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Interview[1] with Piyo Rattansi[2]

Part I and II (02/09/2016)[3]

R. Uchôa:[4] It is an honour and a pleasure to do this interview with you. I would like to explore some elements of your biography. In the first part of the interview I would like to ask about the historical context of your life: camming from India, then moving to Africa.

P. Rattansi: Thank you. I am very glad for this opportunity. It always nice to be able to talk about my own past, biography, memories and so on. My parents were among a large number of people who migrated from India to East Africa in the early years of the XX century. My father came to India around 1910. He was a young man of 16. He came from a small village in what is now called Gujarat. He learned to read under the electrical light of the post office of the Village. He went for a short time to Mumbai, where his brother had started a business. They heard that the British were building a railway in Kenya. They were recruiting people that could work on the construction of the railways. But they also needed people with abilities as station masters and so on that were also recruited from India. My father became very interested. He was an adventure person. He took the ship from Mumbai to Mombasa and started working with this British firm that was recruiting people from India. The plan was, as the railways advanced, they would set up a store in each place to trade with the Africans that lived there. Trade for ivory, skins and other products. He was transferred to a small place in the Mount Kenya. The mount from which Kenya derives its name. The biggest mountain in Africa is Kilimanjaro, that is Tanzania, which the Germans occupied. Kenya was occupied by the British. My father worked in a station called Nyere, which was in the foot of Mount Kenya. After this whole enterprise of setting up stores died, after the dissolution of the mercantile

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[1] The interview took place at Center Simão Mathias for Studies in the History of Science (CESIMA), at Pontifícia Universidade Católica de São Paulo (PUC-SP). The interview was performed on the occasion of his visit to CESIMA as part of an exchange program established for the Thematic Project "Revealing natural processes through the laboratory (phase 2): the search for the principles of matter in the three kingdoms until the specialization of science in the 1800s", funded by Fundação de Amparo à Pesquisa do Estado de São Paulo (FAPESP).

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empire, my father had the opportunity to buy up the store in Nyere. This is where all of us was born and grew up. It was a large family, five brothers, and five sisters. Of course, he had to go back to India at some point to get married to a woman who lived in a village not very far from the village my father was. My mother was very young at that time. When she was 16, they got married and went to Kenya. So, we all were in this place called Nyere, a very beautiful place in the foots of Mount Kenya. Not a very tropical place, because it was situated in the Highlands, which was also a place suitable to Europeans to live. I was the youngest of the sons.

R. Uchôa: How many brothers did you have?

P. Rattansi: Five brothers and five sisters. Very large family.

R. Uchôa: How was your father's work there?

P. Rattansi: Because the Europeans were able to settle in Kenya, in the highlands, unlike Uganda. In Uganda, there was Malaria with a tropical weather. There were a lot of settlers that settled close to Nyere. So, he opened the shop to supply the needs of these Europeans, mostly British costumes, who lived nearby. This was how we got used to the idea of a British cuisine. Most of the people came after the first world war. People who had been officer in the army. The first school I went in Nyere was built by my father. It was a costume in these towns that the government would provide money for appointing teachers and supervising the curriculum and test the educational standards. But the schools would not be used by India merchant at that time. Education was as aspect of society that was racially stratified. There were the English settlers on the top, middle layers made up of Indians and the lowest levels made up the Africans. So, schools were provided for the Europeans and for the Indians by the Government. Most of the African education was made by the Christian missionaries.

R. Uchôa: Was that just elementary schools?

P. Rattansi: Yes, if one wanted to go to secondary school he had to go to Nairobi, which was the nearest big city to the town. Two of my eldest brothers were sent to Nairobi to attend the secondary school. There was still the problem with the rest of the family to continue with its education. This led eventually to our family to migrate to Nairobi, and my secondary education was also done at this city.

R. Uchôa: Were your father and mother supportive of the education of all the children?

P. Rattansi: Yes, there were five brothers. Most of them were very keen in getting a higher education after getting a secondary education in Nairobi. Generally, in Kenya, after getting secondary school people went to be lawyers, doctor or

pharmacist. These were the usual profession they took up. Even though one of my brothers got admission in pharmacy, my father said he needed manpower for the shopping in Nairobi, so it had to be canceled. So, none of them got the opportunity that I got at the LSE. There was very little opportunity for getting higher education at the time. Uganda was more advanced in the respect. There was the recently founded University of Makerere, but normally one had to think in terms of going to India or Great Britain to get higher education.

R. Uchôa: Did your sister got education as well?

P. Rattansi: My sister did go to the school in Nyere. Then we migrated to Nairobi. In Nairobi the education of boys was financed by the government. The education of girls was taken by various communities. We belonged to the Aga Khan sect of the Ismaili. So there were Aga Khan schools for girls.

R. Uchôa: Could you comment something more on the religious background that you just mentioned.

P. Rattansi: My father was part the Ismaili in Nyeri. We attended the Ismaili mosques in Niery. For the Aga Khan, the education for girls was very important. Education, in general, was very important for the Aga Khan, so we all benefit from that too.

R. Uchôa: Did all the sons followed the same path as your parents?

P. Rattansi: Yes, it was not a very demanding religion. Among the Sunni Muslims, the great emphasis was put in the teaching of the Quran. The idea was that one should be able to master the Quran at a certain stage. Ismaili are different in this sense. At some extent, their system education did not concentrated so much on the mastering of the Quran. It is difficult to tell, but we had the impression that it was superior to the other kinds of Muslims.

R. Uchôa: What was you study trajectory, coming from the elementary school towards higher education?

P. Rattansi: My father had a great impact on that. Although he was a person with very little education, he had a great level of literacy reading in the Indian language we spoke: Gujarat. It has quite a rich literature. He himself wrote very beautiful Gujarat. The house was full of books in Gujarat. And, as my brother studied in Nairobi, they accumulated English classics. With all the books belonging to both traditions, we all became very good in languages. These were my best subject in school: Gujarat and English. And of course, we also had access to Latin literature. But the other subject I became interested was chemistry. By the time I got into the secondary school, I became more interested in different subjects. Psychology,

Psychiatry, and Psychoanalysis were among them. I was around 13 years old. But that time I read Ernest Jones's lectures in psychoanalysis. I even considered becoming a psychiatrist eventually. Then, I was told that in other to be a psychiatrist, I had to study five years of medicine plus three years of Psychiatry. By that time my father was well off and could afford a course in medicine in India or Britain. However, while I was finishing my secondary school and waiting for a response from the Medicine faculty at the University of London, I joined an Indian newspaper that started in 1914. I finished school in 1947, and I joined this newspaper in 1947. I became so interested in journalism that I stayed 4 years with this newspaper. That was quite an awakening. The paper became more and more identified with the cause and improvement of the conditions of life of the Africans, although the staff was Indian. It was at that time that I realized I was in an African country, that this people had its own needs that had been neglected. By that time the independent movement pro some kind of self-governance was going up among the Africans. This was affected by the Indian independence movement. We also felt that this should also happen in Kenya, with the independence of the Africans. Then, I started studying the African needs and so on. I gradually became involved with politics and with stratified nature of the society. However, the paper also took some left-wing stance. We became associated with new trade union which had been organized in Kenya by a man called […][5]. He was himself a communist. He was involved with the communist movement in India. Eventually, he was put in prison by the British movement. But he came back to Kenya, and he tried to organize the trade union in Kenya. This called the attention of the authorities that saw it as dangerous developments. The paper came under the suspicion of being influenced by communists that were trying to subvert the Africans. So, the paper started been persecuted with the charges of sedition. Sedition very widely defined as someone who incentive the disobedience of the colonial authorities. They started to take some of the journalists to court for writing some particular articles. Often, they had to change the names of the editors, because of the persecutions. The paper was not widely supported by the Kenyan society. Actually, there was not enough educated Africans. Literacy among Africans was not very high. So there was not enough people able to support the newspaper. There was Indians leadership that wanted tactical allegiance with the Africans. Afterwards, various companies that used to give support to the paper stopped given support to us. Their arguments were that the Indians had told them that they were inciting the Africans and in the end, the paper was taken over by somebody from the Indian leadership that we had criticized and we were all dismissed. It happened around 1950.

[5] Editor's note: During the interview transcription process there were excerpts of difficult listening. These passages were flagged in this edition by "[...]".

R. Uchôa: You mentioned your perception that you were an Indian living in an African country. Tell me more about the cultural identity aspects of this experience.

P. Rattansi: The key point is that you are in a stratified society. You don't notice that you are discriminating racially. Africa was my home and my house we had three kinds of servants: a cook, a Gardner and the "house boy". They were not given a housing that allowed their family to join them. It was difficult for us living in that society to notice how badly we were treating them. There was exploitation in this context.

R. Uchôa: You mentioned you applied for Medicine in London, but then you got involved in the Newspaper, how was this transition?

P. Rattansi: Earlier I decided to apply for medicine. But after four years working in this newspaper, I became much more politicized. I left school with 16 years old. I got into the newspaper with 17. I spent 4 years with them, until 1951. There were also tensions with my father. He thought I was working for a very left wing subversive newspaper. I had to tell him that my ideas had changed and that I did not want to go the medical school anymore. I wanted to stay on in journalism. In order to become a good journalist, I would like to go the London School of Economics. Then I could get a better education to comment on economic matters as a journalist. So the original idea was, I will go there to study economics, then return, and try to see some sort of role in journalism. So I left 1951 to go to the LSE.

R. Uchôa: How was the educational structure of LSE at that time?

P. Rattansi: You first had to get a BSc in Economics. I found that I had come somewhat prematurely to England. Until then the preliminary qualification you had to have in order to enroll as an undergraduate was that you had a London Preliminary Examination. So I spent the first two years to get this qualification. During those two years, I continued to work as a journalist, because my friend founded a weekly newspaper. I then became a correspondent. During those years I came to know a lot of journalists in Britain, people from the Statesman, for example. I also attended different conferences in London on Kenya. I continued in journalism in that sense. During that time in Kenya, there was the Mau Mau uprising. While I was working in the newspaper I wrote that something dreadful was about to happen in Kenya, because the State was neglecting the serves the people deserved, and that this would escalate towards violence at some point. I had said we were seated in a Vulcan. The prophecy seems to become true and after I left Kenya, my friends at the weekly journal were suppressed by the government. Then I lost my link with journalism and I concentrated more on taking a degree in economics in LSE. I think you asked about the structure of undergraduate education.

R. Uchôa: Yes, I was wondering if you had done all your higher education in England or had done part of it in Kenya.

P. Rattansi: No, there was no way I could have done higher education in Kenya. By that time there was Makerere University in Uganda, but I wanted to go to London to get a degree. The secondary Kenya system was integrated to the Cambridge system. Most of the tests and curriculum applied at the secondary school in Kenya were produced at Cambridge University.

R. Uchôa: You mentioned that you had plans to go back to Kenya after your degree in Economics. But you ended up not coming. Why was that?

P. Rattansi: Something unexpected happened while I was the BSc in Economics. It was a very good degree as preparation for journalism because you study a whole variety of subjects: political history, political thought, history of ideas, constitutional history, economic history etc. These were part of the degree. But then there were some options and the third year you chose to specialize in given topic. I choose sociology. But I could also get at least one extra option and one of the options was a logic and scientific method. And I enrolled for that. The course was taught by none other than Karl Poper. That was quite a turning point in my life to attend Poper's Lectures. He was a very charismatic teacher. It was almost as religious conversion. He could really make you think that the only subject worth doing was logic. After I finished I thought I would like to do a Ph.D. My father would always give support in anything I wanted to do. So he said yes, of course, you can continue. Although he did say at that stage that I could do something useful [laughs], thinking in qualification in Law, that would be even better. I had friends in Kenya that had done exactly that, continued to do their Ph.D., but also focusing on Law. In my Ph.D., the topic I choose was, before I have to give some detail… At that time, there a debate started by the novelist C. P. Snow. He said that there was a great problem the way culture developed in Britain. There were two cultures: the culture of literature and arts and there was the culture of science. CP Snow himself was a novelist but he had scientific training. He said this is the great deficiency in British culture. That these two cultures can not talk to each other. We have to make something to repair this situation. I got interested on that myself. Since I had done sociology as part of my studies. I wanted to empirically explore this divide. I talked to my supervisor that I wanted to explore this debate doing interviews with scientists and then to compare it with people from the literature and arts. And see if there really was this divide. So I developed some kind of questionnaire to study a particular kind group, first the scientists. These scientists came from National Physics Laboratory, from the government institution. I worked with that for about three months. Gradually I became deluded with my own project.

R. Uchôa: Why?

P. Rattansi: Are people really going to tell you what novels they read, what newspaper they read every day? I also became asking who are "The" scientists? There is different kind of scientists: chemists, physicists, mathematicians etc. Then there would be differences among people in the physical laboratory community, so the kind of naïve questions I was raising could really deal with the problem. So while I was thinking the structure of my thesis I began to think when do you have the beginning of something like the scientific profession in England? Studying this I notice that it probably it dates back to the royal society of London in 1665. As I studied the royal society founded in 1665… It had great opposition from the literary people. Again, I was still thinking about the CP Snow thesis, but I was looking at it in a historical way. Then I discovered that there was also political differences here. People who attacked the royal society were predominantly from what was called the Tories and the opposite side was the Whigs. There was different kinds of people who supported the new kind of science. As I studied it more and more I came to be interested in exploring the literary attack on science in XVII century England. I took this chronological limit between 1620 and 1665, and I worked on it. I worked more than I expected.

R. Uchôa: How long did you work on that?

P. Rattansi: Five years… I still had the idea that I would go back to Kenya after finishing this. But having spending five year on it… having become acquainted with various people from the academic community of the LSE… although I was doing the thesis at LSE, I had some sympathetic supervisor at the beginning. But I could not get much help on my topic in history of science. But then you can ask why didn't you go to the Poperians, after all, they were the department of logic and scientific method, but they were really interested in the scientific method. So I felt I could not get that much from them in that kind of thesis. (So I made contact with people outside of this kind of community) I started working on Jonathan Swift, a particular figure to whom I dedicated one or two chapters of my thesis. He comes towards the end of the XVII century. But he launched quit a few attacks on the royal society. For example, in the Gulliver's travel. People think of it as children books, but actually it is very deep satire on human been. He has some chapters on the Academy of Lagado, where people spend their time in things he conders useless pursuits. For example the mathematicians cut up their food in mathematical shapes and things like that. Their experiments was mocked. One of the target of the satire is Boyle experiments of ar pressure. He said that Charles II, who founded the royal society, laughed mightily when he heard they were trying to weigh air. Besides his satire on what was going on the royal society, there is another piece of writing which concentrates more on ideas. This is a debate he calls it "The Battle of the Books". The two people whose books he takes for his satire is Descartes and Paracelsus. Until that time I had heard very little of Paracelsus. And I wondered who Pareselsus were, why he was target of Swift attack. I stated to look on paper on Paracelsus and started

coming across papers of Walter Pagel. He had written a series of articles, it was called, religious motives of the medical biology of the XV century going up to van Helmond. I read those papers and became very interested. One of the people I got in contact, since I could not get much help from LSE, was Joseph Needham at Cambridge, because he had begun to publish his great work on science and civilization in China and had begun to be interested in Chinese painting. I wrote him a letter saying that I was doing this thesis at LSE but could not get that much help. I asked I could come and see him. He wrote a postcard to me saying: "yes, yes, come for lunch at my college." And I went to see him. And talked to him about Paracelsus and he said that the great expert on Paracelsus was Pagel and he said Pagel did not live very far from the place we where. He said: "I will write a proper note to ask if you could see Pagel". So, when I got back to London Pagel invited me home… I would like to talk a little bit about Pagel. He was born in Berlin. He was the son of a famous historian of medicine called Julius Pagel, who was professor at the university of Berlin. Pagel grow up at that kind of atmosphere. He attend the gymnasium system of German education, which means that he studied greek, Latin and Hebrew as well as the sciences. He became specialized in Pathology. Eventually he got the position at Heidelberg university. His particular interest was Paracelsus. He told me he came to study Paracelsus through Helmont, the Flemish Iatrochemist. He came to study Helmont this way: Pagel had contracted tuberculosis. He became quite ill. He asked the doctor who was treating him: if you want to study the history of tuberculosis, who was the first person who mentioned something we would now recognize as tuberculosis? The doctor said: I think it would be van Helmont, who published his work in the middle of XVII century. Pagel said he got from the library some of the works of Van Helmont. At first, he thought it was completely rubbish. It did not make any sense. He thought how a sane person could write a text like this? But gradually he started going in more deeply in Van Helmont's life works… Pagel eventually became professor of history of medicine and pathology at the university. But in 1932, Hitler came in to power in Germany, and started an anti-Semitic drive in Germany. And Pagel said, in 1932 he received notices demising him from his position at the university and saying that if he wanted to continue in Germany he could become a doctor in a Jewish hospital. By that time Pagel was also married to a medical student. She happens to be an Arian German. That seemed to make his life even more complicated, because not only he was a Jewish academic but also he was married to a Arian German. So he decided to leave Germany. He first went to Paris to the Institut Pasteur. I suppose he said he was working most of the time in tuberculosis and trying to find some kind of cure, which he told me the great disappointment of his life was that he was never able to find the cure for it. I should add one more detail. While in Paris he started corresponding with Nedehan at Cambridge. And Needham said: you should come over to England. Perhaps we can find a place for you here in England. So, after about two years, he left Paris and went to Cambridge to work with Needham. The collaboration between Pagel and Nedham was the beginnings of the teaching in History of science at Cambridge.

They formed a committee and invited people to give talks on history of science. The talks took a form of a volume that was published afterwards. But Needham was quite unable to get any support for the establishment of the history of science as a discipline at Cambridge. Nor was he able to get any department place for Pagel. Pagel said he stayed around for a couple of years and decided he would not get any place at Cambridge. But he was a Pathologist, so he applied to a hospital in a town near Cambridge. So his first appointment in England was in a small hospital. Later on he got a appointment as a clinician at Central Middlesex Hospital in London. All of Pagel's work on history of medicine was jointly with his job on Pathology at the hospital. He never got any academic position in England. So, this is just to give some background on how Pagel got interested in Paracelsus studies. I had the honor of collaborating with Pagel on two papers. One paper was called "Vesalius and Paracelsus". The other paper a collaborated with Pagel was about a man we studied in great detail called Johannes Marcus Marci who taught in Bohemia and Prague in the xvii century. I would like to mention the main point of this articles: people had speculated whether Johannes Marcus Marci ever meet William Harvey. Pagel, besides been authority on Paracelsus and von Helmont, also studied Harvey in great detail. Harvey as regarded, until recently, as a kind of mechanistic physiologist. I mean, if one would take a course in History of science you would be taught that the great trend in XVII century was the new mechanism corpuscular philosophy and as representative of this movement would be Robert Boyle, later on Newton, but Harvey was taught as a mechanistic physiologist. Why? Because he talks of the heart as a pump, so a mechanical device. He talks of the veins as kind of hydraulic system, that is a machine. So he goes from a Galenic model of life to a mechanistic model of life. Also, another factor, Harvey employs a quantitative proof for the circulation of the blood. According to Galen, blood is made in the liver and transported through the veins, then goes to the lungs and pass through the left side of the heart, goes in to the lungs and travels through the body. So Pagel showed that Harvey was a Aristotelian, so when he talks about pump, it is not a mechanistic explanation he is doing, but Aristotelian. Harvey is deeply influenced by the Aristotelian methods, concepts and ways of argument. So, Pagel's originality was shown in his works on Paracelsus and Helmont, and part of the reason was that he knew enough the Aristotle's works to show the connections between them. In this, sense, there are various kind of innovation in the history of science that Pagel is involved.

R. Uchôa: So, Pagel and Needham seems to be key figures in this period of change in the History of science.

P. Rattansi: Yes, let me add a third figure. Although I said I did my thesis at LSE, I did have some difficulties. For example, the normal way in which you pursue your studies in LSE and most of the universities in England. You would first be registered for a MPhil. Then after you had produced sufficient chapters, your supervisor would apply to the higher degree committee, to promote you to a Ph.D.

So, this is a higher degree with people from all departments. When my advisor applied to my registration to promote my MPhil to a Ph.D., there was a lot of opposition at the committee, particularly from an economic historian whom I admired greatly, who said, why was this person ever allowed to choose a topic which seems to be not related to his qualifications. It is a thesis on the attack on science. This person does not come from the sciences. Besides, he is not a historian, and he has no degree in literature either. Nowadays, projecting myself back in his position and somebody was doing a thesis of this sort, I am not sure I would accept this thesis either. So it was decided that I should see someone who knew about XVII century history. And the person selected was a famous figure at that time in English establishment: Hugh Trevor-Roper. He was known for his works on Hitler's last days. But his specialty was XVII century England. He worked on the civil war period, what was the so called English revolution, sometimes referred as Puritan revolution. It [00:28:00] was decided that it should be fixed an appointment for Hugh Trevor-Roper to see me. So I traveled to Oxford. I had taken the precaution of wiring him to mention what I was working on, what kind of conclusion I was I had come, what further works I felt need to be done. So I was showed his rooms, I waited for him. He came in. I confess I was a bit nervous. His reputation was that he was a very fiercely person when he criticizes a person's work. But I find him very friendly. He said he was very interested in my summary, that I was working on a very interesting subject and he would give any help he could. So I kept meeting him until his death some years ago. These three people had some role in my academic life. That kind of people who opens the door for you. When I finished with the thesis, I still thought I would go back to Kenya, but I said to various people that I would like to get a two or three years fellowship so that I could put some of my thesis into print. This was the point. Needham tried to get a position for me at Cambridge but there was no position at the moment. I also tried with Pagel, I could not get any position either. It seemed I had to go back to Kenya because there was anything available. My supervisor did try for me, there was this sociologist of science Bernard Barber, who was a *projeté* of Robert Merton at Columbia University. This came in my studies as well. When I was studying sociology I came across Merton works on science, society, and religion in XVII century England. Perhaps one should say that it is a very famous work on the sociology of religion. Max Weber, who had written on the sociology of religion, wrote a famous work making a connection between puritanism and the beginnings of capitalism. A famous thesis in an endless debate on the relationship between Puritan religion and early capitalism. Now Merton wrote his own thesis on puritanism and science in XVII century England. He was using categories drawn from Max Weber. So anybody who works on sociology was acquainted with this thesis. My supervisor did write to Barber and said there is somebody here working on the same thing but come in different conclusions. He said yes, we can set up a fellowship for him, but it can take a year or so. There is nothing immediately available. Dr. Nedeham also wrote to Barber to indicate me. Needham then wrote back to me and said, "I am sorry I am not able to get anything

for you, but I have a friend, Dr. Ravetz, who is now head of the section of science at the philosophy department at Leeds university in Yorkshire. Write to him. I have already talked to him about you". So, I wrote to Dr. Jerry Ravetz, he replied and said come and give us a talk on the theme from your thesis. Come and talk about Swift and science. I went there and gave a talk to students and postgraduate students. He [00:34:00] said I am delighted with your talk, I think we could set up a research fellowship for you here. It will take time, of course, but in about 3 months we can offer you. I took this time to get back in Kenya and see my mother. Then, I came back to Leeds. (…) They were expanding the department at that time. In fact, he asked me if I wanted a lectureship at the department. Of course, I had to go through the whole interview process. But I said no, I am going back to Kenya after three years. I am not interested. So they recruited new people. One of the new people was Dr. Maurice Crosland. He had done a chemistry degree. Then he had done a diploma in History and Philosophy of Science at UCL. His thesis was the history of the language of chemistry. It was an interesting thesis because he showed how language changes. So he was appointed for one of the lectureships. The second person appointed to another lectureship was J. McGuire or Ted McGuire. He was a Canadian, who had taken his degrees at Ontario, in physics and philosophy. A years later we got someone else on the team, Charles Webster, the famous historian of the XVII century, the hartlib group and so on. He had studied botany at the University College London. Then he did a diploma in History and Philosophy of science at UCL as well the Master and Ph.D. So it was a very lively team. Ravitz was a very good leader of this team. My first paper was on Paracelsus and the Puritan revolution. The second paper was a study on Helmontianism in London. This was my second paper.

R. Uchôa: Let's us take a moment here and we came back to explore in more depth this stage of your life.

Interview Part II

R. Uchôa: Professor Rattansi, you mentioned your contact with some of the major figures of the historiography of the history of science: Pagel, Needham, etc. What was at that time the main conference that you gathered to discuss history of science and what was the main topics. I mean, the "hot topics" in the history of science that you were discussing.

P. Rattansi: That is a very interesting question because I never attended any course in the history and philosophy of science. At LSE I attended the lectures of Popper. Just to give an example of how he started his lectures. He started it by asking us: am I right to think that science is the method of observation and induction from observation? In other words, you study the phenomena, then you make notes,

then you make some conclusions and you discover something if you are lucky, a so on. After that one would answer, yes, of course. He said, all right, suppose I took you to a high-energy physics laboratory and assuming you are not a physicist. I put you in front of this dials. Of course, by that, you could have the digital and the analogical screen. So, you would be reading the needle, the effects of the needle. And he said, start observing. What would you be able to do? Nothing. Unless you know the theory behind it. The point he was trying to make was that everything starts with theories, then you create a kind of horizon of expectations, then you make the observations. So, observations operated in the context of a broader theory. I remember, Thomas Kuhn told me, Poper was a philosopher of science and not too inclined towards historical illustrations. So I had been exposed to these ideas of Poper but never had taken a course in history of science. In a way, I learned by teaching it. Ravetz told me, it was also part of my duties to take some tutorials. In other words, Ravetz used to give a general course called history of science, from Babylonians up to the end of XVII century. Then there were specialized courses that other staff could teach: chemistry in XVIII century, history of technology etc. But, although he taught the main course, all of us was suppose to take groups of students and tutor them. I mean, if you want to know something really well, the best way to do it is trying to teach it. So, this was the first time I was exposed to teach history of science. The course was taught through texts. So you could take texts from Aristotle, then you had to explain what it means. Students used to say that this method of teaching was very well come to them, I mean, you first read the text and you don't get any sense of it. Then, after you go to some of the lectures and tutorials you get able to read and understand the text. So, a great deal of my knowledge on history of science came by teaching this course. The main approach was take the course from Babilonia, Greek culture. I did not spend some much time on China, Islan or India. But then, we would go through medieval science in Europe, giving attention to some Islamic figures. Then the high point was the scientific revolution in the XVII century. The approach was also biographically, like taking a particular person and talking about him or talking more broadly about historical and social contexts of this given person. One could ask, what was the textbooks we used? This is a difficult question because textbooks get outdated quickly, either because of the material there or because of the approach. I think Ravetz had great doubts of what would be the best textbooks. By the time he chose a book published by the Oxford University press, by Charles Singer. Singer taught at the University College London. I don't think he was ever member of the department. I think he studied history of biology and medicine, I am not quite sure. He had written a lot of short histories. So, Ravetz peaked the history of science by Singer which went from the Greeks right until the XVIII century, I think. So, it was used as the basic text book.

R. Uchôa: A key question is how did you start your research on Newton? I mean, how did you come from a Ph.D. in economics to the works of Isaac Newton?

P. Rattansi: I started looking at the history of Paracelsus and in XVII century England. My thesis had been on the debates between science and literature in XVII century England. So, I concentrated mostly on this period and looked at the history of the Paracelsians in England and their role within the social and economic setting, or how the social and economic context was a necessary condition for the emergence of the Paracelsians. In that, I was inspired by the second part of degree which was in sociology. So I took a sociological angle. It is important to notice that sociology by that time was not concentrated on nowadays important figures like Foucault. It was really a Mertonian kind of approach. Similarly, when I approached the topic on the Helmontians, I did not say that much about the content of the Helmontian's works. It was more on why they became important in the time of the great plague and so on. Up till now, my interest was in this kind of work. Just to tell you a history about Pagel. When I meet him at the first time, I asked him what historical period he used to work with. He said he worked till middle XVII century, that after that science get to rational. So, we all assumed the 1660 marks an important transition in England's history. People still use the term early modern, as if after that the curtain opened to the modern. So I had no studied historical period after that. How did it happen? [00:13:00] As I remember, just to give some details. I came to Leeds, with a research fellowship given by Leverhulme trust. The Leverhulme are the people who produce the Sunlight soap, they also give money to charity. Part of charity money was directed toward the history of science scholarship in various universities. The scholarship lasted 4 years. After the four years, I realized I would not go back to Kenya. After two years, they asked me if I would stay in England or go back to Kenya. I said I would stay, and they said, in that case, there is a lectureship for you. They interviewed me and gave me the position. By that time, they also had two other positions in the department that came to be occupied by Maurice Crossland, focused on the history of chemistry and the other by Ted McGuire, who was a philosopher of science… Meanwhile, there was an academic appointment at Utrecht University. They had a chair in the history of science, which was occupied by a very distinguished professor of history of science called Hooykaas. You might have heard of him. He wrote on the history of geology, science, and Calvinism and so on. Hoykas was retiring, so they were looking for somebody else to occupy the chair. They approached Jerry Ravetz to take up the chair. He was married to an English lady and had a baby son at the time. So, he said he was unsure of taking up the invitation. He ended up trying it for 6 months, to see how things go. It went ok and he decided to take me with him to Holland. But, McGuire and Crossland argued that they had come to Leeds just because of Ravetz and me, and as it was as recently contracted group they said, at least leave Piyo here. And Ravetz agreed. Meanwhile I used to have lunch with McGuire at the cafeteria. Most of the time we used to talk about academic gossips, and I said why don't we talk about something more interesting than academic gossip? [laughs] What are you doing research on this days? He said: I am studying what use to be called the two great unifiers of British science: Newton and Maxel. I started with Newton and there are some things about

him I am trying to make sense of. So he showed me some of Newton's papers. It is difficult to trust in your own memory but leaving London this time I came across some letters by Ravetz. I wrote him letters saying that I was talking to Ted McGuire. He showed some papers and I described the papers there. And then said, I suggested to him he was studying things linked to things I was studying myself. He mentioned he thought after XV and XVI century this things went away. Well, I have the letters and can give you and then you can check it. [firework noise]. Is it demonstrations or?

R. Uchôa: Yes, it is the impeachment movement against the president.

P. Rattansi: So, we are bringing the contemporary scene to the recording. Even as we talk... [laughs]. So, he digs up some more letter from Newton. For example, one that Newton writes to Christiaan Huygens. I mean, Newton had not tried directly, he asked Fatio de Duillier who Newton seems to adopt as a kind of son or something. I mean, people that study psychoanalysis says that Newton had homosexual attraction… we don't know, how can you tell? Anyway, Newton was very selective about this man. Fatio de Duillier was a mathematician himself. At one time Newton said him you will publish the second edition of Principia and puts him in charge… Newton does not write himself to Huygens, he asks Fatio de Duillier to write this letter, which is very interesting, because now looking back there were particular reasons why did Newton was concerned with Huygens? He is the same person who is writing to Leibnitz and saying the gravity is an absurd. A couple of reasons you can think of why Newton wants to contact Huygens… When Newton makes his first appearance on the scientific stage with his letter on light and colors, he gets a very hostile reception by Hooke and other people and Christian Huygens, also writes criticizing him. But Newton greatly admired a work by Huygens called Horologium oscillatorium which was a very good example of using mathematics to deal with philosophical problems. It is in this exchange of letters that we find Fatio de Duillier saying to Huygens that Newton has discovered all the great truths found in the Principia were known to the ancients. So Pitagoras knew the laws of gravitation and so on and Huygens writes back that Fatio has been too nice to the ancients. They might have known something of the now known Copernican system, but to think that they knew the mechanics of it, that is the achievement of Newton. He gives an example. Newton great example is that he is providing an explanation in terms of mechanics not for planets in circular orbit, that is big enough problem, but for an elliptical orbit. Mathematically, using an elliptical orbit is much more complicated. The point I am trying to make is that Huygens says the achievement of the Principia is a unique achievement of Newton. Therefore, it is his personal, individual great achievement. So, to think that the Greeks have done, it is nonsense… McGuire used to go to Cambridge and look at the university Library collection and found a group of documents dealing with that phase when Newton was collecting material for his annotations and they included the very important

thing on the Pythagoras discovery of the laws of gravitation. So, that thing was McGuire discover, then we decided, let's write it up. We wrote letters for Ravetz talking about this finds. When he came back he was already working on the writing of the paper. It seems that McGuire had met the editor of the Notes and Record of the Royal Society in London, Sir Harold Hartley. McGuire mentioned he and I were writing a paper on Newton, giving a new kind of light in Newton. And Harold said why don't you send it to the Notes and Records of Royal Society? Send it to me. And if the paper was good enough to be published… we can discuss later which part of the paper was produced by whom. His main contribution was on Newton, I did not know that much about Newton at that time. So he constructed the first part. He gives an outline of what happens in the Principia. Principia is a very difficult book to read, but McGuire studied it and so he was able to give an outline of the propositions of the Principia, then fit this newly discovered material and decide where it part goes where, that kind of thing. I got the second part. I had, of course, studied the pristine idea, *Prisca theologia* idea, because while I was writing my thesis I came across of some part of that tradition. I studied the work of people from Warburg Institute. I did not know them personally, but the work of Yates and the works of D.P. Walker, who wrote a great deal in *Prisca theologia*. Their papers were wonderful. So, I was trying to link it up with the Newton material. Ravetz also made some contributions. Perhaps many of the excellent phrases in the paper are from Ravetz. The last part, for example. I think another thing that Ravetz really gave us confidence was when he said "if you are merely going to say that Newton is bringing back occult qualities, I mean if this is a mere apologetics of Newton it won't interest the historians that much, because they would say: "all right, Newton is adopting any means to save himself, so what?". But if you could make the point that we can't rule out the possibility that the problems that Newton had to solve in order to write the Principia [00:34:00] (the idea action of God, conceptions of space etc).. we can't rule out the possibility that believing in that kind of stuff influenced his science. In this way, we are not talking about apologetics, we are talking about the constitution of his scientific theories… So, Ravetz said, don't make this as just a curious thing Newton made up to save his system." Of course, there is also the question of what was his audience, that he though this kind of defense would save his system. If [00:35:00] you think the fact that Gregory comes to interview him, he does not seem to be surprised by Newton's ideas and he does not think Newton has gone mad. Also, if one thinks in his audience, the idea of *Prisca Theologia* was widely accepted. Besides, in XVII century people took for granted that your task is to write natural philosophy. The idea is that you are collecting the wisdom of God's creation. This is one particular idea that strikes me while I was working on the paper. At that time I meet a young lady, fell in love and married her. Meanwhile, Allen Debus invited me to Chicago for three months as an Associate Professor. So I told the person I was engaged: I have to go for three months to Chicago, when I came back we can marry. She said, what make you think I will be waiting for you. [laughs] I hope she said it as a joke. I got a bit scared and then I said, well, in that case we

better get married before I leave. So, we went to Chicago, we did not have a really romantic honeymoon because I was working on the paper. Right after McGuire arrived in Chicago as well to urge me and say please go on with your part of the paper so that we can publish it quickly. So, I was working on that most of the time. When I returned to Leeds, the paper was almost ready. Towards [00:39:00] the end of it Ravetz took it to revise it. This is how the paper came in to be. Now, England been England, when you get any kind of fame… although the paper, in a sense, had a very mixed reception, I mean, a lot of people said it was terrible to show that Newton was involved in all these stupid ideas. Rupert Hall, who was, I think, the chair of the British society for the history of science, in his presidential address he talked about this idea he had never thought that Newton was involved in this kind of ideas. When I was in Chicago, one thing that came to me was that, in a way, one thing that runs through Newton's involvement with the study of prophetic books of the bible and his studies of alchemy and his pristine theology, in other words, the assumption that there was truth that was given to us in ancients times and that it was corrupted and that you can only recover the true meaning of the hermetic when you are using a true inductive experiment and observation. That is, keeping the idea that after the fall: "By the sweat of your brow you will eat your bread..". It does not comes easily, just by looking for the textual meaning of it. So, the idea is that only after hard work with nature you can go to the text and understand it. I used to tell to the students, it is retrospectively that it is recovered by hard work (…) (…) I think McGuire, on the whole, was not very pleased about all this other kind of speculation I was putting in the paper. In the end, he said we should not publish it at all. He said, the history of science community will laugh if we publish something like this. I said, look, If you don't want to be part of this ok, I mean, your contribution is the first few parts, most of the rest I have written. Then I said, ok, I will publish it in my name. I don't mind people laughing at my work.

P. Rattasi: Eventually the paper comes out. Two incidents, off the record… While I was at Leeds I said "Why don't we invite D.P. Walker to talk to us?" So, we invited him to come to Leeds to give a talk. He said he was talking to Yates and that she was interested in our work. So he invited us to the Warburg Institute, me Ted and Ravetz to talk about the paper. There was a very interesting assembly of people there. The director was Gombrich, the great historian of art. I presented our paper. Walter Pagel came to the meeting. He rarely attended seminars, because he had tuberculosis at a young age and that affected his physical condition for the rest of his life. He went to another famous seminar at UCL where Koyré was giving a talk. Afterwards, in conversation, Pagel asked Koyré what he thought of Newton's alchemy. Koyré said he does not deal with this side of Newton. So, Pagel also comes to our seminar. Mss Yates also gave a paper. Incidentally, the publication of this paper immediately made us a target for people from the outside invite us to give talks. I had been in contact quite early with Bob Young from Cambridge. I think I told the history, that he had just been appointed to assistant lecturer in the history of

Biology at Cambridge. He is from Texas. He was doing a thesis at King's College. He said he asked Needham what he should read in the history of biology. He said there is the thesis of Ratansi which I have a copy here. Needham asked Bob: why don't you read it, there is interesting stuff there. He wrote to me an astonishing letter saying that it changed his whole approach to the history of science, the thesis relates science and society. He asked me why I don't publish it. I wrote back saying I know so many things now then I knew when I wrote the thesis, that should change a lot of it. I think I told you the story, then he invited me and Ravetz to a Seminar at Cambridge, in the history of science society meeting. In a letter, he told me I don't want to take you out of Leeds, but there are many people at the Department asking why don't you invite Rattansi to join us? On the other hand, things at Leeds were a bit tense between me, Ted Mguary and Ravetz. One fundamental reason was that I got more from the academic community for our paper, among other things. So the next time Bob approached me I said yes, I will take the position. So that was how I went to Cambridge. I have to say I never found the kind of fellowship I found in Leeds. Of course, there was also very nice people at Cambridge. There was Bob Yong, there was a man called Skinner, that worked in Political Theory and history of ideas; John Done that worked on John Locke.

R. Uchôa: Still on the reception of your work, if we look to the USA, on Thomas Kuhn, my question is how he received your paper and how was your relationship with him? I know you spent some time working and talking to him. Because it seems that you have different ways of looking at the so-called scientific revolution.

P. Rattansi: I read Thomas Kuhn when I started teaching History of Science. In the astronomical revolution of the section, I took one of his books as compulsory readings for the undergraduates, because he talks about Ptolemy, Astronomy and so forth. Obviously, one could see that it was one of the topics that made Kuhn see the paradigm shift. He used the astronomical revolution... So I knew about him, and when the Structure of Scientific Revolution came I read it. But I was always... anyway. While I was at Cambridge, I meet some of his students from Princeton. One of them was called Brown, who was very interested in History of Medicine, he had a biological background. We became friends while he was there. Then I got I letter from post by Thomas Kuhn, who I had never met, in which he said that: "we never met but I saw your works and your paper, and one of my students at Princeton attended some of your lectures, so I have a proposal for you. Would you be able to come to Princeton for one year? I had talked to the head of the Institute for Advanced Studies, and we are able to offer you a membership for one year while you are here. You will be staying at the Institute for the whole of your year, with your wife and son". Sometime later, I found out that it was very rare that they invite people for more than three months. For the first three months you are allowed to carry out your own research but at some point, I had to deliver lectures. In the rest of

the year, I would be involved in seminars, lectures and things like that. In Cambridge, I used to give a course Science and Society in XVII century England. Then I said I would use the same kind of material for my course at Cambridge. And it was very successful, there was very good students, responsive and hard working. So, this was about the teaching and so on… I got the feeling the Kuhn was unsure, I mean, he had just published the Structure, which was a great success. It had a great impact on the history of science community, but he did not know which way he should go now. Whether he should go to this sociological approach or should to the linguist side. I think he wanted me to help him to decide which side to go, [00:17:00] because the only work of mine that he praised was the Helmontian thing. Non, the Helmontian thing I did not look into his doctrines very much, when I look back at that paper to hear one of these deficiencies is: I haven't spoken enough about Helmontian doctrines and so on. But at any way, at least I have tried to show how particular kind of bodies of work may be utilized by interest use for their own causes and things like that. So I am trying to show how ideas in… I mean coming from the background in the newspaper and seeing how… I mean I sometimes say […] I mean because I have this exposure to politics, and so there are a lot of people who come, they are very good students in school, first class, then they go to university, they get a first class, and then they become academic and then go to study history and things like. I get the feeling […] that because I had to […] and see how ideas, in colonial society, but how ideas relate, so I always had this feeling that you had to look at ideas at it particular context […] so that's what I think helped me, that kind of background. I remember […] telling me that for them the wartime experience and things like that were very important, that they had given them […] political, social circumstances. So maybe I was lucky in the fact that "wasting" time doing journalism for years. That's why I finished much later than other people in my university training and so on. But anyway, back to Kuhn, I'm not sure whether he got very much out of… One of his—of the people that was running for chair at UCL was a man called […] Sabra, who came to my lecture at Warburg because he was… at that time Warburg Institute was had on historian of science… he was an Egyptian from Alexandria. His special subject was optics [...]. He told me when went to do his PhD with Poper, he said "I am sorry, you are not trained in the sciences, go and get a degree in science". Eventually he wrote a book on the history of optics from Kepler to Newton, I think, which was his PhD. Later he came to focus on Islamic science […] and got a chair at Harvard. Why did I mention him? Oh, yes, he came to Princeton too when I was there, he was also with Thomas Kuhn. And of course he's such a good human being [laughter]. Another thing I found about Thomas Kuhn was that he couldn't take compliments. I mean, when I met him for the first time I said to him, "Oh, "The structures" has been adopted by the anthropologists at Cambridge." And he said, "You see what happens when a book gets into the wrong hands [laughter]?" So defensive about his work. And anyway, I praised his book very strongly […]. I said, "Because I usually do recommend the book to my students." He said, "Yes, but it's all secondary sources [laughter]. Not primary

sources." I said, "It's true, but it's what you made of it." And he never seemed to enjoy his success. He was kind of troubled by it all […]. Should I go that way or that way? Where do I go now? It seemed to be for one or two reasons. I met his family. My wife was working at the university in an administrative position, and his son was very bright, he got his PhD from Harvard. But since then the writes about the psychology of his father, and things like that. This I got from […], when I met him at a lecture recently, at the Royal Society, Hasok Chang talk. And I met […]. […] had just finished a thesis on Kuhn, and he showed it to me. I said, "This is a very bright student." I wish I had students like this. But I asked a […] who I was sitting next to. I didn't know him very well. And he agreed. He said he couldn't enjoy his success. He was always anxious about it. Anyway, why he left Princeton and went to MIT? It seemed his marriage broke up. The boy incidentally, yes, he was very good. The youngest PhD at Harvard. And I guess […]. He started writing about psychiatry and things like that. The psychology of his father. And I said, "Is that published?" He said, "No." He did it as a lecture somewhere. Some […]. But I mean, in a sense I didn't get much out of him. [I felt like he couldn't because… maybe my left-wing emotions during my days in journalism has made me more accepting to the side that wants to see discontinuities, revolutions, happening all the time. Discontinuities and things. And yet I found myself, on account of the… particularly because of the Newton thing, it looked like discontinuities. I mean, I don't know how… I didn't talk about this to Kuhn, so I don't know how he would have answered this. I mean, if I accept the Kuhnian kind of idea of revolutions and so on, then would I myself make use of the generational […] kind of idea. So that if you do the generational thing, Boyle and Descartes and all these other people who want revolution, Newton is of the second generation, born in the day on which Galileo dies. And he's of that generation. So I would have thought the *gestaltic* shift […] in terms of the great mechanical philosophy kind of thing. […] and therefore they said […] he was forced to conclude that […] [and he was never very happy with it?]. But [I don't see any of that?]. I see continuity in all sorts of other things. Sorry, [continuation of that, discontinuation?]. So […] easy for me to work with [laughter] […].

R. Uchôa: The other place, the other American university, that you had a chance to work that you just mentioned was Chicago with Allen Debus. Could you talk a bit how was your work with him in terms of ideas and academic work?

P. Rattansi: I think in one sense, […] people, I think he was kind of lone worker. He didn't like to share lectureship […] so there wasn't, in that sense, collaboration. But he was […] in the sense that […]. And I was given research fellow at Leeds to be invited to take an associate professorship at Chicago. I think he is a… I should also mention the fact that I think it was he who brought Pagel to […], because Pagel kind of identity was not that of historian of science. He saw himself as a kind of… maybe coming from a Germanic background, he didn't really put himself in a particular class, but until Allen Debus stat bringing prominence to the themes that

Pagel worked on, people didn't see the Paracelsian thing as part of the scientific revolution circle. So I think Allen had a great influence in making us realize the greatness of Walter Pagel but also the fact we had to include the […] people used to… I mean look at Pagel papers. These were published in […] and there were all called religious motives in the medical biology […], so something on the medical biology. Perhaps he also had some influence in Pagel […] difference it made but it made difference too in the sense the we, people working in the history of science, started looking […].

R. Uchôa: Yeah, it's I think… final question on this. You are here in Cesima, Brazil working with Professor Ana and Márcia. Could you talk a little bit about your work with them and how did you come to collaborate in this project that you are working now? And if you could, how does that relate to your work from the 1960s?

P. Rattansi: Yes. My encounter with Ana and José and Márcia was quite sudden. At that time, I was, of course, head of department and […] some time how I came to be appointed at UCL from… because […] going up from my research fellowship at King's College, Cambridge. But we hadn't […] transitioned to UCL. That is something we should perhaps discuss at some future… but talking about the Cesima connection I was, at that time, at UCL. And I received a phone call or a letter or something announcing that we are here in London at this moment working with the Royal Society manuscripts, so we thought we will get in touch with you. I would like to present to you a copy of a thesis I did on Arabic alchemy, and it would be a great pleasure for us to meet you. At that time, my eldest brother had passed away […]. And he […] the hospital […]. He had died and the funeral was […]. So I wrote back saying that, "Look, I'm very busy at the moment. My brother has passed away and […] funeral tomorrow. I am busy." And I said, "But […] let's meet and you say that you are working at the Royal Society. Now, I am a member of the Oxford and Cambridge Club, which is […] 5 minutes to 10 minutes walk from Royal Society. So what I suggest is that you come and meet with me at the club and then we can talk." Okay. So I go to the club and I sit there. I completely forget [laughter]. These English clubs are very peculiar places. And I'm a member of this club but I never joined that club until I retired. It's situated in a place not near the university library. Why should I pay £1,000 a year […] to this place unless I've retired? But anyway, just so that I can go [and have?] a cup of tea there once. But I joined it after I retired because it's a nice place to be and to invite people for dinner. When you are in London the next time, you tell me… But they have a dress code. If you go into the club, you must be dressed in a jacket and tie. And women must be modestly attired and so on. I was sitting there. And the door opened and I see three people coming in. They are all dressed in Levi's [laughter]. […] the […] in charge said, "Sir, I can't let them in. Didn't you tell them they have to wear special the dress?" I said, "[…] sorry. I forgot [laughter]." So I told them, "I'm deeply sorry but we can't stay here." But in summary, let's go pubs around or something. It was about half past 5:00 […] they

wanted to complete their day's work there. So the drinking places are full of business people working for the business and so on coming out of the work and go to the pub to drink a glass of beer. So we couldn't even find a place to sit down. So, José saw a place. Then we talked and they then said that… so she gave me a copy of her book on Arabic alchemy. I think they were leaving quite soon after that. They said, "Have you ever thought of coming to Brazil?" I said, "No [laughter]." They said, "Well, we would like some time for you to come to Brazil and we should […]." They also reminded me something I'd forgotten. They said that they had applied to UCL. They say that what made them come into history of science was reading *Pipes of Pan*. Now, a couple of people have told me this, it changed their life. There was this lady called Lisa Jardine. I don't know if you can recognize her name.

R. Uchôa: Yes.

P. Rattansi: Now, this is, again, another story. Sometimes because they have […] Oxford and Cambridge Society, it's not […] I think, unfairly, it's […]. If you belong to it, you get preference when you apply for a job […] you're from Oxford and Cambridge. Even now, if I'm in an English circle and I mention the fact that I was at UCL, okay, before I say I was a fellow of King's College Cambridge, they will pay attention to [laughter] completely nonsense. Why did I mention this? So what was I talking about?

R. Uchôa: About the Cesima connection.

P. Rattansi: Oh, yes. Yeah. But how does it relate to the flying to… anyway. This is the […]. Oh, I see. I was talking about changing my life. I was [...] Lisa Jardine […] [laughter]. I went to a meeting. They called me to a meeting of the […] near my […] Oxford and Cambridge Society. [...] lecture on the scientific revolution by Lisa Jardine, we were wondering, would you be able to come? So I went with my wife to the talk […] by Lisa Jardine whom I've met from time to time. Her husband… former husband, got a divorce. Jardine is well known to… sorry, to the young lady who was visiting here…

R. Uchôa: Jennifer Rampling

P. Rattansi: Yeah. She knows Jardine. Jardine was junior Fellow… Anyway, she was married to […] okay. So, everybody, sit down to listen to the lecture […] Jardine comes in, sits down, stands, and then she looks at me and she said, "That man should not be here." So I said what is going on here? "What have you done [laughter]?" Why should […] be here? Then she continued. She said, "Because it's because of him, because I went to his lectures at Cambridge that I came into the history of science." So it's embarrassing for me to have him attending a lecture where I'm […] [laughter] because mine is […] with […]. Oh, she was your student. Because

I always tell the truth, so I said, "[…] a student but it seems she […] [laughter]." So […], of course, to inflate myself [laughter]. But even talking about Ana and Marcia, they said, "We came into this subject because we read your paper. And we said history of science can be as exciting [laughter] as this? We'd like to be part of it." And they said to me, "[…] apply to […] and we would like to [come and?] do our PhD there." They said, "You replied and said, 'well, I'm very sorry. I've just taken [over?] the department […] I can't take any PhD students at the moment.' You sound strange. You should be looking for business." And so we've been to London… sorry, to McGill University in Canada and did the thesis with Mario Bunge. Anyway, this was the story I heard then. And then how did we come to collaborate? I forget how. But I remember, in the end, we started working together and I got the […] in the department. But then they could use the libraries in London freely […]. And the first paper we wrote… sorry, I didn't put it together. I better ask them whether they remember. I can't remember much about this […].

R. Uchôa: It's okay. Oh, yeah, I think that's it. I think we can start finishing up this part so that [it can take some?] […]. And thank you very much, Professor. Thanks for giving such a fantastic account of your life.

P. Rattansi: Thank you.