

Interview with Piyo Rattansi¹

Interview² – Part IV (13/09/2016)³

R. Uchôa:⁴ Was there an event or something specific that made you change your political view?

P. Rattansi: I suppose there was not a tremendous disillusion or anything like that, but I suppose studying with Karl Popper had some influence because he was, of course, the man who was supposed to have shown that Marxism is scientifically wrong, I don't know if one can prove that [laughter]. I think his point, really, was he selected Marxism, Freudian psychoanalysis as theories which you can [...] refute, and psychoanalysis because if you say that... I remember when I wrote a review of... there's a man called Frank Manuel. He wrote *A Portrait of Isaac Newton*, which was a psychoanalytical study of Newton. And I saw that he... I think we've probably discussed this before, but he can... Newton got very annoyed if people tried to be very fatherly to him. If people behaved as if I'm your father [...], he'd get angry with him. Why? He says because, you see, Newton's father died a couple of months before his birth. So by dying, he had kind of deserted him. So he's annoyed with his father. Okay. On the other hand, he has this image of God, the father, and the idea of attraction. He says maybe attraction towards either his mother or attraction towards the dead father. So behind this attraction idea is such [...]. So, you see, you are explaining... and when I used to talk to Bob Young, he would say, "But how can you explain? There's no way you can check if I say he was attracted to his father and, therefore, the idea of attraction. On the other hand, he hated his father because he had deserted him." And you can [...] everything [laughter]. He said [...]. But Popper's point was, in that case, you can put forward any hypothesis. You should be

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² The interview took place at Rattansi's apartment in São Paulo while working at Center Simão Mathias for Studies in the History of Science (CESIMA), in 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).

³ The interview was designed, recorded and transcribed by Raphael Uchôa, Ph.D. candidate at Postgraduate Programme in History of Science, Pontifical Catholic University of São Paulo (PUC-SP) and supervised by Professors Ana Maria Alfonso-Goldfarb and Márcia Helena Mendes Ferraz, both researchers at PUC-SP and Honorary Research Fellows at UCL.

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

able to, at least in theory, provide a kind of... find a way of refuting this theory. But if you say [...] one side, this, and the other side, that, there's no way you can refute it. So he imputed that kind of form, both to Marxism and to Freudian psychoanalysis.

R. Uchôa: So, for him, it was not a proper scientific theory...

P. Rattansi: It is not a theory that you can in principle refute. I forgot how he does this for Marxism...

R. Uchôa: And he, himself, wrote lots of things on politics, right?

P. Rattansi: Yeah. [crosstalk] in Vienna and was close to the Vienna Circle at one time and then he rejected their ideas. And he was also in to a left wing phase some time and then came out of it. I think he tells a lot of this in his autobiography. [silence]

R. Uchôa: Yeah. So this conversation, it's more a attempt to clarify some parts of the letters that you exchange it with..

P. Rattansi: Ravetz?

R. Uchôa: Yeah. With Ravetz. Jerry Ravetz, right?

P. Rattansi: Yeah.

R. Uchôa: So, who was he? How did you come in contact with him, and why did you start this exchange of letters with him?

P. Rattansi: Yeah. When I think back I see that Joseph Needham was quite important in my life. I met him... as I said, I became interested in Chinese painting. Because I went to an exhibition of Chinese painting, then I became very interested, and then to Chinese civilization, and just at that time, Needham's work was beginning to appear. The first volume had already appeared. This question of why did the modern science take off in a place like China? Why not [...] in the West? That seemed an interesting to ask. So I read his first volume, and then I just dropped him a letter to say that I was working on this thesis, and I outlined what the conclusions I was coming to, and I couldn't find much help in London from people there. So can I come and see him sometime, and he said, "Yes, of course", and I went to see him and we became very good friends. And when I finished my thesis, he asked for a copy to be made specially for him. He paid for the copy to be made from the thesis, and he mentions it in his second volume of his great work. And I contacted him, and I said that I'd finished my thesis now, and I was going back to Kenya but I was looking for a place where I could get some kind of research fellowship for a year or two. So then I'd put down my thesis for publication and then go back to Kenya. And he said to

me that, "I'll fix an interview for you with the great man here at Cambridge in philosophy and history. It was his science head of the department there. And that was again [...], and so I went to [...] and talked to him and so on, but it was obvious there was no research fellowship they could offer me anything. So I came back from the interview and I wrote to him, and I said it looked as if there was nothing happening at the moment. My supervisor did contact a man called Bernard Barbour, who was a, maybe, I think, a student of Robert Merton at Columbia University and later became a professor and intern, and he worked on the sociology of science, in the Mertonian ways, and also became a professor at Barnard College, which at that time was a [...] Scottish part of Columbia University. And some [...] teacher recommended me very much. Needham also wrote to him and said, "You know, I'm highly recommending him,". And he wrote back and said, "I'm afraid you're applying very late in the year. All the research fellowships are gone. If he can come next year, I might be able to find him a research fellowship here." So I told Needham that this is what he had written, that he can't do anything this year, so I'd be going back. And then I got a letter from him saying that, "I know somebody called Dr. Jerome Ravetz who's at Leeds University, and I've written to him and said, "You should take a look at him," and find a place for you there. So I wrote to Dr. Ravetz, and I got a reply saying, "Why don't you come and give us a seminar so I said let it for the seminar and part of my thesis on the literary attack on science. And I went to Leeds, gave my presentation and at the end of it they came to me and said, "We are very impressed with what you're doing and we have got a [...] research fellowship here at Leeds and I would like to recommend you. So maybe I can start the machinery going for you getting a research scholarship here. So I came back yesterday. I hope to say I was a bit downhearted because living in London, London by then two years and 10 years tenure. I hated to depart from London, but anyway, I also asked Dr Pagel and he started the motion to try to see if I could get in something at Johns Hopkins, for example. But they were looking for a medical prefect person. He looked at Johns Hopkins, I remember with great [...]. He said they had been a better year trying to find an assistant, a surgical assistant. He forgets that I am an assistant medically [...] for instance since [...].

R. Uchôa: Like Pagel himself?

P. Rattansi: Yes. Yes.

P. Rattansi: And so I talked to Pagel and Pagel said, "Are you sure Dr. Ravetz can do something, etc. etc." and then the letter came from Ravetz saying, "Yes, we are succeeding in finding a fellowship member, when can you move?." At that time my mother was still alive in Kenya and so she was ill. So I said I'd like to sort of take three months off to go and see her and then when I get back I can come to Leeds. So I went to Kenya and then I came back here, I think about February of 20.. sorry, 1962 I think it would be '62, and I came to Leeds. And that's a very good congenial

environment. At that time the department consisted of three people. Dr. Ravetz was the head of the department. There was also Domino Cardwell, who was in the History of technology. Ravetz said... Ravetz was educated, I think, [...] College in the United States and then he had come to Cambridge University to do a thesis on specializing in mathematics and he did a thesis on mathematics at Cambridge, a MPhil, and then he had a bigger interest in the history of science and the history of mathematics. And at that time [Stephen] Toulmin had come to Leeds as head of the Section on History and Philosophy of Science in the Philosophy Department at Leeds University and Toulmin appointed Ravetz and Cardwell to research fellowship in the department. And he left for Columbia University and then stayed on in the USA and I Ravetz took over the sub-department. And so, right. So by the time I got there, there was Ravetz and Cardwell. A third person who had a research fellowship at that time was someone who was interested in history of religion and science. I think he was thinking of going into the priesthood. He became quite well known. Eventually, He got a professorship at Oxford on religion and science, which was an endowed chair, I forgot his name... So, the department was three or four of us at that time. I was supposed to be research fellow. I was supposed to concentrate on doing research. Ravetz did get me to start teaching, doing tutorials, and he used to give a course on history of science from the Babylonians and the Greeks down to the 17th Century. And he wanted various to be able to teach tutorials. So I was also brought in. Well, this is the first time I had seriously studied the history of science, and it straight from the Greeks up to 17th Century. I know it informally, but this is the first time I have to teach students, and I have to learn myself, and I think it was a good experience. And so then he was able to... it's just science. I should explain at that time they were merely doing undergraduate teaching. In other words, they would teach mostly chemists because the chemical... the development of chemistry. It depended very much on good relations with a particular department, and who felt they could spare their students to take an option with us. So much of the students came from chemistry. Some from [...] chemistry, and we could not give any post-graduate degrees in history of science and such. But then as time went on, I mean, my research fellowship was for... I think for four years, and after I'd done two years there, Ravetz said he had gotten money to enlarge the department, and they would make two appointments of lecturers, and if I was interested, I would be a favorite candidate. I still had the idea that I was going to move back to Kenya. So I just, stick to the fellowship. And the first two people in this recruitment were first... I may have gone over this ground before, but first was Crossland, Maurice Crossland. I think we discussed him before. And the second one was James McGuire or Ted McGuire as we called him. Crossland had done as many other people did like Cardwel. He had done the post-graduate diploma it was called in the History of Philosophy of Science at University College London. It was [...] department, he was head of development. It was [...] student of chemistry, and then go into teaching. Crossland had done that, and the future person we recruited in a year's time is research for was Charles Webster. He similarly had done degree at UCL, degree in

Botany. Then he went into teaching. But while teaching at Sheffield, he also took the post-graduate diploma in History of Philosophy of Science. Both these [...] came in at that time in History of science. That's the way they were recruited. They would do the post-graduate degree at UCL. So, we had a department that was doing quite well in terms of undergraduate. However were just doing teaching at particular course [on?] just one option for the people from the department. Form of teaching which [laughter]... [when?] the things become tighter in the educational sector proved quite difficult, as we saw later on [laughter].

R. Uchôa: How so?

P. Rattansi: Well, I think at this time, as I implied, for example, we were able to get research fellowship from the Leverhulme trust, which is a very big trust in Great Britain. It relies on the benevolence of the Lever brothers, famous manufacturers of soap and West African oil and so on, so on. They handed out this Leverhulme research fellowship in the history of science. So this is how I was able to get into Leeds. And this is how Ravetz and Cardwel got fellowship at Leeds. And then, of course, as I said, this other person, whom I can't remember his name, and Charles Webster. Obviously, the fact that they were then able to add two lecturers to the department, [...] because then, by that time, Ravetz and Cardwel became lecturers. And there are these two more, Maurice Crossland and Ted McGuire, sure there was a lot of money being spent in education. It was not so difficult to get [...] out of the way [laughter] subject like history of science [and be able] to get this kind of money to [...]. But I think things became a more difficult by the time... this is, of course, looking ahead. When I came to UCL, for example, by that time things were tightening up. This is looking at... I mean, from Leeds, I then went on to Cambridge and things started getting tighter just when... I spent four years at Cambridge and then I [go to?] London. So [...] looking into the distant future.

R. Uchôa: By this time, you got in contact with Jerry. And then how did you develop your relationship with him, in terms of academic work?

P. Rattansi: Jerry was a wonderful teacher and colleague and head of department. He had a wonderful sort of vision of the whole of history of science [laughter] from the earliest times up to Einstein and quantum theory. So when you went and discussed something with him, he can place it in a much broader context and a much wider chronological kind of setting, so he was a very interesting and inspiring person to be with. We often discussed things which [...] together and wouldn't know what came about [laughter] [...]. As I said, the collaboration with the department, my collaboration, started with Ted McGuire. And this was—perhaps I'm giving too much detail but while I'm still at Leeds, an offer came from Utrecht University. Hooykaas, who is a very famous historian of science at that time, historian of geology, etc, had retired from his chair at Utrecht. And they approached

Ravetz saying that we would like to you are highly recommended to succeed Hooykaas at Utrecht. Now, Jerry Ravetz had published some papers on Galileo etc. He didn't publish his big work or anything like that. But he was well-known. He attended lots of scientific history of science meetings, international [congress?], and so on, and had become very well-known. So he said that if he was going to Utrecht, then he would like me, he would like to take me to contract as a lecturer. And they said this was acceptable to them, if he wanted me to come with him. Now, Jerry, he had only recently appointed both Ted McGuire and Maurice Crossland. And they both said that look, if you go there... because his wife was English. She had done archaeology at Cambridge. That's where he met her. She also said that we have a young child. And I'm not sure how we would settle down in an atmosphere of Holland, how my child would sort of take to that kind of environment. So Jerry said he would go to Utrecht for the three months as a kind of trial. And he wanted to take me with him. But all these people said, "Look, we have just come into this department, hoping [laughter] we'd collaborate with all of you. Now you're leaving. At least leave Piyo here for the time being, you understand something about how the department operates. So I stayed behind and it was... I used to meet McGuire for lunch and this was when the origin of the paper on Newton and the Pipes of pan. He told me that he was [...] with this [...] before. But he told me that he was... I asked him what he was working on at the moment. And he said, "I'm working on these two great unifiers," as I remember, in the history of British science, especially one is Newton and the other is Maxwell. And he told me... I said, "I haven't worked on Newton very much." My thing stopped at about 1760's and 1770's something like that. And he recently come across some papers of Newton which he found really puzzling. And I said, "I haven't worked on Newton but if I looked at it, maybe I might be able to have something to contribute." And he showed me this paper which I think is described there. I would have to refresh my memory what it says about— Do you want to see?

R. Uchôa: Yes.

P. Rattansi: I guess I do have at least something to add to my memory. This is a letter dated 10th of March, 1965. This is me writing to Jerry Ravetz at Utrecht. And I say here that... [silence]...this is a letter... sorry to interrupt you, but there must be a letter before that. There must be a letter before that, in which I said that I'd suggested to McGuire that maybe the Prisca Theologia topic.

R. Uchôa: Yeah. That's his...

P. Rattansi: This one. Right. So it must be the first letter I wrote to him. Yes. So it must be... this is Jerry Ravetz letter from Utrecht, dated 18th March, 1965. So that must be a first letter, in which I'd written to him and said that let it draw my attention to the very interesting Newtonian material, and when I looked at it, I was

able to suggest to him that this whole thing seemed to have the *Prisca theologia* idea behind it.

R. Uchôa: Oh, yeah. I think you mentioned this right here. It's underlined right here. So, okay.

P. Rattansi: This one. Right. So it must be the first letter I wrote to him. Yes. So it must be—this... Right. Okay. Yes. Yes. Right. So, this is a document that McGuire found around that time and the letter says, "In the new rules and definitions the state has found, Newton notes that all the major systems of natural philosophy in the past with a Democritus or Epicurean or Aristotelian or Cartesian agreed on a number of basic principles. One, that there is a prime matter. There is also a difference in the other streaming four elements from all things derived by condensation and oxidation." So he doesn't go into what all of them have in common. He then says, "I suggested to Ted that all of this seemed to have a ring of the *Prisca theologia* about it, that is that there was one true natural philosophy. It was intimations as certain true propositions were to be found in a chain of philosophers... if a certain belief is found to be held in common in the ways the major systems and traditions of belief, then it must be true." And then it says that Ted has been working on this idea, and he has found some material bearing on this in the memoranda of the conversation held by the... sorry the Scottish mathematician David Gregory when he came to visit Newton in 1694. And this appears in the Newton correspondences published by Turnbull, 3rd volume. That he would spread himself... this was an important contribution because it shows that Newton was quite serious about ending adaptations to the main propositions of the *Principia*, and the annotations to show that most of these basic principles which is laid down in the *Principia* were well known to some ancients. So I quote from that and then I also note that... I suppose that McGuire found this. That there was a letter from Newton's companion at that time, his *protege*. He called him Fatio de Duillier young Swiss mathematician who writes to Huygens and says that Newton has discovered, that all of the main propositions of the *Principia* were well known to certain ancients, and I add that there's a [...] from Huygens saying that he's being too nice to the ancients and to stick to their knowledge of these principles. And he points to the fact that there might have been things like... I mean, after all, if you think about it, Copernicus says that the system which I have given to you in the *De revolutionibus* is not original with me. There are certain ancients who knew about it, and he quotes ancients who knew about it. So, Huygens says maybe... there may be some truth in this. That they had known that you could give an alternative system of the world where the sun is stationary and the planets go around it. But he says, "But you have given proof in the *Principia* which goes much beyond this. It sort of, you have found a proof for this for elliptical orbits which are post-Kepler [...]. So you should take Kepler's so-called three laws, they're much more than that, but this is three laws of Kepler, and it's only Newton who succeeded in introducing a system

which is able to account for and predict the elliptical motion described by the planets and the earth as a circle and other people have tried it, but they're never able to do it to an elliptical only. This is the signal and unique achievement of Newton. And therefore, I don't think you can attribute it to the ancients. And then he goes on to say, "I'm not going to publish some of his mathematical work now." So that is also an important sort of breakthrough in trying to see if the *Prisca Theologia* idea, which I suggested, really fits Newton. So this is at the stage that in March 1965. The article was eventually published in I think November 1966, so it took more than a year really to put all of these ideas to gel together.

R. Uchôa: So, I was thinking that going through this [...] the idea of *Prisca Theologia* is really something key that you are working with them. So I'd like you to comment more on this topic. I mean and not only on Newton's conception, but in how people conceptualize the *Prisca Theologia* in his context and why was that important to people at that time. That's one question. The other question I can think... it's how scholars in the 1960s were working with this concept that you are using. So it's a past and present question.

P. Rattansi: I think as people did at that time, I came into historical science from really around [...], and the thesis itself was concerned with the relation between science and literature in Restoration England. And this is how I think I've told you of how I first was trying to do a sociological study of the "two Cultures". And from that, I became disillusioned with the sociological study of this kind of topic. And then I went into looking at the origins of the scientific profession in England, something like more of a scientific career or road, and I decided that the place to look at was the foundation of the Royal Society of London in 1665. And then I started seeing that the Royal Society became the butt of attack, became the subject of attack of the leading literary figures of the time. These attacks start very early in the career of the Royal Society and continue really to the end of the century and even beyond. And I was trying to see why they were attacking it. But among other things, I found that one particular author that I studied in some detail was Jonathan Swift, who was well known for his *Gulliver's Travels*, which was sort of as a children's book but is a bit of a satire of humanity, and I think it's well known that there is a chapter in that *Gulliver's Travels* which talks about the Academy of Lagado, which is a kind of veiled reference to the kind of thing that the... the kind of program that is being implemented by the Royal Society. And yes, there's people who eat only meat carved in the form of particular mathematical figures and so this kind of satire, they're wasting their time on these kind of studies when the country lies in poverty and famine strikes and so on. But I also found that another well-known work by Swift, which is called the *Battle of Books*, which shows books fighting other books in a library, there were lots of references to Paracelsus. This is the first time I'd really heard about Paracelsus. And I wondered what Paracelsus, I mean what did he write which was in the literary annotations to the *Battle of Books*, I found they would just

refer to him as one of the dark authors of Swift. But then I started doing library work or research on this, and found that the work of Walter Pagel, until then, an unknown figure to me, was very important. And I started reading his papers, which were published in English as it had the heading Religious Motives of the Pneumatical Biology of the, I forget the centuries, but it goes from I think late medieval right into the, well into the 17th century. I started reading these, and it made much more sense of what Swift was attacking. So this brought me into looking at Paracelsus. And from Paracelsus, I also went on to study Helmond, again, because of Pagel's work on Helmont. And so the idea of the *prisca theologia* in studying Paracelsus and Helmont, I also started looking at the work being published by various people connected with the Warburg Institute, notably Frances Yates first, and then D.P. Walker, Daniel Pickering used to call, Pickering Walker. And around that time, so these were two figures whose work particularly attracted me, and I started looking at, this is when, Pickering Walker especially had done a series of papers on the *prisca theologia* in France and he's also written this book called *Spiritual and Demonic Magic* from 1502, [mumbles] in further, and here, the idea of a *prisca theologia* was given much more a, I mean it was an elaborate discussion of how this *prisca theologia* idea he thought originating with the Florentine Academy or Marsilio Ficino and so on had a great influence as the Renaissance, so to speak, humanistic Renaissance spread northward, first in France and then also in England. So this is how the idea of *prisca theologia* became very interesting for me. In fact, while I was at Leeds, the first task I undertook was something that was not mentioned very much in the theses, but an offshoot of it. This was that I started seeing first of all that it was possible to see... sorry. I should say it... I became interested in the influence of Paracelsus and then of Helmont in England particularly, and the first thing I started studying was the influence of Paracelsus during the 1640, 60 period, and I found that Allen Debus was also working on this one. Meanwhile of course, I think I already discussed this with Needham, I was introduced to Pagel, he wrote to Pagel recommending me and saying that I would like to go and see him. So I went and saw Pagel, met him. We became good friends, and he said to me, "There is a young American called Allen Debus who is working at Harvard on his PhD, and he's also working on the Paracelsians a little earlier than you. He's looking at the Elizabethan Paracelsians. So it seems a pity that you two don't know each other, and I call you for tea sometime, and you can meet him." So at that time I also met Allen Debus, and we became great friends, and so I started working on these paper [00:38:50] on [...] Paracelsus. I called Paracelsus and the Puritan Revolution, showing how the ideas of Paracelsus got a very positive response from the people who belonged to the kind of opposition movement, and how in a sense this kind of politicizes Paracelsianism. I've never [...] sociologically interested in how the ideas sort of respond to me as things going on in society. So it was a paper on... it was a kind of reaction against Paracelsus again, and I thought this was also of great interest for looking at the career of Boyle. I held at that time a kind of view which Paracelsus would strain, but the view that you have to look at two phases in the life of Boyle and the time before he goes and joins the

Oxford group. When he's very much influenced by Paracelsus, Helmont, and so on, and it gives him this kind of word, and then when he goes to Oxford he changes. He sees dangerous links between Paracelsus and then the sectarians, and he thinks we're after the killing of the king, and were great enemies of political order, and stability of society, and so on. So if you like, a kind of conservative reaction against these people, and I saw Boyle as going from one kind of phase to another, and going from a kind of Helmontian Paracelsian kind of outlook to a mechanical philosophy, and of course this was giving a kind of political dimension that often of mechanical philosophies that kind of is influenced by political events and things like that. As people become sensitive that in certain situations the Paracelsians' philosophy is more in harmony with the people who are trying to overthrow the established social and political order, while the mechanical philosophies connect more as a kind of support for a more conservative kind of... of course, if I had published this it'd have attracted a lot of people because of the political dimension I was giving to this thing. And then, of course, the debate has become much more complicated now. But it seemed to me a breakthrough that type. Then I also started working on the paper on the Galenism versus Helmontianism in Restoration England. And this again was a paper which was noted by other people and a lot of people told me that it did change their outlook on how to do history of science and it was very flattering, that is true. But, of course, I did it coming from a sociological background and so then I thought this was the obviously way they do these things. And in my thesis I had approached the whole issue of the origins of the Royal society. I treated this as what the anthropologist or sociologist would do. It was a foundation myth, this is how the world society came into being. So I decided this is what they're saying. And just look at what... why particular people are saying this giving this explanation. And so I was trying to use this kind of sociological angle in dealing with these things. Some people, later, said they're disappointed that I foresaw this very promising kind of approach to history of science. They said the work was Pagelian [...] kind of ideal, in just treating philosophically things ideas clashing with ideas not always placing them in their social and political dimension. But any way is fine.

R. Uchôa: Ah, that's the interesting part. But why do you think that you have changed your approach?

P. Rattansi: I think I was very aware that I changed it. But perhaps the focus did move. I think particularly with the paper on the *Pipes of pan*. It started moving onto the world of ideas. I hope I haven't forgotten to move the [laughter] what's good around them not just, but it is true that yeah, the later papers were more concerned with ideas than in the [...].

R. Uchôa: Than the so-called social contents?

P. Rattansi: Yes. That is not a problem there and the...

R. Uchôa: The thing is... don't you think that it's part of attempt to get away from the Mertonian or Weberian way of looking to history? So you kind of... that's the thing I am not doing that, so I...

P. Rattansi: Yes. I continue to concern myself with the Merton thing. I did write to, when I came to Cambridge, I wrote some papers on the social interpretation of the scientific of revolution. I'm trying to show that you can't... you can't do this only on the ideological plane, you got to pay a great deal of attention. I made the point at that time that perhaps I was too biographical. That... I said there in these... the paper which is called the... which is on the social history of science and how we can make life in 17th century England. I gave that as a lecture at Cambridge, a series of lectures on the scientific revolution, and I said there that people say that the social history of science is dead now, which is a crude Marxian kind of attempt to in the history of science on the side of progressive kinds and so on. But I said that there are good reasons why a lot of attention has been... I mean, we have really brilliant people who have shown us the sort of philosophical contours of this new kind of scientific revolution, people like Cassirer, Koyré, and, three people [laughter] particularly influential in this kind of... Koyré, of course, has a great influence on all of us. He wrote very well. He seemed to know his science [laughter] at the same time he was saying that it's ideas which matter [...]. And there, I did say that I think it's time to move on now. We're very busy just examining the kind of philosophical structure of the scientific revolution. We should now try to anchor it in changes that are going on in society as well. So I [...] it. But [...] perhaps, yes, it is more—

R. Uchôa: Another thing that it's really comes out of your letters is an attempt in understanding the natural philosophy not only in England, but in other parts of Europe. How you with other scholars and peers are working with different context at that time?

P. Rattansi: Yeah. I think we became more and more aware that much of Anglo-American work on the history of sociology of science, historical sociology of science feel like Merton work was too centered on Britain. I remember writing a review of a well-known book at that time, *Ancients and Moderns*, by Jones, I think. [...] Jones [...] the book itself was quite a tour the force.

R. Uchôa: You quote him in a critical way.

P. Rattansi: Yeah. But I wrote a review for it [...], I think Bob Young recommended me for the *British Journal for the Philosophy of Science*. But I came to the conclusion that it was a little England kind of view of history of science. And then we must pay attention, far more attention, to the continent and I suppose the same kind of thing when Charles Webster came to work on history of science that we must be very sensitive to the fact that... and, of course, working on Paracelsus

and Belmont I became more and more conscious of the fact that all these ideas came from abroad and were kind of given a English kind of interpretation and so on [...] substituted these kind of changes. But we started talking of the... I think [...] got us talking about the European republic of letters that we should see things in this kind of context, a much wider context than this England. To be fair to [...], of course, he is taking a theme from Weber. So he's conscious of the [laughter] [...]. But I think things seemed to be very insular...

R. Uchôa: Yeah. Another thing that it's connected to this question is that it seems that you are interested not only in study other parts of Europe, but how people from other countries are going to England and influencing their way of thinking about natural philosophy. There is a line that you mention. I don't know if it was a lecture that you gave that's "What is the Cartesian influence on the period of restoration in England?"

P. Rattansi: I think generally it is just science now is much more international in scope and people take full account of the Paracelsian influence especially on Newton and so on. We can even say this is a battle that has been won.

R. Uchôa: Just going back to the question on the *Prisca theologia*. Do you have any kind of... I know that's kind of the "curse of the origins" but do you know when the concept of *Prisca theologia* emerged in history?

P. Rattansi: I mean, in looking at the origins of the *Prisca theologia* idea, of course, I just followed the lead of Yates and Walker but it's sort of like nobody had applied it to the English scene in any kind of detail. So finding it in Newton seemed to be a quite surprising for us at that time. There is much more to be said on that subject. I mean, if you look at the... as I hadn't done at that time, but if you look at the German, particularly historiographers about the history of philosophy, people like Daniel Guild and Nordhoff, wrote on these two of philosophy nearly late 18th century. You'll find they are aware of this *Prisca theologia* I think, very well. In fact, in Nordhoff, he calls it *philosophia mosaica* because they tended to get back to Moses and refer to that and so on. And of course, when you're studying the history of alchemy there are these traditions. Too often, of course, in Christian alchemy, we go back to Moses and so on. Paracelsus, of course, means with it...you know the old text for understanding nature is to go back to the Genesis text and see it as a description of a distillation. So I'm not sure I'm answering your question but there are all these multiple sources for this idea that *Prisca theologia* and people have noted particular aspects of it in [...] usually.

R. Uchôa: There is a... may I take one of these letters? There are some parts of the letter that there... the answer of your letter, you wrote back, which he mentions

the idea of *Prisca theologia* in a particular way, I'd like to ask this. [silence, looking at the letter].

P. Rattansi: I think he points out that for the more critical kind of thinkers, the *Prisca theologia* idea was a way of drawing attention to do the fact that of course, the classical society that wants you to see that the existing societies of the 17th century in refinement and culture. And so the idea's not to be dismissed as just a kind of... it is drawing attention to something positive. Is that what you were thinking of?

R. Uchôa: No, no. He does say this as well, but he used the idea... I'm commenting on the letter of Jerry Ravetz, 18th March, 1965, and he says, "I'd like to argue that the 'prisca' idea is not so silly, and that it is a disastrous oversimplification to class such things as Medieval survivals. First, I use it to some extent myself in doing history, and in justifying the history of science." That last line was the part that made me puzzled. I mean, Ravetz says: "I used to some extent myself in doing history of science, and in justifying the history of science". How *Prisca* can do such a thing as a justification for the history of science?

P. Rattansi: Yes, that's slightly puzzling. I hadn't noticed that again when I read it because of course, I mean, usually we frown on this as [you got?] a Whig interpretation of history.

R. Uchôa: Yes, right.

P. Rattansi: That we shouldn't be looking for precursors or [...]. We should see that the past forms a unique kind of unity. And if you select bits out of it and so on, then it will mislead that... I know that, of course, Ravetz is not a Whiggish historian of science. So, yes, that is slightly puzzling. I'm not sure what it means. He says it can be used as a justification for doing history science.

R. Uchôa: Yeah, it's...

P. Rattansi: Because it can be the crudes of, "Yes, it's justified because you might..." I remember for years, people used to tell me that: "You study history of sciences because you might find some overlooked insights and things." In one sense, that's not untrue. I mean, that kind of thing where it's... today, a very good example is my former colleague in the UCL department Hasok Chang and he's doing... but that's not a classically Whig history. He's trying to show that there are things we have forgotten. Parts not taken, so to speak. And leads not followed up because... I mean, he's checking things like water boils at 100 degrees centigrade, right? Chlorine is used as his example to show that we hold many of these beliefs, and when you test these beliefs strictly, it will not even be true, but that's a different way of using it. I'm not sure what he meant by that, that I can...

R. Uchôa: Well, yeah, but...

P. Rattansi: Unless he means, "I can use it to get money out of people for [laughter] keeping the development of the subject going." But, find something...

R. Uchôa: Oh, yeah. Yeah, I think that's it. That was the part that came out after so many readings of the letters. And again, I thank you very much for your time to talk with me.

P. Rattansi: Thank you. Yes, it's nice to go back to these scenes of one's youth.

R. Uchôa: All right. That's great. Thank you.