doi: http://dx.doi.org/10.23925/1980-7651.2019v23;p45-64

Interview with Piyo Rattansi[1]

Interview[2] – Part V (15/09/2016)[3]

R. Uchôa:[4] Professor Rattansi, thank you very much for your time. In this part of our interview I would like to keep exploring some historiographical issues and some part of your biography and try to discern how, maybe, these things overlap. I'd like to start this conversation with some comments by a person that your met, a man called Robert Young. And he makes some comments on some scholars that were involved in the historiography of the 60s and then. He talks in the book "Darwin's Metaphor" about the historiography of the 60s, and makes the following statement. "How, in the face of this intellectualist orthodoxy, did the revival of the study of the social dimension of science become attractive? Its relevance began to be felt in Cambridge, not as a result of the intellectual preoccupations of the teaching staff in the history and philosophy of science, but from three main sources: the influence of the Leeds department; the approach of two philosophically oriented political historians in Cambridge, John Dunn and Quentin Skinner; and the work of a group of Oxford-trained social historians. Something very exciting was going on at Leeds under the catalytic influence of Jerome Ravetz and three young scholars whom he had attracted there: P. M. Rattansi, J. E. McGuire, and Charles Webster. None of these was from the mainstream of British academic life: Ravetz is an American emigré and an ex-Marxist whose original training was in mathematics; Rattansi is a Kenya Indian, an Ismaill, who worked as a journalist and took his first degree in economics; McGuire is an Irish-Canadian of maverick intellectual disposition; and Webster is a highly individualistic British radical who worked in education before doing graduate work in the history of science. Rattansi moved on to King's College, Cambridge, and from there to a Chair at University College, London; Webster became Reader in History of Medicine at Oxford; McGuire took up

[1] Dr. Piyo Rattansi is Emeritus Professor of the Department of Science and Technology Studies (DSTS), at University College London (UCL).

[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 Marcia Helena Mendes Ferraz, both researchers at PUC-SP and Honorary Research Fellows at UCL.

[4] Museu Paraense Emílio Goeldi, Belém, Brazil. ✉ raphaeluchoa@museu-goeldi.br.

a professorship at Pittsburgh. All four of the Leeds group were placing the preoccupations of the seventeenth-century natural philosophers in theological and social contexts. They were interested in philosophical issues, but unlike Buchdahl and Hesse, this was not for the sake of the good philosophy to be squeezed out of (or read into) them. Rather, they were shamelessly relativist and contextualist and were far more interested in the social milieu and the philosophies of nature underlying the works of the scientific virtuosi than in the so-called mainline of the development of modern science". So just to start a conversation here was his remark. I know that you know him so if you could just start on how you met him and then leave some comments on his remarks I just read. If you agree or not, I mean, with his views.

P. Rattansi: I think we have already discussed something of how I came to meet Robert Young. He was a young American who had studied for his Ph.D. at Cambridge. It was an interesting thesis he wrote. It was on cerebral localization. One of the features which made his work […][5] different was that he took phrenology very seriously. And phrenology is something we think about as-- antique shops at one time you could still pick up busts of people with the skull marked with area which is responsible for this and that or the other faculty. And it's now discouraging that people don't believe in phrenology. But what Bob Young found out is that it had quite an important effect in neurology because the idea of cerebral localization was assisted by these other various […] kind of-- something which we now classify with palmistry and astrology phrenology. So he was sensitive to this kind of thing, that sometimes, even you are looking at what we can actually think of a science, it may be influenced by occurrence which to us appear "unscientific". So I think that is also one way in which the work was doing on Paracelsians and Helmont and so on, appealed to him. That this could be an important-- like they all looked-- aspect of the scientific revolution. He had a great deal to do with my coming to Cambridge. I think we already to discuss that in some detail.

R. Uchôa: Yeah.

P. Rattansi: You also mentioned that he says something about the group of young historians then becoming quite influential at Cambridge. There are two people he mentioned in particular. One is John Dunn, and the other is Quentin Skinner. Both, of course, now established historians of political theory. Dunn, at that time, had written his page three on John Locke, and he departed from the conventional leaning of Locke by showing that Locke was also a-- not a kind of-- a bridger of a kind of-- a secular kind of idea of political theory and so on. He was a kind of natural theologian at the same time. Quentin Skinner at that time had published a famous article on the conventional history of ideas, which theorizing in the Journal on the Philosophy of History. It made quite an impact. And he's, of

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

course, now, a very well known author. He wrote a sort of multi-volume history of political thought which is still highly regarded, and he's a well-known person. Mary Hesse, I should say something about her. I had read with great admiration. Her own thesis was on action at distance in the history of physics. It starts at the Greeks and works up to Einstein. She taught Philosophy of Science at Cambridge at the time when I was there. I think she was a little perturbed, of course, by the lectures that I was giving at the same time on the History of 17th Century Science. I was not a member of the faculty of the Philosophy of Science. I was a fellow at King's Scholar-Cambridge, research-- senior research fellow, the four-year fellowship. But the Committee on the History of Science, which included, of course, Bob Young, invited me to give a course of lectures on-- which I entitle Science, Society, and Religion in 17th Century England which went from, let's see, Gilbert to Newton. Which to my surprise, too, and their surprise, it took in a very large audience. And they kept on coming. I gave the lecture in the first year, and the second year, and the third year. The fourth year of my fellowship I spent at Princeton because Thomas Kuhn invited me to Princeton for a year. I also was a member of the Institute for Advanced Study, so I had to interrupt my lecture for that year. But even after I-- after finishing at Cambridge, when I went to London, they still wanted me to, if possible, keep on giving that course of lectures. But the course of lectures did disturb, I think, Mary Hesse a little. My popularity and feeling that I was giving an account of the development of History of Science which was at odds with what she believed in, and so on.

R. Uchôa: In what sense?

P. Rattansi: Well, she asked me, I remember, just before I was going to Princeton, she asked me if I could let her have my-- the things I had published on History of Science. Which included, at that time, things like the article on the Hemontians and Galenists, Galenist versus Hemontians in Restoration England. Paracelsus and the Puritan Revolution. Also, of course, Newton and the Pipes of Pan, which I would recommend in collaboration with Ted McGuire. And she then wrote a response, which I only heard about when I came back from, while I was still in Princeton. A paper called Hermeticism and Historiography. Around that time, Bob Young-- it was the we were commemorating, of course, Joseph Needham, a great figure at Cambridge. Also played a role in my own intellectual life. They wanted to publish a collection of essays, a *Festschrift*, for I suppose the 65th birthday of Joseph Needham. And Bob Young was asked to edit the volume. He was very close to Joseph Needham at that time. And Bob Young asked me-- well, he decided that he-- he had asked Mary Hesse to contribute something. And she contributed this piece on hermeticism and historiography which was a critique of me and other people, historians at that time. And he asked me if I would like to make a reply to it which would also be published in the Needham *Festschrift*. And he wrote an introduction, which he said something that you read out already. But where he

seemed to think that this was becoming the major debate, the center of which way the history of science should be going. The paper was of some-- I mean, it did create a bit of a sensation. And other people wrote about it at that time and so on, I remember. So all this is background to this.

R. Uchôa: Yeah. A bit further, he talks-- I don't know if he's talking about the same meeting that you are mentioning. But he says, "It was in the atmosphere of a strong orthodoxy in the history and philosophy of science at Cambridge, alongside a network of overlapping intellectual affinities and sympathies that, in 1968, Rattansi and I brought together a number of scholars and others for a seminar which met monthly to consider the relationship between the history of science and other branch of historical studies." And then he goes, "At quarterly intervals, senior visitor gave papers to the seminar which represented establishment point of view in different branch of history." So he quotes Needham in Chinese culture and history of science, Gombrich in the history of art, F. Yeats in the Renaissance Studies, Trevor Hooper, Phillip Collins and so on. So he talks about the same thing.

P. Rattansi: Yes. I remember these seminars very well. We were able to, through Bob Young, get a grant from the King's College to fund a series of lectures on current issues in the history of the sciences and related subjects, which is a very wide kind of limit. And the seminar met once a month. And we would invite a speaker to give a talk. The speaker and the also, I guess, sort of permanent members of this seminar would attend all the seminars. They included D.P. Walker from the Warburg Institute. They included, of course, at Cambridge, John Dunne and, [remembering] Quentin Skinner. Myself, of course, and Rob Young as the people would attend these meetings. And so the people invited and the people who […] is a prominent members of this seminar attended the meeting. We had dinner at King's College. And then the guests were put up for the night. Usually, they did it on the Fridays, and they will put up for until Saturday morning. And we'll give them their-- we'll pay their travel costs to Cambridge and so on. And most of them were prominent members. They included Charles Webster and some others. One full-time lecturer said he travels at various universities. And it became quite a point for people whom we have left out and felt rather annoyed that we have left them out, and people […] very good dinner. But it was very enjoyable and very interesting, and I hope productive seminar. And so this is what is recalling as a high point of having collaboration together with Cambridge.

R. Uchôa: Did it get in any direction this seminar-- I mean, what came out of this in terms of historiographical directions for the history science?

P. Rattansi: I think because-- think of this seminar as left-wing intellectuals getting together. Very wide variety of political and intellectual commitments were mostly members of the seminar. And we invited very different kind people. I mean,

Gombrich, as you said, came and talked to us so did D.P. Walker—Gombrich was an established figure in the History of art, D.P. Walker was a person who wrote about things like *Prisca theologia*. Miss Yates also came and gave us a talk. I think what we talked about Bruno, I forget what she talked about. So I don't think there was a great uniformity of commitment for these people. Very, very different. I remember one of the person-- he was the only person who was not a member of the teaching staff at—was research Fellow was Roy Potter, who was still doing his PhD, or DPhil as they called it in Oxford and Cambridge, at Christ's College in Cambridge. But he was so bright and obviously was—he had so many ideas and things. That he was in the non-teaching research staff. And he was a very good member of that seminar. Difficult for me to know how much of an impact [laughter]—it really made on the way of history of ideas, really, history of ideas. We hated the term history of ideas which is associated with Lovejoy. Because it seemed to be suggested we were discussing ideas and a solution from a social-political […] of society bearings. And of course, […] if you like social history of ideas.

R. Uchôa: So how was the institutional landscape in the history of science at the time-- I know there is Cambridge. I know there is Leeds, and maybe there is something in Harvard, US. How was the configuration of the field, institutionally speaking?

P. Rattansi: Yes, I think the-- at Cambridge, the department is headed by Gerd Buchdahl. G-E-R-D Buchdahl. Buchdahl was a man who was interested in the philosophy of science. He came from the continental tradition, which is slightly different from the English tradition of natural-- philosophy of science. And more representative of the English version I think would be Mary Hesse, who had been a lecturer at UCL at some point. I think a lecturer in mathematics if I'm not mistaken. So there were really, I suppose at this time, only three kind of functioning departments or sub-departments of history and philosophy of science. The oldest one, of course, was the one at the University College London, which was headed at that time by Douglas McKee, a well-known historian of chemistry. I suppose we all thought of him as an old-fashioned historian of science, who was not interested in the kinds of things we were interested in. In other words, sociological, political, economic, and all these other aspects—

R. Uchôa: How so? How different were you in terms of theoretical approach?

P. Rattansi: At Leeds, of course, it was very different. I mean, under Jerry Ravetz, it was a kind of sub-department of the department of philosophy. We didn't have all that much to do dealing with the department of philosophy. I mean they were, firstly, a kind of English brand of philosophy. This anecdote I sometimes use to illustrate the difference between us and them. We had a departmental budget for

buying books for the department or the sub-department. The department never made much use of that money. And I remember talking to one of the members. Well, he said, "But look, I mean to do philosophy, you just need a language, which is available to all of us, ordinary language. And you need a piece of paper and a piece of-- a pencil or a pen. And you can do philosophy." And he said, "You are in a kind of discipline who needs lots of secondary sources, and so." But anyways, so I suppose the kind of people Jerry Ravetz attracted is, as Bob Young puts it. We were otherwise left-wing inclined. We were, except for Maurice Crossland-- I don't know about Ted McGuire either. I don't think he had very strong political commitments. So it's slightly making it seem like left-wing intellectual thing-- not quite fair to some of these people. Or they wouldn't think it fair of them. But of course, we were very conscious that we were doing something different, something new, something that was not being done by, for example, the University College of London. Besides McKee, there were other people there chairing History of Biological Sciences, which was filled by-- sorry the names are going out of my head. But we saw that as a department which was pursuing history of science and very different lines than the one which we were embracing.

R. Uchôa: Bob Young mentions, and you already have mentioned, those other two authors and scholars, Ted McGuire that wrote with you and Charles Webster. Were you three sharing the same theoretical way of looking into the history of science or did you have some different perspectives that were not so related, even though you were in the same department and you wrote the paper with McGuire?

P. Rattansi: Yeah. I suppose perhaps Ted and I perhaps had more in common in particular allegiances and so on. And Charles Webster. Ted McGuire, I never knew about his political commitment or his political opinions and so on?. And I don't think he was working in the history of science. It's mostly in philosophy of science really. And I don't think that shows any strong social or political commitment of any sort. He does philosophy in a different way. And I admire the way he does-- he's a very good scholar, a very good philosopher. But on the historical side, I don't [crosstalk] we have very much in common.

R. Uchôa: I'm really in a way trying to understand-- because you are publishing this kind of work like “Newton and the Pipes of Pan”, which is very innovative, very new and fresh in terms of perspective in the '60s, and some works like “The Structure of the Scientific Revolution”, which is obviously different from your work, but it is also different from the old tradition of analysis on the history of science. So in which sense do you see your work and other kinds of works that were published in the 60’s being cut out from the old tradition of the history of science or of the so-called positivist history of science?

P. Rattansi: I think at Leeds-- I think we were trying to develop a new kind of approach to the history of science at that time. And we were. When The Structure of the Scientific Revolutions by Thomas Khun appeared, we were greatly influenced by it. And we thought that was a kind of real breakthrough. I think one of our programs […] the philosophy of science. Most of these departments are called department or sub-department in history and philosophy of science. But what exactly was the relationship between the history of science and the philosophy of science? This is not really clear to us. I think I told you that I asked McGuire, "Would it be possible to devise a course on the philosophy of science? Which, for example, when we were talking about Aristotle, we'll talk about, I don't know, functional ideas in the history of-- I mean, is there a different kind of philosophy of science relevant to biology, and physics, and so on […]?" And he said, "No. If you teach the philosophy of science, you have to start from basics. And you got to build from there. You can't […] a handmaiden to the history of science." So when Khun's book appeared, we were all greatly excited by it. Because it seemed to be a way of, not reconciling, but using philosophical ideas to make sense of what happened in the history of science in a much more integrated kind of way. The only thing though which made it difficult for us to accept his ideas once the basic enthusiasm was over, was his idea of gestalt shift which complete break with the former world. Leaving in a different kind of world with evolutions, one after the other, and so on. But I think we found that work very exciting. Because remember that it's not until I get to Cambridge that we begin to be affected by the Continental developments in the […] sort of these developments in philosophy. But the work of Michelle Foucault, for example, I mean we hadn't heard of the thing while I was at Leeds. It was only when I came to Cambridge that I became acquainted with Foucault's work. But already that was having quite an impact on people like John Dunn, I think, and Quentin Skinner, and so on. Mind you, these are people working political theory but people like one of the people was also in the seminars, younger than us but developing quite fast, Rudwick Martin Rudwick. He had studied Life Sciences at Cambridge and he became interested in the Philosophy of Science and the History of Science. And he'd already written a book or was writing it on History of Paleontology. And he particularly, I remember him, very excited by the new work of Foucault. I think the one book we would have retained at that time would have been one of his first works—sorry, forgot the name, translated into English. So besides Kuhn, these new authors were beginning to make it's presence felt. I must confess, I could never-- I never finished reading a book by Foucault. But I was aware of these works and new kinds of ways of looking at issues and which were coming out in the Continent from this different division of philosophy. But it was very influential and I think on Bob Young too. Martin Rudwick of course is contibuting […] history of the geological sciences and his book on the […].

R. Uchôa: So, any other sources of influence, of inspiration for looking differently into the history of science?

P. Rattansi: I think these would be the main one [crosstalk]. And it's also […] got some general ideas.

R. Uchôa: Yeah. Yes. You already knew by that time Walter Pagel. So where was he at this moment that you were working at Leeds and how did his work influence in the kind of job that you're working in?

P. Rattansi: Sorry, I didn't understand perfectly, the work of which person?

R. Uchôa: Walter Pagel.

P. Rattansi: Oh, I see. Sorry. Yes, yes. I was greatly influenced by Pagel, of course already when I was finishing my thesis. When I came across the name of Paraselsus being mentioned by people like Jonathan Swift. And I wanted to know, but who is this person? And all of the annotations to be modern additions of Swift work would say, "This is one of his dark sources." And dark sources they would mention the Jakob Böhme and Paracelsus and all those people. But then I discovered that the great authority on the incident at that time was Dr. Pagel. We may be going again over the same ground, but I read his articles, which were appearing in The […] for the students of medicine, edited by Temkin at that time. And I read them, and I was very excited about them. And I started looking at that time, but I didn't do much of this work during my thesis, but only after I finished my thesis on trying to trace the influence of Paracelsian writings and then Helmondian works in England. In my first two publications on Ambix were on this subject. One trying to show that the influence of Paracelsus becomes very important during the troubled period of 1640-60. And some of these reasons are connected with the political, social events fast developing and changing in the 1640-60 period in England. So, selecting one particular country and what happens to what-- how does what happens to paracelsian ideas into particular social, political, intellectual sphere. This is the task I've introduced; not a very long article, but I tried this in the paper on Paracelsus and the Puritan revolution. It's for that time quite an impact.

R. Uchôa: I think it wasn't in 1945 that he published his well known the "Vindication of Rubbish" where he sets out this idea that the task of the historian of sciences is to understand the context of the people in its own terms. So, it was 1945. How do you think, having known him, had met him personally, how do you know he came into this kind historiographical perception or on how a work on history of science should be done?

P. Rattansi: Personally, it went into-- Pagel started after I had visited Dr. Joseph Needham at Cambridge just by writing to him and saying I wasn't getting much help at LSE while doing my thesis because there were few students of science there, and if I could come and discuss some issues with him. And he immediately

said, "Come over and have lunch." And I went to see him at Cambridge. We became very good friends. And he supported me in my work through the history of his life. He was the person who said to me that I talk to him about Pagel. And he said, "You may not know him, but he lives very close to you. You are living in Fitz New Road in London. He lives in Med Hill. You're both in Northern London." And I dropped him a line and suggested-- and he also dropped him a line. "Go and see him." So I dropped him a line, went to see him, and again, we became very good friends. And he's the person who told me that there's another per-- Yes, he phoned me up some weeks after I first met him, saying, "You know, you are not the only person who is working partly on Paracelsus at London University postgraduate student. There's an American student from Harvard who came to see me. He's also working on Paracelsus, but he's working with Paracelsians during the Elizabethan times. You are extending it into the civil war period and post-restoration. So you should get to know him. So invite him for tea and you also go over for tea on that day. So I went and saw Allen Debus for the first time. And I found that he was living even closer to me on Finchley Road. He had come with his young family. I think two children at the same time and I met his wife and so on. And we decided we would keep closely in touch. So this is how I met Pagel and Debus. And of course, his contact continued then throughout the rest of my life. Pagel's work made a great impression on me. I spoke about it a little when after Pagel had passed away. There was a memorial meeting for him at the welcoming Institute for the history of medicine. They'd invited many speakers. Someone from Germany. Joseph Needham was invited to come and speak because he had been a great friend of Pagel during the brief time at Cambridge that Pagel had spent at Cambridge trying to establish history and philosophy of science there and I was also one of the speakers and there I did recall that-- I compared the version that Merton had made on me. And the impression that Pagel's work had made on me. And I said they were very different kinds of people. Merton was concerned with Weber's ideas of how Puritans had nurtured mercantile capitalism in the 17th century. This was Weber. Merton took from that the ideas of could there be some kind of religious motives inspiring and turning towards natural philosophy, natural science during those centuries. And he tried to then sociologically establish this by his survey of the rise of the Royal Society and the flourishing of science at Soviet, sorry, the English Universities by the mid 17th century. And he thought you should see the positive correlation between Puritanism or Puritan Calvinism and a motivation to engage in scientific activities. Now Pagel was a very different kind of thing. It was about religious motives and the medical biology of the centuries from, I forgot, from which to be well into the 17th century. It was not a sociological study. It was a self-study in the filiation of Ideas and so on. […] But, I felt after reading one sided, Pagel could never see the work of scientific ideas in 17th century again in the same way. He just liked to-- Life changing kind of experience and […].

R. Uchôa: Oh, yeah. Yeah, yeah, so, you stress out that the work of Pagel was done in a different way. But could you trace his own sources of influence to construct his own ways of looking to the history of science. Is it possible to remember his own influence to look into the history of science as well? Because he comes from a medical background, right, so how do he--?

P. Rattansi: It's a good question, I think, and you yourself started by talking about his article called The Vindication of Rubbish, which I think was published in the Middlesex Hospital Bulletin or some such really obscure journal. Perhaps I could recall that he told me how he came to the study of Paracelsus and Helmont-- this was his recollection, because we historians are always suspicious [laughter], people's recollections of how they started off. But anyway, his recollection was that-- how did he become interested in Helmont? He said that he had contracted tuberculosis when very young, and was suffering from it, and he asked a fellow-- I mean, Pagel was born-- I think we've probably been through all this, but he was born the son of a person who happened to be Jewish, living in Germany, who lived in Berlin. He was a famous historian of medicine, his father, Julius Pagel, and he had the chair in the history of medicine at Berlin University. And Pagel in his recollection of his things, talks about how he lived in a street where so many famous figures from Berlin University lived, in so many different disciplines. So history of medicine never seemed cut off from a whole range of gymnasium subjects, with the German academy's pursuit. But he contracted tuberculosis, and he said he studied medicine himself, and he was appointed, was called *Privatdozent* at Heidelberg, so not at Berlin but at Heidelberg University. *Privatdozent* means that you are not a member of the faculty-- I mean, you are a member of the faculty, but you are not a professor yet. And your students pay you fees, so you depend very much on how many students you are able to get. And he was *Privatdozent* both in pathology and in history of medicine. Now, he says that-- sorry, before that […] he had contracted tuberculosis, and he asked somebody, "What is the earliest work which discusses tuberculosis?" And he said, "Well, there's a work by--" something like, "it is mentioned in the works of van Helmont." And Pagel could read Latin, and […] Greek, with his gymnasium education, and he got a copy of the, I suppose, the Ortus Medicinae of van Helmont, started reading it. And he said, "My first reaction was, 'This is utter rubbish. This man is talking complete drivel.'" How a sane man could have written something as […] as this, he couldn't understand. And then, he said, gradually as he thought himself into the little world of Helmont, he suddenly started finding that the whole world of the history of medicine, which orthodox medicine had been […]. Incidentally, of course, Thomas Kuhn, whom I would met many years later said something similar to me in his autobiography that the thing which certainly we can see that there are different ways of looking at the world in terms of natural philosophy. When he had to read the works of Aristotle, he thought they were complete rubbish. “So how a sane man could talk like this?” And then gradually he sort of gets into that mental world and realizes that there are different ways of

conceiving the world. But this is what Pagel told me. Incidentally, something to mention. Pagel remained a great expert on the pathology of tuberculosis. The standard textbook on tuberculosis in English, for the medical people to use, was written by him, and went through many, many editions, almost until the end of his life. So he remained a great expert on tuberculosis until the end of his life. And besides being historian of medicine, of course, he was also a clinician and writing on medicine. He always told me one of the great regrets of his life was he'd always that he would find a cure for or a vaccine which would prevent tuberculosis. But it was the wrong side to pursue. Eventually, it was […] drugs, […] penicillin later on, and things like this. But anyway, so tracing it to this idea that what appears as rubbish may be very important to the way of breaking the mold in which science had been proceeding or medicine may have been proceeding at a certain time. So the influence there would be […] compared to Merton. Merton's influences is about looking for not just the science of the period, but looking at the sociological context in which science develops. So that was what I got from Merton. From Pagel I got the idea that the perception that sometimes what appears as out of the way, utter rubbish, maybe is pointing to different ways of doing science. And this, of course, is one reason why very few want to come to study people like Paracelsus and Helmont. I remember Rupert Hall telling me that "I find it disturbing to read Paracelsus, I know that I must read it to understand the history of science and history of medicine, but I don't like it. Somebody else also said this to me. Well know historian of science, a man called Murtaf. He also told me I hate studying Paracelsus, Helmont and all these—we have to go through so much nonsense for a set of pearls in there [laughter]. So I think what I learned from Pagel-- This man was educated in the medicine of his time and the science of his time. So why is he writing like this? Why is he so--? [And so that's what I got out Pagel and-- And therefore, when I came to things like Newton and saw that he was dabbling in alchemy and these kind of things, it didn't disturb me. I felt it was a new challenge to-- I had never suspected to find this things in Newton, but I didn't turn my back on it. Well let's see if there's things in Van Helmond that might help me to make more sense of Newton than other people have.

R. Uchôa: Yeah. And how other scholars before you and Pagel had treated Paracelsus and Newton, how works on these figures were done before you two?

P. Rattansi: I think before Pagel started taking Paracelsus seriously, and so on. […] I suppose-- I'm not an expert on this, but I suppose most people who studied Paracelsus tried to make a sharp distinction between medically useful ideas, which arise from his work, and the rest, which was nonsense and so on, which you could neglect. So the tendency was to separate invaluable things which lead on to modern science, or came to be called-- and I know this is in this field the Whig approach to the history of science that just as many English historians treated the history of England as if it was all leading towards Whig constitution for England. And everybody had to separate people who are the goodies and who are the baddies?

The goodies are the people who were helping it to lead to this end result, and the baddies are the people who are-- well, then we say, "Well, everything that leads toward Newton and then to the other great figures, that was good. Anything that doesn't lead to it is bad." And then you-- this a approach we should be very suspicious of. So I think before Pagel's writing on his book on Paracelsus and the works on Helmont, people did tend to divide. Well, I'm afraid the tendency's not yet dead. Helmont is still being given an important place in school books and so on based on his [...] experiment, his third state of matter gaseous, and his discovery of the […] of the stomach. Helmont still tends to be treated […] by the toxicologists as the founder of the discipline. The founder of the discipline because he discovered ways of detoxifying things like detoxifying powerful metals and minerals and giving them to patients in a safe dosage and in a safe way. So he becomes the father of toxicology, but they ignore the rest of [laughter]. So I think this tendency is certainly not absent, people have now taken Helmont seriously in conventional histories of toxicology or specialized. But in a sense, misunderstanding, trying to mold his image into our ideal image of how a scientist ought to behave and how he ought to [laughter] make his discoveries by following the true method.

R. Uchôa: And what about Newton?

P. Rattansi: Newton, again, the-- yes. There the problem is even bigger because the amount of work he left behind him, on biblical chronology on matters of doctrine, the Trinitarian doctrine, for example. And the amount on the prophetical books of the Bible. Then, of course, we'd see this exclusive to science, he's working on alchemy. So the challenge, first of all, is the amount of stuff he's left behind, and actually, it wasn't a prophecy, but you can see what he was trying to do, because he says why I'm doing this work. But in alchemy, there is very little what you get from manuscripts, either were taken by or mostly copied by Newton. Your copies of books on alchemy by Newton, some of these differ from the published version. But he doesn't tell you what he's trying to do in alchemy. So you have to infer all this by studying, first of all, which people did he select when he was studying alchemy. What sort of experiment did he perform because he left behind some experimental notebooks. And these have also been studied. And then - sorry to blow my own trumpet, but I discovered this document which now has become quite famous. But I didn't get the credit for discovering it. But it's about the only document that I know where Newton tells you what he's trying to do in studying alchemy and so on. It's just simplistic to say that this gives you a clue, master clue, to the order of alchemy. I'm sure that he changed his ideas about alchemy throughout his life. But this one particularly says, and this was written post Primcipia, we can establish by handwriting analysis. But he is thinking in terms of there are laws of nature which can be discovered through scientific progress through studying phenomenon, observation, generalizing from it and so on. But there are also active principles in nature, like he said, "I have discovered the goals relating to one of these active

principles which is gravitation." And there are other principles of whose laws we don't understand at the moment, but we presumably would be - I mean, we get laws relating to vegetation. So the world, particularly of chemistry and biology is dominated by the phenomenon of vegetation, which he also defines as fermentation. There must be other laws of nature. There are other presumably in the model of these laws of motion plus the law of gravitation. There must be other laws which are probably mathematical formulation in the end when we […]. Now, this is what he thinks of, and this is where alchemy comes in because it gives you a knowledge of what is happening in these fields. So this seemed to be very relevant in trying to understand why is the alchemy very important. But I don't want to say that this is attitude he had all throughout his life. This is post Principia. So the work is still there, and there is a great deal of work to be done.

R. Uchôa: But this is a picture of Newton that is after your work with McGuire, so how he was treated by other historians before your work in the 60s, and how does that relate to the existence of some papers and the manuscript of him that were well known by that time?

P. Rattansi: I think that there were a lot of people around who all the time were saying that today we study Newton as a man who is the scientist or the natural philosopher. Sorry, we study him as the scientist. But we neglect the whole amount. When you think of how many years of his life he devoted to the study of alchemy, to the study of Biblical prophecy, surely there must be some kind of connection between these two aspects of Newton. And we don't know to what extent, the fact that he pursued these other kind of - well, seemingly to us, the kind of study that respectable man of science should not have […]. He should have spent all his time in science. Why was he wasting his time on the other thing? I mean, people often told me that if I wanted to study Newton I should study his mathematics and his mechanics and his optics. Why am I wasting time on all this other side of Newton? In the same way, they can make the same thing about Newton. Why did he waste his time on all that? And of course, when one is talking of various images of Newton, people have to apologize. People like the people who wanted to make him one of the great fathers of the Enlightenment, Voltaire and the encyclopedists. They had to explain why Newton had-- they didn't know the full extent to which Newton was immersed in this kind of writing, this kind of work. But from what little they knew, it was embarrassing. It didn't seem very compatible with the image they wanted to create of Newton. So they said, "Well, his spare time is used to unraveling puzzles. And what greater puzzles than Trinitarian idea or the prophetical books of the Bible. What do they really mean? The biblical chronology. So he's kind of employing a-- he's having some fun in the evening after the hard work on mechanics and mathematics. He wants some fun with these kind of diversion." Sorry, I've forgotten the question.

R. Uchôa: Well, you were talking about the 18th century reading of Newton but my question was really, it's related to that but it's, in the 20th century how historians were working on Newton before your work on his alchemical papers, theological paper, how other like Boris Hessen, for example – supported a Marxist view of Newton - and others that you know very well.

P. Rattansi: I mean, the only person who gave kind of a dissenting note and which was resented by historians of science was given by John Keynes, the famous economist, who was bursar of King's and who was also a very rich man and he spent some of his money buying up some of the works of Newton on alchemy and the biblical prophecy. And this is the collection which I was asked to study at King's College, Cambridge. So he accumulated this thing and he also studied some of these documents and he wrote a very famous-- he was supposed to give a lecture for the […] centenary celebrations of Newton, which were postponed due to the war, which was to be given at Cambridge but which got postponed. Sorry, at London, at the Royal Society. And he had already drafted a lecture which he wanted to give there. He, unfortunately, died before he could deliver the lecture. The lecture was read out from his manuscript by his brother, distinguished bibliographer, distinguished medical man, Geoffrey Keynes. And this is a famous document because it ends with the famous words, if I'm quoting them accurately, "Newton was not the first of the scientists, he was the last of the magicians." Much more than that in there. And this rankled for a lot of historians of science for a long time. This man, basing himself on a partial reading of some of the alchemical works, he can make a judgment like this about Newton who we regard as one of the greatest men of science of the ages and it rankled with a lot of historians of science. Now, amongst them, you said, "How did they usually treat the alchemical thing"? There were some papers written by a student of alchemy and chemistry called-- I forgot the name. An English person. And he started making us-- he'd made a start on how we could see the alchemy which Newton was doing related to his science and so on. And he had made a start with it. Yes. This is a man called F. Sherwood Taylor who was one of the, I think, founders of Ambix as a journal for the study of the history of alchemy and early chemistry. Sherwood Taylor wrote a couple of articles on this, how you might be able to tackle the great mass of Newton's alchemical papers, but he died before he could proceed very further on it. And the next important paper to be published on these ones was by the Halls. Rupert Hall, a famous British historian of scientific revolution, and his wife, American, who wrote on the mechanical philosophy in England. Marie Boas, later Marie Boas Hall, got married to Rupert Hall. They published a paper early after the-- I think it must be some time in the 1960s, something like in late 1950s, which is based on a study of the Newton alchemical papers. They called the paper "Newton's Alchemical Studies". They said they've looked through all these papers and they find that it's-- I mean, Newton is trying to get some clue as to how you could study the nature of matter and so on scientifically. And he's trying to see what help the study of alchemy could-- what

light it would cast on it. And they say there are resemblances-- well, sorry, they pick out two or three points on which Newton's work-- where you can see what Newton is trying to get at. I'm sorry I can't remember it in great detail, but I wrote a paper which after the studying the alchemical papers at King's College, Cambridge, which I called Newton's Alchemical Studies to differentiate myself from the previous Newton's chemical period. I'm calling them alchemical period. And I tried to show that-- I praised about it first. I said that they've looked through all the thing, I mean when you-- but what they see as things of chemical interest I think which also are alchemical echoes. And so to say that we must call this thing chemistry, and it's not really alchemy, it seems to me not quite valid. And it was in that paper that I also gave the first hint that there are these-- that Newton actually wrote a manuscript on vegetation in the three kingdoms of nature, where, as I can see it, he's telling you why alchemy is important. So it is in that paper that I first drew attention to this paper which I think is the only one where Newton tries to explain to you post-Principia why the study of alchemy is important and why he is studying alchemy.

R. Uchôa: When it was published?

P. Rattansi: The paper was given-- I mean, I had started writing the paper and just-- and I was supposed to leave for Princeton. And just as I was about to leave for Princeton, the the librarian had taken Cambridge was a very famous bibliographer. In fact, the man who had prepared-- who used to work for Sotheby's and had prepared-- they had to prepare the catalog of the great Newton sale at Sotheby's of 1937, '36, '37. He was then the librarian at King's? and he came to my room and he said, "I believe you are leaving soon for Princeton." I said, "Yes." He said, "I have bought a very interesting collection of photo prints of some manuscripts of Newton which were absent from-- which we which were sold at Sotheby's and bought by an American, which are now in a library called the [Bundy?] Library. And people know very little about this manuscript. I've brought some photocopies for King's." So I said, "Well, I'd like to look at them." So he left them with me and it was going through them that I came across this [on vegetation?]. And I was astounded. A person who did much more work than myself on the alchemy of Newton was, of course, Betty Dobbs. And I remember meeting her years later and she said to me-- she did give me credit. She wrote a paper in Isis saying that I had drawn attention to this unknown paper. She said, "What did you feel when you discovered this paper?" And I said, "It was like discovering the Rosetta Stone. At last, I know why Newton is doing alchemy." But anyway, so I left soon after that for Princeton and Thomas Kuhn invited me to present a lecture in history of science in their annual meeting. They have an annual meeting where younger students of promise are supposed to present their papers. And he said, "Would you present a paper on your study of Newton's alchemy?" So in this paper, which later is published as Newton's alchemical studies in a *festschrift* for Walter Pagel, that is where I said that I'd discovered this new document. But that is where I analyzed the whole paper and

said, "The paper is valued but they are making a mistake in saying this is about chemistry, this is not about alchemy. It's about the very fundamental kind of concepts and things which have been-- which are very much supposed to be tackled in the field of alchemy." And I also then threw in this saying, "I can now show that Newton does think like an alchemist. Or why does he think alchemy is important?" So I summarized the findings in this document on vegetation by Newton. But I met, actually, Bett Doobs attended, she was still a-- she was a mature student, but she was a student of, I think, Siegfried. And he told me, he said, "This is my student who is also studying Newton's alchemical papers," and that's where I first met her. So there were people then beginning to go to it. And of course, she's been very kind to me. In her first book on Newton's alchemy, she says that she got inspiration from two papers particularly, pursuing this work and also into how she should go about. One of them is-- and she says, "Both papers are published in Pagel *festschrift*. One is a paper by Westfall." Analyzing how many years did Newton spend on alchemy and so on, and how much of the work for me to really-- and making some suggestions. And my own paper, and she said, "These inspired me".

R. Uchôa: And how was the response to the paper that you present? This new Newton--

P. Rattansi: First, it was I think, the mere effect that I really had published it in the […] which I get the feeling that the publisher of the thing was not really well-known publisher. The Pagel *festschrift* very few people have copies of it. I mean, we don't hear, […] have a copy of this *festschrift*. They tried to get copies but it's very difficult. So I don't think that it really makes a concection it's my fault for not generating some other people, given the discovery of this paper get much more publicity and what kind of conclusion we can derive from it about Newton's views on alchemy at this particular time.

R. Uchôa: All right. Going back to Keynes, how did he got interested in those papers and why did he buy those?

P. Rattansi: I think it was a kind of natural thing to-- after writing the Newton and the Pipes of Pan I did in the-- I did throw in the paragraph saying, maybe what we discovered in writing this paper can make new sense why Newton wasn't just in alchemy. Just as Newton says, "There is a kind of parallelism in here, in three feet." Newton says, "Why should we study prophecy? Not because we are unable to predict the future." It's only in retrospect, I mean, God has righted us with the prophetical books of the Bible, written in very enigmatical, symbolical language. Not so that you can predict the future but after the events predicted had taken place, then we can go back to what is happened and say, "Ah, so that symbol really signifies such and such an empire." Or person, or kingdom or whatever, and so it's only retrospectively that by hard work we can trace the true meaning of the

prophecy. So okay, let's say […] prophecy or prophetical books of the Bible. What is the point of giving us an idea of-- what is the purpose of telling us about the harmony of the spheres I mean, this is of course as Newton believes that Jewish knowledge has come down to the Greeks. They believe in the harmony of the spheres, and again, what does this phrase harmony of the spheres mean? Now the Greeks misinterpreted, people like Aristotle. They said harmony of the spheres means that the planets are all included within spheres and they rotated by friction into one and the other sphere until you get the outermost sphere. And then, of course, he also tries to show that it must be geoesthetics system because that's what it means. But the true meaning of the spheres, it was really about musical translate and harmony relate to musical sphere. So then you discovered the true meaning of harmony in music of the depth of the […] [string?] related to the […] limits, and who discovered this kind of laws of reasoning, but how you get an inverse-square law out of it? So then you apply the inverse-square law to the astronomical dimensions. And then you get the law of universal gravitation out of it. What the harmony of the spheres means a harmony of the orbits that the planets describe and so on. And these are ruled by particular mathematical principles, which also obtains in music. It obtains also in understanding astronomy. This is why astronomy and music are thought of as sister sciences in Greek. Sorry, I don't know what we were exactly discussing, but-- right. Sorry, how is this related to-- okay. Prophetical books. The meaning will become clear in retrospect. In the case of the harmony of the spheres, the meaning will become clear when we have made discoveries like the law of gravitation, which now enabled us-- you see, Newton had already discovered the law of gravitation not by meditating on these-- what does the harmony of the spheres means? He discovered it by hard work. No hypotheses here. By hard work, observation, experiment, hard thinking. So then he can go back to Pythagoras, and say, "Pythagoras must have known the law of gravitation." So all of these are showing that God has a plan in which He allows things to happen at a particular time. Okay? What does alchemy show us? Alchemy contains secrets about how God works not in astronomical field, but in understanding vegetation, which means all biological, botanical, life sciences phenomena for us. And, again, it's when we have recovered the knowledge of what really happens in nature, you've understood the phenomena of fermentation and so on. When you have discovered all of this, and in doing alchemy, you are also discovering the-- you are discovering things that have an […] surrounding us in inorganic kind of chemical sphere. When you understood the principles which underlay this, then you will be able to go back. Of course, that field has not come yet. But, in principle, then something similar must be very near. In alchemy, you discover by hard experiment its own secrets about nature, about this realm of nature. Then, when you have discovered these laws which rule phenomena in that world, then you'll be able to back. And that time, presumably, has not come yet. But this gives a meaning of what doing alchemy means for him. [00:22:40]

R. Uchôa: So, yeah, you—

P. Rattansi: Sorry. One other parallel to stress. Just as the prophetical books are wrapped in this enigmatical, symbolical language, so is true knowledge of the true natural system of the world wrapped up in this like harmony of the spheres and music of the spheres. But, similarly, in alchemy, it's all wrapped up in all these words there about the colors of the peacock and the sea and [the Great Work is performed?], and all this kind of-- so the analogy is made more sort of plausible, if you will. Precise.

R. Uchôa: Yeah. If I'm wrong, just tell me, but you get this view of Newton from the papers that you studied at Cambridge the one that Keynes bought to there.

P. Rattansi: Yes.

R. Uchôa: My question is, on Keynes, how did he got interested in the papers of Newton and why didn't he buy the papers to Cambridge?

P. Rattansi: So, I think that's a difficult question for me to answer. But it's actually was Munby, because Munby was the-- he was the cataloguer at Sotheby's, which was arranging the auction of the Biblical manuscripts, and so he studied them himself. And he knew Keynes very well. I mean, he could give […] of Keynes and so on, because Keynes brought him to Cambridge. It's almost like buying-- The whole package. --the papers and also bringing the catalog with you. But, of course, Munby, in his own right, was quite a scholar, and a […], and so on, and very amusing in wonderful conversationalist and so on. I didn't realize until I examined some papers at Oxford with an academic who told me that he had been-- he said, "I was with Munby in the armed forces at Dunkirk. We were captured by the Germans just when the war started, and we were shipped from one prisoners' camp to another." And he said, "Munby's sort of optimism and everything helped us all to survive all those years." So I didn't realize that he'd spent most of the war in German POW camps, but very lively and very, very good speaker, very rich speaker. But unfortunately, I never asked him why did he become interested in these particular papers and the alchemical papers. There are some truths if you read the oration he gave on the posthumous […] lecture he gave. There are some hints but, from what I remember, he just says that, "It seemed to me [00:25:40] an act of disrespect to this great man Newton-- for us to say people can just go ahead and buy his collection. And we, as a nation, don't care too much for-- "I think he calls it an act of-- "I felt it was an act of filial piety on my part as if I'm a kind of son who is doing homage to my father, once great figure of the English scientific succession, that you are allowing his papers, Okay, they are now to allow them to be sold just like that, 20 million. Who has the money to buy them and scatter them all over the world, not in one collection? This

was an act of-- I did an act of filial piety by trying to collect as many gains as I could," Because they'd already been sold by the time—

R. Uchôa: Did you already know of the existence of those papers before he bought to Cambridge?

P. Rattansi: Yes, I did because through McGuire. He had been studying papers from the university library mostly, but he also knew about the Keynes papers.

R. Uchôa: And where was it before Keynes bought the papers?

P. Rattansi: The whole history has been written up. I think that one of the best accounts is in-- I think somebody else has now written it up, but, anyway, if you look at the biography of Newton written by this historian, Frank E. Manuel. Frank E. Manuel's book on the Portrait of Newton, it gives you a story of how the collection came to be dispersed. But if I remember correctly, somebody has recently written a book, "What Happened to the Newton Papers", and that perhaps gives a little more detail. It's a whole book just on that question. So it was in the-- it was-- sorry chronologically. Newton's papers pass on to his niece, Catherine Conduitt, she becomes, Catherine-- that's his niece who kept house for him in London. When Newton became master of the mint, he lived in various houses near German Street, near Piccadilly in London. And Catherine kept house for him. I will resist the temptation to tell you some jokes which Voltaire puts into his recollections of Newton. He says, "That when I came to England, I thought that Newton had been named master of the mint because England honors its greats so much. It's a […] [servant?]. Then I discovered that it's because the Chancellor of the Exchequer was getting out of sort of […] [zone?] with Newton's niece [laughter] and they lived in the same house and so on, and Newton must have been allowing this to happen in his own house. Anyway, this is […] French love of this kind of scandal [laughter]. So she keeps house for him and the papers pass to her and the Conduitt family. And Conduitt is somebody who also writes interesting reminiscence of Newton. So she marries into this family. This family's a noble, the family of the Earls of Portsmouth [or?] some state and they own the papers. I think the new books show that they made very good use of these papers to get lots more money and so on. But anyway, the papers are with them and in the 19th Century, then Earl of Portsmouth tells Cambridge University Library, "I would like to have you-- I want to give to you all the papers of Newton which are related to mathematics, mechanics, optics, and so on. But there are also papers which I don't want you to keep. I want you to return them." So Cambridge appoint a committee which has the task of separating out the gold from the dust [laughter]. I mean separate the mathematical, optical, all these papers, and separate that from the Biblical prophecy and Biblical doctrine and alchemy. In fact, the alchemic papers and the extent of them is for the first time

discovered when the committee examines them. They say, "There are a huge number of papers on alchemy. We don't know why Newton was dabbling in this field, but maybe he was trying to get some kind of systematic chemistry out of studying alchemy." That's more or less what they say when you look at the actual words the committee uses. So those papers are returned. Then they disappear from view. And then in 1936, '37, [suddenly?], the then Earl of Portsmouth is feeling that he has to pay huge death duties on the estate. When anybody dies, the heir dies, you have to pay death duties to the government. So he has got to sell something to get more money. And he says to the British Library that, "I will offer to you the whole of these Newton manuscripts if--" I think about £1 million, something like that. But at that time, the slump has been there, British Museum is short of money, they can't raise the money. So the whole thing is given to Sotheby's to auction and it's auctioned and some of thpaper we still don't know where they are, if they were destroyed by the owner […]. But now with, of course, Internet and everything, and the collection being made of all of Newton's papers gradually, researcher's have tremendous facilities at their disposal. The only thing lacking is [laughter] [...], how do you relate to all these things to—

R. Uchôa: Yeah. I think that's it, for now. I thank you again very much for your time talking about history of Newton and historiography, it's very exciting. Thank you very much.

P. Rattansi: Thank you very much. It's a big pleasure for me to go back and discuss all these issues.