Ferruccio Ritossa – Ricordo di Adriano Buzzati-Traverso

Well the story started when I have finished the university and arrived in Pavia.

I have always been a sap. But when I arrive there at the Genetics Institute, I immediately realize I had just read obsolete books.

It is the beginning of 1960.

I am one of the winners of a fellowship to follow a course on the biological action of radiations.

The organizer is Adriano Buzzati-Traverso. The course is just a trick to get money to teach modern Genetics.

Before, in my prestigious University I heard just once the word deoxyribonucleic acid.

And it was not DNA or AND, but desoxyribonucleic acid.

A kind of "mysterious something" which pushed students more to suspect than to study.

And to think that Genetics had always been my passion and you do not know what I did to know and study!

In Pavia they pick up us, give us courses of advanced Mats, English, Biochemistry, Atomic Physics, Enzyme Kinetics, Radioisotopes, Cybernetics, Statistics. And my beloved Genetics: Formal, Bacterial, Phage, Fungal, Human, Vegetal, Population, Biochemical Genetics.

The Staff is also terrific:

Magni, Cavalli-Sforza, Calef, Bianchi, Boeri, Ageno, Fraccaro, Ciferri, and then Lederberg, Bob Perry, Von Borstel, Giles, Caianiello, Cin Cing Li, Bodmer and others.

But Buzzati gave us more, much more.

Generally, in teaching, he used to seat on a window sill, in his shirt sleeves or in his swater.

No ties or jackets to which we were used at the University.

Research was a respectful relation of a deepening between man and nature, and man was constantly part of that nature in which he was searching.

There was no element of priority or success.

Scientists for Buzzati were somehow different from other men (I did not approve that much, but I was anyway fascinated).

Scientists for him were a kind noble beings working just to open new possibilities of comprehension and andvancement. Once, I remember, Buzzati addressed to us as young scientists.

It was the top.

At the same time I felt destroyed by my ignorance, impressed by all these news, stimulated by the possibilities.

As a reaction I started studying everything again; everything I could.

This time, however, on the proper track: up to date journals, recent books.

A fascinating booklet I read at that time was "Bioenergetics" by Albert St. Georgy (written in '57).

Mitchell P. proton pumps were only about to come at that time.

In his book St. Georgy postulated that electron flow along the respiratory chain could lead to ATP synthesis by resonance.

He postulated that this was possible if exited electrons underwent spin reversal, thus lengthening the life of their excited state.

This was possible, he claimed, if water embedding proteins of the chain had not a melted but an ice structure.

Indeed, UV irradiated flavins in melted water are fluorescent, while when irradiated in ice they become phosphorescent. These were the evidence he gave in that book.

During the second year of Buzzati course I choose to work with DROSOFILA. I did this choice because I felt DROSOFILA was some how in between bacteria and man.

I choose DROSOFILA also because there were no facilities to work with plants which I would have preferred since I am agronomist.

Buzzati, obviously, approved my choice. But belive me, it was not an easy choice. Since working with Drosofila were a kind of subpopulation of scientists. The top were T4 people.

I set up myself tritium autoradiography and started to study which kind of nucleic acid was synthetized at salivary gland puffs.

There was some confusion on the point in 1960.

Beerman and Pellig had just shown RNA synthesis in *Chironomus* but Pavan had shown DNA in *Ricosciara*. Messenger RNA was just about to come when I started.

By the way, I like to remember the emotion when the Brenner, Jacob and Meselson paper arrived

We spent nights to discuss it and to imagine the future.

To make it short, I do not know if it was John Pulitzer or Inge or Clara Ghini or Giordano to shift the temperature of my incubator, or if it was just the old age of that sweet machine, but once I found a different puffing pattern.

I worked out conditions for this shift; observed new RNA synthesis; same puffs in malpighians and gut polytene chromosomes, shut off previous puffs. But I was not happy.

Then I thought heat might destroy ice structure of water around proteins of the electron, transporting chain thus uncoupling oxidative phosphorilation.

When I observed that uncouplers like dinitrofenol and salicilate, as well as recovery from anoxia led to exactly the same new gene activation , then I was really happy.

It does not matter if true or false but a working link between imagination and reality, like love.

Inge Rasmussen, working with triploids in Drosofila, Giovanni Magni working with yeasts and Adriano Buzzati himself, leader, helped me in discussing and writing.

None wanted his name on the paper. This is one of the beauties of the tale that was for me Pavia.

As you probably know the paper was at first rejected (by Nature?) because unimportant.

It was the beginning of a common practice in my scientific career.

I swear, I never seriously suffered for that rejection, neither for the several partial or total failures of my effort to go deeper into this matter.

I swear, not only I did not suffer, but, at least after a certain time, I started to enjoy, and particularly, for the same reason, I enjoy now that I see you growing and making all these beautiful discoveries which I was notable of imagining. I am very happy because, in so doing, I feel a professional