

Nicola Cabibbo: Some Personal Reminiscences

In 1966, an unknown 25 year old Indian physicist teaching in Boston arrived at a summer school in particle physics in Erice. You can right away gauge the naivety with which he was naturally blessed with when I tell you that he thought Erice was close to Rome. [True, no Google maps in those days but still, Erice is in Sicily].

Very surprisingly, he won the Best student award from a committee of icons: Murray Gell-Mann, Sheldon Glashow, Sidney Coleman, Bruno Zumino, Nicola Cabibbo and Raoul Gatto. Given so many more talented young Italian theoretical physicists attending it [I shall withhold the names to avoid embarrassment; but if you cross your heart and promise never to tell more than a few people, I shall whisper their names in your ear], you may justly ask *why* of this choice and *how* were they able to convince the Director of the school Antonino Zichichi.

At the school, in the morning there were lectures, then a break for lunch and beach and afterwards we students had to submit questions for the speakers who would answer them during the later evening sessions.

Nicola was presenting some novel results from his recent paper with Yuval Ne'eman using current algebra boldly applied to compute the Regge intercepts. The naive Indian asked Nicola an innocent question: why would the current algebra result derived on the mass shell with physical angular momenta maintain its integrity to provide a reliable result for zero momentum transfer? In his typical softly enigmatic fashion, Nicola told me that it was a very good question, explained the subtleties of the matter and yes indeed it was a big extrapolation. I was immensely grateful by his graceful answer and for his intellectual honesty.

By contrast, we received a strikingly brash response from Murray Gell-Mann. *il numero uno*. MGM was describing his theory of the algebra of currents and a British student -more timid than I- gathered enough courage to ask him about the Schwinger terms which at that time appeared an anathema to that whole scheme. Instead of admitting that it was a potential problem, MGM became furious and insulting, reducing that student to tears. A few of us got very incensed about it because we somehow felt it as a collective insult. It is well to remember that we were all rather young. I had spent a heady year with the Free Speech Movement [a precursor of the later 1968 movement in Europe] at Berkeley and other campuses in California. After a sleepless night devoted to the sole purpose of an indirect "Gandhian retaliation", I thought I was ready in the morning.

MGM had been describing some of the glories of current algebra through the Fubini superconvergence sum rules derived then from his theory. Since we did not have Fubini's paper, none of us was clear about what it said and we had been much relieved when Glashow admitted not to have understood how it was actually employed and MGM had explained it the day before in a typically condescending manner. Shaking to my roots, I asked MGM, was it not possible -as previously demonstrated in spinor QED- that if higher order terms were included, the sum rule might turn to an identity [$7=7$ is an identity and certainly true but not very useful; on the other hand if you derived the equation $x=7$ for a variable x whose value you did not know previously, you have accomplished something]. MGM angrily replied "who did the calculation, who who, who?" I had guessed it right: he was more interested in who had done it rather than what had actually been accomplished. Then I knew he had lost and that I could play the cat with him in a cat and mouse game because: it was a long series of papers which *he* had written with four other authors which I was referring to and which *he had forgotten about*. In my most innocent voice, I told him that those papers were written by "5 very strong people". At this point, Sidney Coleman joined in to enlighten him "Murray, he is referring to *your* paper with Goldberger, Low, Zachariasen and Singh" [all big names then apart from himself]. Now MGM had no choice. He told me, "Yours is a very intelligent remark". Apart from MGM, only Cabibbo and Coleman understood what I had done. I later found out that MGM, Cabibbo and Coleman convinced Zichichi to offer me the prize.

Towards the end of the school, Nicola asked me if I would like to visit Rome after the conference. I was overjoyed and of course I accepted. Gatto who had by then migrated from Rome to Florence

and had created a wonderful coterie of physicists there, asked me whether I would like to pass by Florence after my visit to Rome. Certain doors closed before were suddenly opening to me. During my visit to Rome, Nicola asked me if I would like to spend a semester in Rome the next year because he would like to learn more about the S-matrix theory. You can imagine my pleasant surprise about that offer. No doubt spurred by Cabibbo's faith in me, Gatto invited me to Florence for a month. My Italian Odyssey had begun.

March 15, 1967 I was back to Rome and to my great surprise Nicola came to pick me up at the Fiumicino airport. I did not realize then what a rare treat it was because I would learn only much later that Nicola hardly did such favours to anyone. Not only, but he had found a *pensione* near the University for me to live in. He then brought me to the department; being lunch time by then and since he himself had to return home, he put me in custody of a colleague, Jona-Lasinio, who would take me to lunch with other physicists of Rome. All of this was such unexpected honour delivered with extreme kindness I could hardly believe it.

The physics department then –as ever- was pressed for space and so I was told I could share a large hall of an office with 5 other young physicists. Outside this office, there was written "*stanza delle streghe*" which knowing hardly any Italian, I could not decipher [It means: The Witches Room], Upon entering I found 6 tables with 4 young women and I thought I had made a mistake [There were practically no women in physics in the US then]. I wanted to retreat with due haste but was politely informed by the young ladies -in some mixture of English and Italian- that yes I had been designated to occupy one of the desks; the last remaining to be filled by a North Vietnamese (male) visitor to the department.

Nicola must have sensed my discomfort at this arrangement and when he had to go to Geneva for a longish period, he was very kind in letting me occupy his office for the entire period of his absence from Rome. This act boosted my standing in Rome no end. Once, Touschek knocked at his door and when I opened, he asked me in his straightforward manner what I was doing in Nicola's office. That was my introduction to Touschek and which would, over the years blossom into a most pleasant personal friendship.

Before leaving for Geneva, Nicola asked me to give a series of ten lectures on S- matrix theory and about Geoffrey Chew's version of Regge poles. While waiting for Nicola to begin my first lecture, as we all smoked in that period, I asked for a light to a strikingly pretty and elegant blond young woman smoking in the first row. That was how I met Giulia Pancheri. She would later properly write and put together my lectures. Before leaving for Boston towards the end of the year to marry me, Giulia would give a big party to which Nicola and his wife would go to Lavinio, a beach town outside Rome.

Before leaving for Geneva, Nicola also gave me the difficult task of supervising theses of two very bright female students of his. The task was difficult not because of the physics but because I spoke no Italian and they hardly spoke any English. Luckily it all turned out well: both are now well known in their fields and we have remained friends.

We would return to Rome at least twice a year from Boston. Nicola would always arrange for me to give a lecture or two at Rome. Later, I am sure through his say so, I would become a regular lecturer at the doctoral program at Rome.

Over the ensuing years, I would meet Nicola off and on in the department and sometimes in his house, in Touschek's house and at one famous party when Sidney Drell was on a sabbatical in Rome. We all heard some juicy stuff there since Drell at that time had been called as an expert witness in a messy court case in California brought on by Oreste Piccioni against Emilio Segre regarding the discovery of the Antiproton and the Nobel prize awarded to Segre. At that party, Touschek declared that Nobel prizes should no longer be awarded as it brought considerable more grief to those who missed than the happiness it brought to the winners. That night, no one could have foreseen that only a few years hence, Touschek would not be given his share in a Nobel prize for having made the first colliding beam machine and that three decades later, Nicola would be denied his share in the Nobel prize in a shocking manner.

After Nicola assumed the Presidency of INFN, he became particularly busy. In 1984, when I was called as a Professor at Perugia, I had to meet him in his official capacity twice. Once to obtain funds for acquiring an old villa outside Perugia for local INFN conferences and meetings. Initially Nicola was not favourable to the idea but he granted it. Later, we went to him to ask money for updating our inadequate physics library and he was very sympathetic and provided enough money twice which made our library respectable. My esteem for him went up even more.

During this year, I had the good fortune of working with Luciano Maiani and Nicola Cabibbo on the fascinating subject of excited leptons [very heavy electrons, muons and taus]. Nicola had so little time left from administrative work that Luciano and I stood by an office one day until he could finish his committee meeting. Then we went out for lunch nearby where he looked over the draft of the paper we had prepared and made incisive comments and additions. In those days, papers had to be typed by a secretary and the three of us went to the lady for this purpose. Nicola used all his charm to convince her to make it her top priority of work. The reason for this insistence was that we knew another group was developing similar ideas and we wanted ours to get out as quickly as it could. It did and the rest is history. The idea [CMS, 1984 vintage] is still alive and a search for them is continuing on at the LHC in Geneva.

If you think I am only recounting your successes, let me mention something rather embarrassing for me. In the middle eighties, I had been working on the Quantum Hall Effect in Boston with my friends Allan Widom and Marvin Friedman. At a conference in Rome, where both Nicola and I were present, a young Italian physicist was discussing the QHE who gave all the credit to the MIT group. Incensed, I asked the speaker why he had not mentioned our work where the same results were derived and earlier than them. The young man was quite disturbed. At the dinner afterwards, I mentioned my displeasure to Nicola. His sole response to my lamentations was, "You know Yogi, that young man this evening reminded me very much the way you were at his age". Rendered me stunned and speechless.

During his presidency of INFN, notwithstanding the most strident criticisms raised against Giuliano Preparata, Nicola quietly continued to support Giuliano's research. Nicola's instinct was right as much of that research has now been verified by others and has paved new pathways for further development.

During the last two decades, Nicola came to listen to two of my lectures. Nicola had been a pioneer in analyzing the gravitational wave antenna experiments by the Rome-Frascati group and so he was very curious to know why we were modifying the theoretical expressions then in use. He drove to Frascati to hear me speak and asked me some very hard questions. Needless to say that our paper afterwards was much improved thanks to Nicola's remarks.

More recently, he came to listen to me on another controversial topic: the subject of nuclear reactions occurring in the Solar Corona and the Solar flares. He seemed quite impressed by the fact that we could provide a mechanism to accelerate the protons in these flare bursts to such high energies that the L3 group at CERN could detect secondary muons with energies averaging 45 GeV. Nicola asked us to look beyond the Solar flares, to study the exquisite tapestry created by the magnetic flux tubes and estimate the particle and energy spectrum therein. That is a hard problem and I hope that one day we shall fulfill my personal promise to Nicola about solving it. But already, here is a theoretical prediction: In 2013, the problem of Solar activity would become of truly global concern.

For a couple of physicists, Giulia and I were immensely fortunate: She found a mentor in Bruno Touschek and I found a mentor in Nicola Cabibbo. We could not ask for more.

God bless you Nicola.

Coda: I hope I shall be forgiven for speaking about myself so much. As Abdus Salam said on a similar occasion, when we celebrate an outstanding scientist, we celebrate ourselves. To achieve grace by reflection, shall we say.

Perugia October 12, 2010