doi: http://dx.doi.org/10.23925/1980-7651.2019v23;p23-30

Interview with Piyo Rattansi[1]

Part III (05/09/2016)[2]

R. Uchôa:[3] Hi, Professor Rattansi. It's a great pleasure to have you here again to this interview. Today, I want to explore the writing process of the paper "Newton and the pipes of pan". We now have 50 years since its first publication on the *Notes and Records of the Royal Society*. My first question would be how did it come the first ideas? How was the first conceptualization of the paper?

P. Rattansi: It would be to the best of my recollection.

R. Uchôa: Yes.

P. Rattansi: Although I have some papers which I found recently, some correspondence, which probably helps to recall how exactly we happened to ... how did the paper originate. I can remember this happened in late 1965 when the head of our department, Jerry Ravetz had gone to Utrecht because they wished to appoint a successor, too. A very famous historian of science, Hooykaas. He had retired and they were looking for somebody else to take his place. They had chosen Jerry Ravetz to be the head of Department of History of science, I should say it was really part of the philosophy department. There was a section called history and philosophy of science. Ravetz headed it. They so to speak headhunted him. He said that he had a young family and he would like to try out a period in Utrecht to see how he could fit in line with the-- He had already gone to Utrecht. By that time, the department ... and this […] a number of appointed two new lecturers. We had already. When I went to the department, the first two people there were Jerry Ravetz and Donald Caldwell, who was the head of the technologies section. Meanwhile, we had appointed two new lecturers. One was Lewis Kalsliver who specialized in the history of chemistry, 18th century chemistry. The other person was the philosopher of science, J.E. McGuire, or as we called him, Ted McGuire. It was when we use to ... I used to go and have lunch with Ted McGuire. I asked him during this time, I said, "What are you working on at the moment?" He said he was working on the two

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great unifiers would happen to come from […][4]. One was Isaac Newton and the other was James Clerk Maxwell. He said to me, "I have come across some puzzling manuscripts of Newton." I said, "Really? I haven't worked on it myself. Let me have a look. I maybe have some idea." He didn't make […] to send some manuscripts of Newton which he was working on. I was quite surprised to find that the kind of ideas Newton was expressing in these manuscripts seemed to have a link with the kind of ideas and intellectual currents which I had been concerned with during working on my thesis and later on while I was too working at the university as a member of the department at Leeds. That's how we got appraised. I said yesterday that these documents which were puzzling him would make more sense if you link it to this *Prisca theologia* tradition which I had been working on partly, on which a number of people at the Warburg Institute in London had written very interesting papers, notably D. P. Walker, Gerkins, as we used to call him. Just to get back to what was the idea that, I think, it was linked to the stuff I was working on. I found there, I thought I saw this idea of *Prisca theologia*, this idea that much of the important knowledge which we think we have, which people thought in the 17th century they'd discovered, was really rediscovery, was something of some truths which were known to some of the ancients, especially Greek antiquity. Perhaps, much further than Greek antiquity. […] We later found that German philosophers, historians of philosophy. The 18th and late 19th century had a special category for this kind of philosophy that they called the Mosatia Philosophy that we didn't know at that time. That was the suggestion I made, that there seems to be a link here with these ideas of ancient knowledge which was lost in time and we are rediscovering it now. So, he went and did some more work on these ideas of Newton and he found in the correspondence of Newton a very interesting letter, which was not written by Newton but Newton asked at that time a young colleague of his, a young protégé of his Nicolas Fatio de Duillier. Swiss. A young Swiss mathematician who was at Cambridge to whom Newton had entrusted the second edition of the Principia. He thought very highly of him. The letter is a letter to one of the great physicist at the time, Christiaan Huygens. Huygens had taken notice of Newton's intellectual development so to speak because when Newton published his first important paper, which marked his, if you like, sensational […] of the stage of […], the paper on light and colors. Huygens had some criticisms because a member of the Royal Society invited people to give their comments on Newton's first paper, which was read out […]. He did not actually come to […] his behalf. Huygens had been somewhat critical after he was himself putting forward a theory of light and colors. He was meaning to call it the wave theory of light, but it was the impulse theory of light. So he had some criticism, but after a while he didn't push that very much. He writes that he thinks Newton is of a somewhat ... he doesn't respond well to criticism. […] On the other hand, we saw that Newton himself had great respect for Christiaan

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

Huygens. Most of the time, we think of Newton as passing very severe judgments on his contemporaries, Robert Hooke and so on. But he seemed very appreciative of Christiaan Huygens as a most important contemporary that is in physics and mechanics and mathematics. Anyways, the letter is sent not by Newton himself but his protégé, Fatio de Duillier and saying that Newton has recently discovered that many of his discoveries, including the law of universal gravitation was well-known to some of the ancients but it was lost in time and now we are recovering this kind of knowledge. Huygens promptly wrote a reply, which he said, "Mr. Newton has been too nice, too kind to the ancient. They might have discovered some of the ancient […], but that it is not the Earth which is stationary and the sun goes around it, other planets, but rather that […] it's the sun which is at the center and the Earth and the planets rotated, revolved around it." He said that he was prepared to concede. Some of them may have known about that. But the idea that the old ... new kind of mechanics and the systems of the world could be rebuilt and you would be in a system which would then make our predictions much more accurate, would explain so many different phenomena accurately ... No. He said this is the unique achievement of Newton. Even in his own time, people trying to provide a mechanistic […] for planetary movements and so on. He mentions a particular person. […] He says, "All right. Some people succeeded in providing a mechanistic explanation, system […] of the world based on the idea that planets go around in circular paths around the Earth. Of course, Copernicus is a […], went to provided for elliptical orbits. […] said orbits, this is far more difficult mathematical achievement than Newton alone and he should be published now more of his mathematical discoveries ... ", suggest he rather than waste his time on trying to find […] on the ancients. This letter we found of course of great interest. Then we were able to find more evidence. The fact that Newton had written to […] mathematicians in Scotland, particularly […] Gregory.

R. Uchôa: Gregory.

P. Rattansi: Gregory who he invited to Cambridge stayed with him for several weeks I think. To him, what he chose is it was not at all perturbed by Christiaan Huygens' edition. He told Gregory that the second edition of the Principia, that then was supposed to be done by Fatio, it would contain annotations for the basic propositions of the novel as the propositions of Principia. In each case showing that this knowledge was well-known to some of the ancients. This should do us a ... I'm very serious about this. Then as I said, I found that all this connected up with the ... I've been doing, and profiting from the people I was working with at the Warburg on the *prisca theologia*. We've pursued these inquiries further. We found that there are lots of people in England who went to take these kind of ideas of the rediscovery. For example, the Cambridge Platonists. Except for the Cambridge Platonists thought it was Descartes who had been discovered the original system known to be ancients

and how this changes our ideas about the atomists and Epicurious and Lucretius and traditionally being thought of dangerous people. That's as far as--

R. Uchôa: Yes. About the material that you used to write, what was that? And how did you have access to the material?

P. Rattansi: It was very difficult for us to access this material. Mind you, we are talking of days when there was no internet so that you could get access to books or manuscripts and so the manuscripts had to be photocopied at Cambridge's library and then brought back to Leeds. Even the kind of books we needed, we had to make trips to the British Library or collections of that sort. Fortunately, Leeds had a very good collection of historical sources, 17th century sources. Histories that one of the […] of Leeds had been a fellow of Wolfson College in Oxford. He proceeded then to partly duplicate copies of some of books on the history of science from the 17th century. So this is from the Old Souls Collection, Library. It's an university library. So the sources were pretty accessible to us. We would look up the Cambridge papers and most of the books in Latin… So it was mainly probably. It was just trying to link it up with these sources […]. How did we-- So we started writing a paper based on it. Ideas were taking shape. We shared the writing of the paper between us and McGuire took the task of […] giving it an exposition of the particular propositions of the principia that he wanted to annotate by showing that ancients knew about this. I undertook the task of trying to relate it to the 17th century intellectual milieu. So I wrote the sections on these things. We also got a great deal of help from Jerry Ravetz. I thought he was going to manuscripts and so ideas on how now to frame all. Why. It took this kind of enterprise at all.

R. Uchôa: All the scholia that you accessed, it was included in the material you both were working on? Or you went to another library to find out more material? I mean, I know that ... I'm not sure about the year, but Keynes had bought the whole material from to Cambridge. So how--

P. Rattansi: I think much of the material we used because McGuire was working on the second scientific manuscript of Newton, which are available at the university library. So I don't think he'd done much work on the material at King's College. I think it had both together meant to be Cambridge to look at the Kings ... so some of the material had come from there. Most of the material is really ... Because when the manuscripts that were owned by the Dukes of Portsmouth were ... one of the Dukes of Portsmouth sent the whole lot of the material which he possessed to Cambridge University in the 19th Century, but he sent material which is the scientific part of the manuscripts, but return the part which is not. So the manuscripts of alchemy and so on, both, they all went back. But papers which were relevant to the revision of the Principia were counted as one of the scientific papers. So they kept it, the university library. If he was thinking of revising it and giving the

second edition to all the documents, which of course would include these Prisca manuscripts. So McGuire had been able to copy those ones. He already had them. It was not. But we hadn't made much use of the Keynes manuscripts at that point.

R. Uchôa: But you did afterwards?

P. Rattansi: Yes. We authored both in our own ways. He continued to work on Newton, more of the philosophical side on Newton. After the publication of the paper in which we talked about the reception, I was invited by Kings College to take a research fellowship there and to work on the alchemical and other papers and see how we fit it into our idea of Newton. So I continued to work on that.

R. Uchôa: Throughout the process-- You said that you started thinking on the subject of the paper in 1965. Right?

P. Rattansi: In 1965. Yes.

R. Uchôa: Then you published one year after that.

P. Rattansi: That's right.

R. Uchôa: December, 1966.

P. Rattansi: I have to check the correspondence to because we can get the exact date on this to the moment.

R. Uchôa: Then about the difficulties that you had, what was the main difficulties that you had during the writing?

P. Rattansi: While we were writing it, we had an invitation from the Warburg Institute from Miss Yates, Francis Yates who was the then president who worked at the Warburg Institute. They had already made contact with Dr. Walker and invited him to come and give a paper to us. When he went back, we told him that we were working on this paper on Newton now. When he went back, he mentioned this to Francis Yates and she said, "Why don't we invite them over to a session on hermeticism and how it influenced the workings of the sciences?" So I believe it was ... and of course Jerry Ravetz also was invited to the Warburg Institute to attend a session at the last of the afternoon. We were supposed to read our paper and then proceed to read a paper on hermeticism and its. She had recently at that time published a work on Bruno, showing that Bruno was not a martyr for Copernicanism. He was more of a martyr for a kind of idea of hermetic religion of the sun in which the Copernican cosmology was also contained, important to the sun. It was he who planned to restore the, what is now, Hermetic religion of the world in place of Christianity. This was where he was burned at the stake.

So two papers we presented. One would be given by one of us, on our work on Newton and the second would be given by Miss Yates. They would be my greatest schedule. It would be chaired by the then director of the Warburg Institute, Ernst Gombrich, a good art historian. So the day came. We went to the train from Leeds to the Institute in London. Between us, they said I should give the paper. We went there, and had a very nice welcome. Then I started giving my paper.

Oh yes. Sorry. I should mention one of the guests was my friend and Walter Pagel. Walter Pagel very seldom attended any events of this sort. He had suffered from TB, tuberculosis as a young man. He found difficulty in walking and so on. But he did us the honor of coming to the Warburg to attend the lecture. So I spoke for about 45 minutes or so. The people who commented, Pagel commented very well and said something like, "This shows that real historians could do work on this material. They can come up with ideas that weren't really thought of before." There was also ... I don't think Gombrich himself commented on the paper, but the head at that time, the Warburg Institute had a historian of science working there. This was a man called […] is now working in Egypt. He had worked with Poper, written a thesis which was published as Tales of Optics: from Descartes to Newton. So he was a well-known figure. I can't remember what his comment was, but he was […] the paper. Anyway, after lunch, then Miss Yates spoke. She gave a talk on Bruno. So, at the Warburg, we were well received and encouraged to persist to go on with writing the paper and publishing it as soon as we could.

R. Uchôa: Yeah. It was a reception of the article before publishing it. Now if we come to the question, how was the reception afterwards, after publishing it, how would you recollect the whole thing, how did people respond to your paper?

P. Rattansi: Now, why was the paper published in the General Notes and Records of the Royal Society of London? McGuire had met at some point the person who edited at that time the history of science journal for the Royal Society. His name was Herald Hartley. He was a distinguished chemist who had been done contribution to ... I forget whether […]. I think Ted McGuire mentioned the fact that we work on ... we prepared a paper on Newton which maybe it should be of interest to the Royal Society. He said, "All right. Send it to me and I will get it and publish it." It was not a free system at that time. It was not free. It was Harold Hartley's say so. The paper went. We never saw the proofs. There was a misprint between […] and the judgment and the proof. Sometime people said […]. […] But anyway. He went and published it. So I think he published in November of 1966. I should mention that fact that while I was working on the paper, I had an invitation to go and visit the University of Chicago as a visiting associate professor. This was when my friend, Allen Debus ... because I was in the middle of writing the paper, my sections of the paper. Meanwhile, I met a girl and proposed to her and got married. So we went to University of Chicago together. A honeymoon was expected. But of course I was

preparing the paper as quickly as I could. McGuire came over to Chicago and urged me. By the time I returned from Chicago to Leeds. Jerry Ravet then had a look at it and sent us some improvements which I think added greatly to the paper, one consistent whole. So it was sent on by McGuire to Sir Harold Hartley. He said, "Harold will publish it soon." You were asking what was the initial perception?

R. Uchôa: Yeah. Yeah.

P. Rattansi: I think for a while, we didn't hear anything about the paper. I can't recall very much else about it. There is this ... at the end of the year, for it to publish in November, it would have gone ... the journal called ISIS that every year publishes a volume of ISIS which lists all the papers published in history and philosophy of science and technology and medicine. I remember McGuire looked at it and he said to me, showed it to me, and he says, "There is in a paper in the comments of the index of it." It just said two words: Prisca, among commas, notions ... Maybe in the index, it was also baffled by […] like this. But that's all I can remember about the initial reaction.

On the other hand, the paper took us to various places because at King's College, Cambridge for example, eventually they made an offer to me saying that this paper shows that you may be in a good position to be able to make sense of the manuscripts which we have now a collection which comprises his biblical studies on the biblical chronology, on the Trinitarian, on the dogma of the Trinity, of Newton's criticism of it. He's quite succeeded and seen. How does this fit into the whole of Newton? Maybe you are a good person to be able to do that. So they offered me a four year fellowship there. McGuire was offered a chair at the University--

R. Uchôa: Was it in Canada?

P. Rattansi: No, no. It was in the States.

R. Uchôa: The United States.

P. Rattansi: Yep.

R. Uchôa: Yeah. Do you know if— I mean, you are talking about people in the history of science community, but do you know if the paper had some impact on the scientific community, people from outside history of science whether it be scientists or philosophers or other scholars? Even people from literature that your work had some-- you know-- Did you notice the reception of people of other areas of science?

P. Rattansi: Yes. I think we began to get the impression that although the older generation of historians of science either dismissed it as, "Okay. Newton is in trouble because he has to testify." Particularly the idea of gravitational attraction to

people who have been brought up on a Cartesian-- From now on, all explanations must be reducing them to contact action and pressures, something of that sort. In other words, a matter of physical cause a sign for the gravitation. Until Newton produces a physical explanation of universal gravitation. All he's presented is a mathematical treatise. Now that our paper had shown that a lot of people accepted the idea of pristine knowledge, that explains why he chose this particular apologetic tactic. But it's just a tactic. What other […] the rest of his work? So why is it so important? It's important showing of Newton when he's in trouble. He goes back to ideas of this sort. But did he tell any other? Now that is not quite fair to the paper because in the paper, I think especially […], he said that you should make the point that this is not a statement of curiosity which has no real relevance to Newton's own scientific work. Newton latter made the point. He was out to make the point that there are all sorts of questions where induction or hypothetical deductive and things, method, aren't helping, that the nature of space and time and nature of all sorts of issues like this, this kind of material may have-- inclined this-- It may help to explain perhaps even why such an idea as attraction-- It was easier for Newton to offer this idea and make a whole system on it than for lots of other people around. So it is not really a curiosity tactic that Newton uses in his difficulty. It may in a sense explain why it was him who could use attraction as the basis of a system like this. I did when I started working at the alchemy also, the idea that somebody who already had one foot in this kind of camp, maybe it was easier for him to build the whole system of gravitational attraction. So it was a constitutive element of what we were thinking. It's not really a curiosity which is introduced as a defensive tactic.

R. Uchôa: Okay. So that was a response that it got, that people were-- You say that's not really important on Newton's thinking on science. He's just…

P. Rattansi: Yes. I think the younger people were more receptive and the people may have…were taken more serious by them than the older generation. That's a generational attitude towards it.

R. Uchôa: Yeah. I think we have come through some of the main questions on the publication, of the writing process, and the reception of work. Again, I thank you very much for spending your time answering those questions. I think that's it.

P. Rattansi: Thank you very much.