Conclusive remarks

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I wish to thank the Physics Department, the colleagues of the Organizing Committee and Raffaello Diambrini. They made possible this celebration in memory of Giordano. The speakers presented us a full, vivid image of Giordano, of his research and personality.

Giordano was my professor in the Advanced Physics course whose owner, Marcello Conversi, was on leave, I think at CERN. It was then, with the clear lectures by Giordano and the beautiful book, which he had adopted, the book of W. S. C. Williams, that I learned the basics of elementary particle physics. Among them, the first application of symmetry to particle physics, the isotopic spin symmetry, a concept that fascinated me from the very beginning. And on the isotopic spin Giordano questioned me at the exam, much to my relief.

Gherardo Stoppini, experimental physicist in Frascati, held that year a part of Diambrini's lectures. He was very informal, sometimes arriving at a cigarette exchange with his listeners. He was talking to us of μ mesons and pions with a, very reassuring for us, *romanesco* accent. I learned a lot from Diambrini and Stoppini, they gave me an imprint that I have kept with me for long.

Giordano spent his best years in the golden age of post-war Italian physics. A period of hope and, above all, unity of intents. What I mean is that the physics community was then small and compact. You could discuss the particular aspects, but there was a common feeling and the idea that research in physics was one only, with shared objectives. And that everyone understood and could judge what others were doing.

After that, the fragmentation came, each cultivating his field regardless of what others are doing, as if each group had imploded in itself.

In more recent times, a more disturbing phenomenon has emerged: the proliferation of researches in fields, often not well defined, which place themselves in antithesis to the consolidated knowledge, and proceed without the scientific community submitting them to timely and satisfactory scientific scrutiny. The phenomenon occurs in various fields, famous is the "water memory", a phenomenon initially based on data officially recognized as having been manipulated, but that

surfaces periodically in a "parallel" literature, to arouse the interest even of a scientist of great depth as the Nobel Prize Luc Montagnier.

A few decades ago, a similar case arose with the so-called "cold fusion", the hypothetical observation of phenomena associated with the nuclear fusion of hydrogen and/or deuterium in a Palladium lattice subject to high electrical currents, by Stanley Pons and Martin Fleischmann [1].

The international scientific community immediately became interested in the phenomenon, but soon lost interest in the absence of unambiguous experimental evidence that would confirm the occurrence of a phenomenon, by itself difficult to reconcile with the known nuclear and atomic physics: the overcoming of the nuclear Coulomb barrier, known to occur only under the extreme pressure and temperature conditions which occur inside the stars or in the high temperature plasmas in the Tokamak [2]. Experiments and theories about cold fusion continued, nonetheless. To many, it seemed that the exploration of a phenomenon that was so important for its possible applications could present a sufficient interest to be pursued even if only in the so-called Edisonian Approach [3].

A sort of segregation from the general discussion has ensued of a group of "followers" who interact among themselves on the network/internet "as if" the existence of cold fusion was a fact well established and not recognized by "official" science for academic reasons, or for supposed links with business interests related to the production of energy from conventional sources.

A more recent, but similar, development are the so-called LENR (Low Energy Nuclear Reactions) where it is assumed that nuclear phenomena can be initiated by a supposed increase of the electron mass in solids, due to their interaction with the polarization modes of the crystal. This increase [4] would allow the capture of the electrons by hydrogen nuclei: $e + p \to \nu + n$, with the release of neutrons and the possible activation of nuclei present in the lattice, gamma emission and release of nuclear energy in a "cold" state. The needed mass renormalization, however, is very high: at least a factor 2.6 to exceed the threshold of the reaction and a factor 30-60 for reaching useful production rates. In fact, many people think unrealistic electron effective masses above one MeV up to several tens of MeV [5], and the experimental evidence does not seem so compelling for neutron actual production in solids subjected to high electrical currents.

LENR would represent, like cold fusion, a short circuit between atomic and nuclear physics, which in our textbooks are considered distinct realities, separated by too many orders of magnitudes in spatial dimensions (from angstrom to fermi) and energy (from eV to keV the first, MeV the second).

It is almost superfluous to underline how much these segregation phenomena are harmful to the credibility of the scientific community, which appears unable to resolve issues that would be of great potential interest, or at least to indicate

concrete procedures that could lead to the clarification of the problem. The lack of attention paid by most scientists to these issues may be justified by the skepticism with which you may look at the incursions of atomic physics in nuclear energy, but could fire back in terms of credibility and, last but not least, could lead to a dispersion of funds for research towards areas scientifically unjustified.

In recent times a massive protest of scientists from condensed matter physics has been directed against some areas of LENR, but I think that an experimental demonstration of the absence of the phenomenon would have been much more effective. Diverting time and funds from its own research to pursue the claims of others may seem unnecessary, even harmful, but the proliferation of attacks to the neglect by the official science of potentially revolutionary claims might be even worse.

Such cases have occurred in the past, for example, in the '700 the French Academy established not to consider anymore projects relating to the construction of perpetual motion machines.

A thorough and impartial consideration of the possibility or impossibility of field invasions from atomic to nuclear physics would be desirable, today, and would give a proof of what we all believe, that there is only one science and that disputes must be solved using the scientific method.

Fortunately for him, Diambrini lived in a time when there was no talk of "official science" as opposed to a hypothetical genuine and "free-range" science. I hope sincerely that we will return to science *tout court*, an activity in which personalisms and superficiality should be rigorously excluded.

References

[1] M. Fleischman, S. Pons, *Electrochemically Induced Nuclear Fusion Of Deuterium*, **J. Electroanal. Chem.**, Mar 1989.

[2] Even in Rome, we investigated the phenomenon, see L. Maiani, G. Parisi, L. Pietronero, *Cold Fusion: a first look*, presented to the *Workshop on Cold Fusion*, Erice, 12 Aprile, 1989.

[3] *The Edisonian Approach to innovation is characterized by trial and error discovery rather than a systematic theoretical approach* (Wikipedia). In the same article an interesting disclaimer is added: *This may be a convenient term, but it is an inaccurate and misleading description of the method of invention actually used by Thomas Edison.*

[4] A. Widom and L. Larsen, Eur. Phys. J. **C 46**, 107 (2006).

[5] S. Ciuchi et al. Eur. Phys. J. **C 72**, 2193 (2012).

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