the doctors give evidence and the police colonel, Goosen, gave evidence and it was very exciting because when the judgement was delivered, a month or so later it vindicated us really, ruling that we did have a valid case. The Medical and Dental Council subsequently met and ruled that one doctor had behaved unprofessionally and disgracefully and the other had behaved unprofessionally. And one was eventually struck off the register and the other was reprimanded and given a suspended sentence. So we felt that we were vindicated, but more importantly, Biko was too. That stands as a very important case; from the point of view of our profession, the judge ruled that the only defence that the Medical and Dental Council had was that we did not have locus standi to raise a complaint. We were not going to suffer as a result of the council's ruling. They sort of implied that if Mrs Biko had complained, then they might have ruled she had a case and should be compensated, but they said that doctors, who were not going to be affected adversely had no grounds for complaint. The judge ruled that not only did we have the right to complain but we had a duty to complain. So that's the ruling and will be important in future cases as well. That we are each one, in the profession, responsible for the integrity of the profession.

PSH. That must have had wide international implications.

TJ. Yes, although I don't know. We are having a re-run of this with regard to the Guantanamo Bay case, and did you see a letter recently in the Lancet?

PSH. I don't think I did actually.

TJ. A very fine neurologist in Birmingham, David J Nicholl, has recently published a letter in the Lancet, September 8, 2007 with 400 signatories, about the appalling behaviour of the Americans in not holding an inquiry into the conduct of doctors there and it was published on the 30th anniversary of the death of Steve Biko, and I'm involved in that. So it was a relevant decision I think.

PSH. Trefor, on the wider ethical front, was it the Biko case or was it other things that made you aware that there were areas of scientific ethics that you needed to be involved with.

TJ. Well, race classification was certainly one, and we were involved in that, not from a legal point of view, but we would have cases come to our genetic counselling clinic. I remember one, the sister had put down diagnosis 'query albinism' because a patient had phoned up who said they were concerned about skin colour and when the patient came into my office, a man and his wife, and I won't mention any names, but people who know the situation will work out who it was. I said "Welcome. What have you come to talk about?" and he said "Does my name mean anything?" and it did, because he was the brother of a girl who had been at the centre of an infamous case; really, of a schoolgirl being expelled from school, a white school, of course, because she was black or too dark. It was in the low veldt of South Africa and was a Sunday paper case, of great notoriety. And this brother had come because he had now married and he was concerned that he and his wife might have a child with what they call a 'throw-back'. I took a history and so on. I suggested that there may be some other factors operating in the family. Would he be prepared to ask his parents and his sister to submit to parentage testing for example. Of course he didn't want to and I was grateful for that. But that sort of case and there were others that I could relate but that was a particularly startling one and if, as some of my colleagues felt, we should not get involved in these cases. If they had their way, I think we would not be behaving in a compassionate, medical way. So I did get involved with cases of race classification.

For example, there was an advocate in Johannesburg, a well known advocate, who seemed to have a lot of cases where he fought for "re-classification", so called, of people, and he would take a case to the courts after having gone through a race classification board, as it was called, which consisted of lay people, largely, who pronounced on the race of an individual and he would bring cases to me to have genetic testing of the blood. And we would test for a number of polymorphisms that we did as routine in our anthropological work, and he would take that evidence to the supreme court, the court of appeal in Bloemfontein, and would come back and tell me the judge wouldn't admit my evidence, which was fair because it was really worthless from a genetic point of view! I would say, whatever I found, that "These findings are compatible with the subject being classified as -- (whatever they wanted to be classified as), usually as white, and I would say, 'in the South African context'. There is now a study being carried out by a PhD student on race classification in South Africa, and exposing it for what it was – "perverted sociology" . I've anonymised my families, (23 or 24, I think) and she is analysing those in the context of all the other cases that she has gleaned from the legal records and so on, to expose this and draw comparisons with Nazi Germany I guess, so people may learn from history.

PSH. What other particular ethical issues have come up, that you have been involved with, have got a particularly unique flavour to South Africa?

TJ. Well, we did a lot of parentage testing of course, but they were very strict maintenance cases. I don't think there was anything unusual there compared to other countries. Could you explain a bit more what you are looking for?

PSH. I suppose what I am thinking is whether the ethical areas you have been interested in have been ones of general international significance, in terms of medical and human genetics, or whether they have been ones with a local situation.

TJ. Well, as you are well aware, the HIV epidemic is an enormous problem and will be in South Africa for a long time, so I have got a bit involved in the debate on selection of blood donors and I think we have been rescued in a way, my view initially, let me be honest, nobody had the right to be a blood donor. I'm firmly committed to the "gift relationship" of Richard Titmus, that a voluntary blood donor service, unpaid, is the only way. So starting from that principle, and what is more, the interpretation of the motivation of the donor is one of altruism. So I take that as a gift. Not everybody does, but I do. And if that is a given, then I believe that nobody would want or would insist on being a blood donor if they had the slightest suspicion that they may be infectious and hazardous to a recipient. Now what we had in South Africa, going back to the fifties, long before I got there, was a segregated blood donation service. According to law, every unit of blood that was collected had to indicate on it, not only the blood group and rhesus status, but also the race of the donor, and incidentally, in passing, that was a law passed at the request of the South African Medical and Dental Council of South Africa of the time, criticised by the way, at the time by British blood transfusionists. An article in that wonderful Penguin series in Biology,

PSH. Pelicans?

TJ. Pelicans, yes Pelicans There was a series that went on for years, New Biology.

PSH. Yes, was it was New Biology, I remember.

TJ. You know, wonderful collections, Tony Allison edited some of them, anyway wonderful; Haldane and Huxley, all had papers in it. One of them in 1959 ran an article on the South African blood transfusion situation written by Dunsford, NBTS, Sheffield. Anyhow, when I arrived in South Africa the blood bottles were all labelled with the race of the donor. The blood transfusion service had started in Johannesburg before the second world war, with paid donors, unlike Britain I think. Oliver in London had a Red Cross blood transfusion service. In South Africa it was the South African Institute for Medical Research in Johannesburg, and, later the University of Cape Town, that set up blood transfusion services with paid donors in, Johannesburg, certainly. Generous payment, £5 for a unit of blood, and this is an aside perhaps, but it was medical students at Wits University who protested against the payment, although some of them were the recipients and doctors were also recipients of the £5. Anyhow, it was in the immediate post-war era that the South African Blood Transfusion Service was set up, with Dr Maurice Shapiro as its first director. He was a remarkable man and set up a first class blood transfusion service, praised by Geoffrey Keynes, who had helped set up the blood transfusion service in London after the first world war, and appealed for donors on the BBC and that sort of thing. Well the blood transfusion service in South Africa functioned well but there was definitely a paucity of black donors. One sociological study carried out in Durban in the mid sixties, to try and find out why there were so few black donors, a study which Titmus cites in his famous book, “The Gift Relationship”, showed, as he said, that the black donors in Durban were not voluntary donors. They were what he called, captive donors on a par with prisoner donors, because the African schoolchildren would be brought up to donate because their headmaster told them to and they never donated again; or they were factory workers who rolled up to be bled because the foreman said they should be bled and they never donated again. So he didn’t analyse South Africa in any more detail than to review a report that had been made by the Natal blood transfusion service. Well, when I got there I did get involved in blood transfusion work because one of my early jobs was attached to the South African Institute for Medical Research Blood Transfusion Service, and I felt extremely uncomfortable being the doctor supervising the collections, for which we paid each miner they were usually the gold miners, one rand which was 10 shillings at that time, a unit of blood. But I was very junior and I was passing through and I did nothing about it. However, Shapiro with his vastly bigger blood transfusion service, did not pay but he had very few black donors. In the seventies, when the epidemic of hepatitis B was identified and the enormously high prevalence of positives in the African population, he wrote then, it’s there for anyone to see, that he didn’t want the race of the donor put on the blood. He was against that from the beginning, but he did concede that he would only give white donors’ blood to all recipients if he possibly could, and he could except in a very small proportion, a few per cent of recipients. There was no blood for them except black donor blood, because he collected black donor blood, he would give the black donations to black recipients. He tried to justify that on the basis that members of the black population were more likely to have antibodies to the hepatitis B than would members of the white population; now that is completely unethical in my opinion. If it is ‘dirty’ blood it shouldn’t be given to anyone. Anyhow he wrote that and when the HIV epidemic began and it was projected in ‘88 or thereabouts, that it was going to increase enormously, the blood transfusion service, under Shapiro still, but increasingly changing over to other people directing it, good blood transfusionists, they decided that they would continue to collect blood from black donors who presented, but they would not use the whole blood for transfusion. They would take off the plasma, store it on the shelf for three months and then use the plasma but they would throw away the red cells and other components. Now, I think most people in the field would know that that was going on, but it was only a disgruntled nurse working for the transfusion service, when she was apparently dismissed because she was incompetent, who took revenge by going to the press and telling them that she couldn’t carry on in this job any longer because she was being untruthful to donors, telling them that their donation would save lives when she knew it was going to be destroyed. Now that of course caused an uproar. I don’t know whether, it got any publicity in the UK?

PSH. No.

TJ. Oh well, it's a very interesting story. So she went to the press and the Minister of Health said 'this must stop, this is racism.' The press, a lot of the press felt it was racist and must stop immediately; the blood transfusion service had been trying to increase its black donors by the way, because in future it was predicted that white donors wouldn't be sufficient, confessed what they were doing but tried to justify it by saying they wanted the safest blood for all recipients. I remember a letter from one black mother saying that she wasn't interested in politics. She would prefer to have the best quality blood for her child whether it came from black or white. The blood transfusion service set about this problem in a very commendable way. With the approval of the Department of Health which they had failed to get the previous two years, they got approved the purchase of mass PCR machines, to be able to identify the HIV RNA in every blood sample to be used for transfusion. The window period would be reduced from some weeks down to perhaps 3 or 4 days; This resulted in a 20% increase in the cost of blood, This strategy, together with an emphasis on the use of blood from regular donors, in other words, after someone had donated for two years and all the samples had been HIV negative, and they didn't test for hepatitis B by the way as well, and hepatitis C, they then would issue the blood. With the increased cost and strict implementation of the regular donor system, they are able at the moment to cope with the demand. I have found, in the last couple of years, that this is the most interesting ethical problem, I have written on the topic because the implications are that, if it is shown that this scheme is not successful and people are going to get HIV from blood transfusions, then I think that there will have to be another strategy, but to use race as a surrogate for HIV positivity is obviously not desirable; but I do have a problem. I think what will happen will be, because the people in the know, like doctors, will strive for designated blood donors, as you have in some American hospitals, I understand. Relatives will be recruited, and tested perhaps, to serve as donors if the need arises, which could divert, perhaps, regular blood donors from the transfusion service to an inward looking family set up or neighbourhood, or whatever. I think the Jewish population in Johannesburg have already enquired about this possibility, by the way, the very strict orthodox Jews, so that would be counter productive and it would mean that the private sector would set up such services and the cost of blood would go up enormously and the people who would lose out in the end would be the poor people who are having to use the regular national blood transfusion service. This is a very challenging problem. We have had one preliminary report after six months of the new programme, and it is looking good.

PSH. Looking ahead to the future Trefor, how long now is it since you retired?

TJ. Nine years.

PSH. How do you see medical genetics continuing to develop in the South African context?

TJ. Well, since I retired, the South African Institute for Medical Research has been taken over by the state, as part of the National Health Laboratory Service; this has meant effectively an injection of a lot of money and the creation of posts for medical geneticists to be trained. When I was active we were funded by a private body, the South African Institute for Medical

Research. The State would not give me posts to take on clinicians for training. My successor, a very good clinician, Denis Viljoen, has however succeeded in getting significant numbers of training posts for medical geneticists and he has already trained a number and this will continue and may even expand. So there will certainly be the possibility of increased numbers of specialist medical geneticists in the country. We have already supplied one, I believe, for Durban and one of the graduates from Denis's department has gone to Cape Town, where there is a non medical person as Head of the Department; and I think, two have stayed on, in our own department, Human Genetics at Wits/NHLS and this should produce a number of people to cover the country, in a way that wasn't feasible under the old system.

PSH. Do you think you are beginning to reach the black African population in terms of genetic services?

TJ. Well, increasing numbers come to the clinics, there is no doubt about that. There is undoubtedly a greater demand from the black population, a greater awareness and better educational standards, and medical insurance, that makes a difference too. Although the majority of patients will be from the State Health Service, increasing numbers of private patients, people with medical insurance, come for counselling too. I don't honestly know what the proportions are at the moment, but it is going to increase, the proportion of blacks to white will increase over the years. But the gap between rich and poor, which correlates with the educated and not so educated, is still very great. That's a national problem, not affecting health only, and certainly not affecting genetics only. But I am optimistic that the country will develop and with it, better education of the formerly disadvantaged peoples will result in greater equity in the distribution of services.

PSH. Trefor, I would like to finish. I have been asking everybody I see two questions and one of them is, is there any particular person or people that stand out in your mind as having influenced you and been formative in helping you develop your career in work in human and medical genetics?

TJ. Well, I think that my post doctoral fellowship with Arthur Steinberg makes me think of him first. I suppose I had more contact hours with him, than I had with anybody else in human genetics. I told you the story I think about him not wanting me to go into medical genetics, because he was so frustrated personally with counselling, but I think as a role model, he has had an important influence on me. I think also Phillip Tobias, because interacting with him, first on his staff and then across the road in another department, and his efforts in creating a Chair in human genetics. In addition, his role model for the social aspects of our field has been very important; I would say that unhesitatingly. So those would be the two people who stand out; and then, being far away from the centre, these twenty four Philip von Wielligh lecturers who came to South Africa to share their knowledge and expertise, you being one of them! Malcolm Ferguson-Smith was the first and almost every year since then we were able to bring someone from the US or UK. That was important. I was privileged to go to Cedric Carter for a couple of clinics, but being in letter contact was very important with so many colleagues. But the two most influential would have been those I suppose. Yes, as you might have experienced yourself, there was nothing in our education, undergraduate education, to suggest that genetics was important. Fascinating really. I have been interested to read the history, what was going on say in Race and Sanger's lab when I was in London. We weren't exposed to those. I did go to a lecture by Penrose, I think one evening at University College, but I don't think we heard much about genetics in our training.

PSH. The other thing I have been asking everybody is, is there one particular piece of work or area of work which you feel you specially identify with, if you had to just keep that one bit, that you would feel well that's been something that you can feel a special identification, an affinity with?

TJ. I don't know whether I can answer that. I think that for me, I would have to say the excitement that has come from the use of molecular genetics in the diagnosis of genetic diseases and in the study of human evolution beginning at the end of the 1970s. I think so many discoveries have been made in the recent decade or so, two decades at least, that have excited me tremendously, things like the dating of human origins, whatever that means, and the emergence of language. I think that these are probably what excite me most, and. of course, it is very challenging to one's whole thinking; and friends of mine and family would challenge me on my views on evolution. They may say, "Well, how can you believe in evolution? It can't be blind!" So although I don't go along with Dawkins and all that he says, I think that the excitement for me about science is the contribution to our understanding of where we come from, and I would hope it might teach us something of where we might go on to, but certainly that must be it. Yes, I think I would have to say that's it, without thinking of myself as a great evolutionist or anything, I think that's the excitement about being in science at the moment!

PSH. In fairness, it must be a factor that you have been able yourself to contribute to this new field of work, while having been one of the ones that was involved many years ago at the beginning too.

TJ. Oh yes. I couldn't possibly have envisaged how it would have developed. So, yes. it was the challenge early on: where do the Bushmen fit in I suppose and how could the Germans in South West Africa have been serious when they thought they were dealing with a different species! Yes, I don't think I was ever taxed by the magnitude of that problem, but really the whole process of human origins and evolution is an exciting field and to have seen some almost approaching to certainty, of what's happened, evolutionary speaking, is exciting, yes.

PSH. Trefor, thank you very much. I shall stop the recording there.

End of recording.