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Setting Cold Fusion in context: a reply

Giuliano Preparata
Dipartimento di Fisica – Universitá di Milano
INFN – Sezione di Milano

Abstract

This talk consists of three parts: the first on the "pathological" nature of Cold Fusion (CF) phenomena, the second on a wide theoretical effort based on the new ideas of QED coherence in matter, and the third replying to explicit criticisms to my work.

The "pathological" Science of Cold Fusion

Six years after the momentous announcement of March 23, 1989 the world of Science is still divided: a large majority that regards Cold Fusion (CF) an eminent example of "pathological" science, a small battered minority that still carries on with very limited means, both financial and, alas, intellectual, struggling for the survival of the grandiose dream that Fleischmann and Pons (FP) brought to mankind that now distant spring day of 1989.

Why did it go this way? Why a phenomenology[1] that has found so many diverse confirmations by several different experimental groups, has failed to convince such a large number of scientists, that have turned their back on this fascinating field?

I believe we must try to give an honest answer to these important questions, for if we fail to understand why so many of our colleagues perceive CF as a pathological form of science, we are doomed to remain marginal even if we will able, as in fact we are doing, to make substantial progress.

It seems to me that the basic reason why CF is almost universally considered "pathological", is that it is so at odds with deeply rooted expectations based on the generally accepted picture of condensed matter and nuclear physics, that it requires the established scientist an almost heroic effort even

[1]Throughout this talk I shall confine myself to the Pd/D system pioneered by FP, i.e. I shall not attempt to overcome the "Pd/D barrier" of CF.

to bring himself to look into such a bizarre set of physical phenomena (the situation is of course different for the scientists that live somewhat at the margin of Academia, and this has been part of the problem of CF).

Without a well defined frame that can accomodate the strange looking facts of CF, indeed its "miracles", the natural reaction of scientists is at best to ignore those facts, at worst to believe them the result of some kind of fraud. And, of course, we have seen all that. But, instead of moralistically condemning such attitudes (which in truth have caused us no minor suffering), as I have just stressed, we should try to put ourselves in the shoes of the many colleagues with whom, before that fateful day of the spring of 1989, we had a perfectly reasonable kind of relationship.

Confronted with the experimental landscape of CF, what do they see? The following "monstruosities":

i) the deuterons, that are well known to crowd a Pd cathode in a heavy water electrolytic cell, and to accomodate in the octahedral sites of its lattice, are supposed to undergo nuclear fusion at rates hundreds of orders of magnitude higher than what one expects in the D_2-molecule, where the deuterons are located even closer.

ii) not only do the deuterons (supposedly) fuse, defying all known condensed matter physics, but they would do so following a nuclear physics path, that is completely at variance with the known path, for which the two reactions

$$D + D \rightarrow n\ (2.45\ \text{MeV})\ +^3 \text{He}\ (0.82\ \text{MeV}) \quad (1)$$

$$D + D \rightarrow p\ (3.02\ \text{MeV})\ +^3 \text{H}\ (1.01\ \text{MeV}) \quad (2)$$

have each a branching ratio close to 50%.

Countless papers, books, sermons etc. have been written and uttered to admonish us all about the senseleness of what the CF scientists pretended to have experimentally demonstrated, and how their refusal to repent and recant jeopardized, indeed discredited the whole scientific community. By the way, the tones and the practices of the keepers of scientific orthodoxy in the CF debate have been remarkably close to those of the critics and enemies (mostly churchmen) of the Renaissance science, who finally saw the better part of Bruno, Galilei and their followers. And indeed, in the present frame of understanding both condensed matter and nuclear physics, there is no way to reconcile the above two "monstruosities" with the huge body of knowledge that is based upon the dominating paradigm. Thus, let us make a little investment in philosophy (a most abhorred and derided intellectual enterprise by the normal scientist[2] of today) and try to characterize the deep roots of the paradigm, for without it not only are we

[2] I am using this term in the technical sense of T. Kuhn [1].

condemned to a permanent and futile warfare and misunderstanding with our colleagues, but we will also remain unaware of the remarkable revolution that the "monstruosities" of CF are bringing about in modern science.

If one wishes to characterize with a single concept the paradigm, I believe the best of all is : Asymptotic Freedom (AF). The concept of AF, as far as I know, was first explicitly discussed in 1973 [2] within the then consolidating Standard Model (SM) of particle physics: it denotes the property of a non-abelian gauge theory (Quantum Chromo Dynamics (QCD)) to become (almost) free from interactions when probed at space-time distances which become asymptotically negligible with respect to the sizes ($\simeq 10^{-13}$ cm) of the strongly interacting particles (the hadrons). This peculiarity, which was proven within perturbation theory, has become enormously influential, due to the apparently natural solution it engenders of the unexpected simple structure of the cross sections of highly inelastic scattering of leptons off protons and neutrons (Deep Inelastic Scattering (DIS)). Besides its seemingly high predictive power in DIS, I believe that AF owes its universal acceptance to what appears to me as an unconscious psychological attitude, that pervades our advanced societies, which has to do with the jealousy for our privacy: the wish to become "asymptotically free", when we choose to go far from the "madding crowd"; the final triumph of the individual over the constraints of collective life. To see this now highly popular dislike of collectivism supported and corroborated by a rigorous result in (perturbative) Quantum Field Theory (QFT) must have certainly enhanced the belief that perturbation theory is a very good tool to describe the interactions among different levels of reality, involving far away space-time scales. The irony of all this, as I thoroughly pointed out in the last several years [3], is that just within the theory (QCD), that has led to a rigorous perturbative demonstration af AF, there has arisen the most spectacularly paradoxical notion of all modern physics, that of the **confined** quark. For those who are not very familiar with all this, let me only point out that the quark is the only known constituent of (hadronic) matter that is **essentially inseparable** from the matter it constitutes, i.e. nobody has ever been able, by any physical means, to take the quarks out of the particles (hadrons) of which they are, allegedly, constituents. It so appears that, as far as the quarks are concerned, they **are not asymptotically free**, in the sense that under no circumstances can we realistically talk of the individual quark, for unless a quark finds in the close environment partners to compensate its colour charge, it is condemned to disappear in an all embracing, sticky (recall that the quanta of the colour fields are called "gluons") vacuum. Thus the physical reality (or better unreality) of the quark discredits the notion of AF, just in the theoretical framework where this concept was first introduced and perturbatively proved.

But as far as condensed matter physics is concerned, within the prevailing electrostatic picture of interactions, that we may call the "**Electrostatic Meccano**", AF is a true property of the QFT describing it, and its expectations coincide with those of the physics community. Thus according to AF it does not make any sense that the nuclear physics of deuterons, with its space-time scales six orders of magnitude smaller than the space-time scales of the lattice, be any different in vacuum than in the Pd-lattice. Any attempt to concoct, through some quantum-mechanical trick[3], an explanation of the CF phenomenology that stays within the tenets of the paradigm must necessarily fail, for AF is deeply rooted in the paradigm — I daresay it is the paradigm — and there can be no doubt that AF rules out CF altogether.

I believe that it is the poor understanding of this general implication of the paradigm that is responsible on one hand for the criticism of the majority of the scientific community of the "pathological" nature of CF, and on the other of the inanity, worse the outright falsehood, of most of the many theories of CF that have been proposed in the last six years.

But before introducing a new paradigm that avoids the narrow confines of AF (which I shall do in the next Section), I wish to conclude this Section with a small observation on "pathological science". As well known CF has been associated in its "pathology" with another subject that caused a scientific scandal in the seventies, the "polywater" affair. Unlike the discoverers of CF, the discoverer of "polywater" — Deryagin — finally recanted, conceding that the water that is found in very thin quartz capillaries is not a new strange phase of water but some kind of gel containing a sizable fraction of solved silica. This admission was hailed with relief and the deep rewarding feeling that scientific rationality had finally triumphed over unsound scientific claims and deductions. But, should it have really been so hailed? I doubt it, for, if not "poliwater", Deryagin had discovered the incredible fact that pure water (a collection of small, rather featureless molecules) is able to attack and dissolve a material that can stand the strongest chemical agents. Is this understood within the paradigm? A reply to this simple question would be highly appreciated.

Curing the "pathologies": QED coherence in matter

In the introductory Section I have tried to explain that the "pathological" nature of the science of CF, as perceived by the large majority of the scientific community, lies in fact not in the wide experimental body of knowledge

[3] Another unhappy aspect of the dominating paradigm is the subjectivism of the Copenaghen interpretation of Quantum Mechanics, imbibed of irrationalism. Irrationalism that very often is played against whoever asks embarassing questions about the physical bases of universally accepted but extremely obscure quantum mechanical developments (such as, for instance, the BCS "theory" of cold superconductivity and the Mössbauer effect).

that reproducibly has been accumulated throughout the world in the last six years, but rather in the intellectual framework — the paradigm — that dominates not only in physics, but in chemistry and biology as well.

When dealing with condensed matter phenomena the normal[4] scientist is led by the paradigm to expect that the constituents of condensed matter (atoms, molecules) are bound together by the same kind of forces that bind the elementary building blocks of molecular physics, namely electrons and ions/nuclei. As we know well, these forces are **all** of electrostatic nature, and have the fundamental property of extinguishing themselves at distances larger than a few $\mathring{A}$'s. This property stems from the well known fact that condensed matter at scales of just a few $\mathring{A}$'s is neutral. Thus the long range part $(1/r)$ of the Coulomb interaction[5] is ineffective (Debye screened) at distances that exceed a few atomic radii. The mental picture of condensed matter, that the paradigm suggests to the normal scientist, is that of a huge **electrostatic Meccano**, where the electrostatic two body (local) interactions are the nuts and bolts that keep the pieces of the Meccano — atoms and molecules — glued together to form the enormously complicated structures of condensed matter: crystals, glasses, hydrogen-bonded liquids etc. In this picture macroscopic order, that we contemplate often with marvel in matter, both living and inanimate, is the outcome of the juxtaposition of the pieces of the Meccano through the holding action of the electrostatic nuts and bolts. But just as the Meccano picture supports, with the aid of properly designed blueprints, the possibility of building meaningful, ordered structures, when all this is cast in the necessary quantum mechanical framework, where all nuts and bolts must be somewhat loose, in obeyance of the Heisenberg principle, it becomes highly implausible that such looseness, compound over about 10^{24} (the Avogadro number) elementary systems may result in macroscopic structures with the kind of stability and strength that we observe in condensed matter. And as a matter of fact, I know of no solvable theoretical model that achieves macroscopic order through short range interactions[6].

But there are more specific reasons not to believe in the **electrostatic Meccano**; just to mention a few: the Mössbauer effect, ferromagnetism, superfluidity, superconductivity, catalysis, experimental realities for which there exists only "kinematical" theoretical frameworks, based on "self-consistency" , the deep "dynamical" reasons being always postponed to some demiurgical

[4]see footnote on page 4

[5]It is thus very surprising to find that the most popular theory of the quantized Hall effect is based on a Hamiltonian for the electrons' system whose interaction term is the pairwise Coulomb interaction.

[6]The usual objection that I hear to this contention is, of course, in terms of the Ising model. However (i) the Ising model is formulated on a lattice, thus on an "a priori" ordered structure, (ii) I know of no successful application of the Ising model to any realistic system.

future computation/simulation.

It is for the keen unsatisfaction with the present state of affairs that a few years ago (1987), having completed a long and difficult analysis of the Quantum Chromo Dynamics (QCD) ground state [4], in search for an explanation of the peculiar confinement property of quarks, I started on a research programme to investigate the possibility that also for QED in condensed matter systems the ground state (the vacuum) show the highly non-trivial structure that in QCD explains the paradoxical confinement behaviour of quarks. This research quickly met with unhoped for success, and, as I explained in a series of lectures already in February 1989 [5], the idea[7] that the transverse electromagnetic field in the ground states of condensed matter is well described by an infinite set of quantum harmonic oscillators in their minimum energy states (the perturbative vacuum) is just simply wrong, for it is based on an approximation (the slowly varying envelope approximation) that, though fully valid in Laser physics, is totally inapplicable in systems that are cold and dense enough. And if the ground state of the e.m. field in condensed matter is in general the perturbative QED ground state, this must mean the doom of the paradigm, for it systematically ignores an important, indeed a fundamental actor of the dynamical drama of matter. And if the ground state of a piece of matter, as the paradigm envisages, is not the loose structure where the elementary matter systems and the e.m. field each perform their zero-point quantum oscillations chaotically and independently, what is it, really?

The answer, as widely and thoroughly discussed in a book that is due to appear soon [7], is disarmingly simple: it is a laser-like state where matter and field oscillate in phase on a frequency that is characteristic of the particular atomic/molecular species that make up a particular piece of matter. But there are two fundamental differences:

i) the laser-like state, being the ground state, is accessed **spontaneously** without any pump nor optical cavity, when it is below a critical temperature and above a critical density;

ii) no e.m. radiation can escape from such a state for, being it the ground state, this would violate energy conservation.

It turns out that with the kind of "oscillator strengths" (matter - e.m. couplings), and the kind of densities ($\left(\frac{N}{V}\right) \simeq 10^{22} \div 10^{23}$ cm^{-3}) typical of the atomic/molecular systems of condensed matter the critical temperatures are usually very high ($\simeq$1000 K) and the critical densities are usually well below 10^{23} cm^{-3}. Thus such states are predicted by QED to occur very frequently, as indeed they do! It should be abundantly clear, at this

[7] As forcefully argumented by P.W. Anderson in an influential book on condensed matter physics [6].

moment, that the doom of the paradigm, implied by such new developments, demands that it be replaced by the new approach, that is based on a solid (through well defined approximations) analysis of the ground states of QED in condensed matter systems [5]. We may call this new approach the **electrodynamic Network** (EN), for it involves the multitudes of atoms/molecules of a macroscopic piece of matter in an intricate dynamical interplay mediated by a large amplitude (classical) e.m. field.

One of the basic aspects of the EN, that being very conterintuitive is most difficult to visualize, is the peculiar behaviour of the matter systems, which comprise well defined quantum wave fields, the matter wave fields. When we concentrate our attention upon the elementary matter systems it seems completely natural to think that when in the ground state they are in the lowest state of excitation, for this appears to be a straightforward consequence of energy minimization. This, in fact, turns out to be generally wrong for, due to the phase coherence between the matter field and the e.m. field, energy is minimized when both the electromagnetic and the matter fields are in an excited state, the (negative) interaction energy being sufficient to bring the value of the total energy below that of the perturbative ground state, where both e.m. field and matter are indeed in their ground states.

As I have stressed [8,9] time and again to the CF community since the first paper published in May 1989, the ground state of a piece of matter, say a Pd rod, is so crowded with large coherent e.m. fields that the idea that AF should hold there does not seem at all reasonable. Indeed one should carefully evaluate (and this has been done in successive steps [9]) if and in which way the expectations of AF are violated. However, one thing can already be said with certainty: AF is not a general property of the coherent ground states of QED in condensed matter. Thus, from a purely conceptual standpoint, in the new approach the "pathologies" of CF are completely cured, for they are all based on a general property, AF, that does not hold in a large class of physical solutions of QED, the coherent ground states (CGS). At this point one may conclude that the " pathology" of CF is in fact in the eye of the beholder!

In an article written together with M. Fleischmann and S. Pons [10] an articulate discussion was presented of the strange behaviour of Hydrogen in a metal matrix such as Pd, arguing that, even ignoring the phenomena of CF, what is well known since long about the way Hydrogen enters and diffuses in Pd is totally at odds with the paradigm, and its qualifying AF. In particular, the high potential barrier ($\simeq 30$ eV) that a D_2-molecule must overcome before entering the Pd-lattice (where, according to a series of fascinating experiments of A. Coehn [11], Hydrogen is found in the ionized state) is something that within the paradigm has never found any sensible explanation. This and the many other mysteries that still remain to be unraveled in the Palladium/Hydrogen system, as we have argued in Ref. [10], should be another warning signal that one does have to go beyond the paradigm, even ignoring (but how?) the dry and reproducible facts of CF.

Let me now devote the rest of this Section to paint a possible scenario of the Pd/D system at high loading (x =Pd/D), where the CF phenomena are known to occur. I shall do this in the framework of the new approach, whose conceptual bases have been described at length above, and whose calculational details have been worked out in several previous talks and reviews [12,13][8].

The Pd-lattice (a face centered cubic lattice) is the locus of several coherent dynamical processes, that give rise to coherent, collective plasma oscillations of the different elementary systems. Those in which we shall be mainly interested in are:

i) the coherent plasma of the Pd-ions;

ii) the coherent plasma of the d-electrons;

iii) the coherent plasma of the conduction electrons.

These plasmas realize an energetic gain with respect to the (mono)atomic high temperature gas, and such gain constitutes the previously ill-recognised "raison d'être" of the crystalline structure of the metal. When the Pd-lattice is entered by the Hydrogen (and its isotopes D,T) atoms one finds that the electrons go to augment the conduction band, and the (partially screened) nuclei arrange themselves in particular sites (octahedral and tetrahedral) (See Fig. 1), where they perform coherent, collective plasma oscillations. Again, it is the energy gain that is associated with the latter plasma oscillations that justifies the incorporation of Hydrogen by the Pd-lattice, that in fact turns out to be exothermal ($\simeq$ 15 Kcal/mole). Thus in the Pd/H system there is another all important plasma:

iv) the coherent plasma of the (partially screened) H (D,T) nuclei.

As explained in Ref. [13] there are two types of H plasmas, according to the sites the H-ions occupy: the β-plasma in the octahedral sites, and the γ-plasma in the tetrahedral sites. I have shown (and a more refined calculation [14] confirms it) that for $x \leq x_o$ ($x_o \simeq 0.7$) it is the β-plasma that realizes the ground state, while for $x > x_o$ the γ-plasma possesses the minimum (free) energy. Thus on a purely theoretical basis it was concluded that a (second order) phase transition (from the β-phase in the octahedral sites to a new phase, called γ-phase, in the tetrahedral sites should occur for $x \simeq x_o$. This prediction has now found many confirmations ranging from

[8]This reasonably exonerates me from repeating what can be found in easily accessible literature.

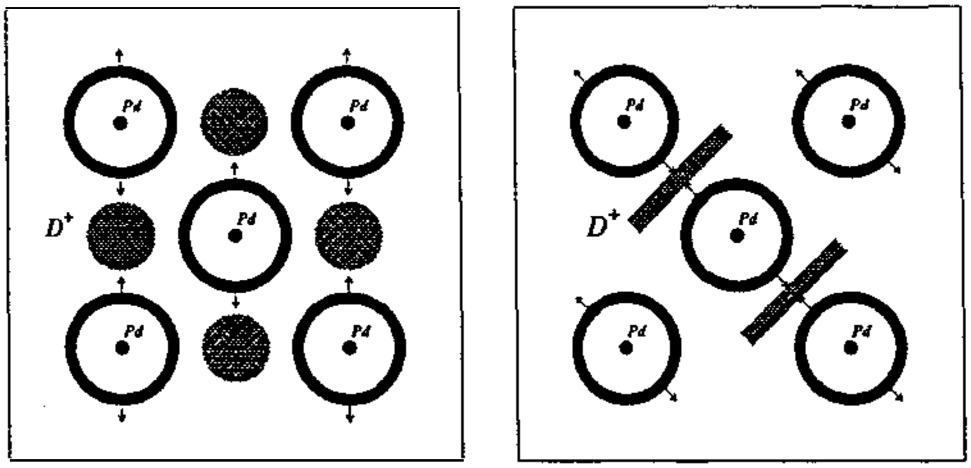

Figure 1: The octahedral sites (left) and the disks of the tetrahedal sites (right) in the (1,0,0) plane of the Pd lattice.

the measurement of the x-dependence of the diffusion coefficient [15], to the peculiar resistivity curve of the Pd/H system, which shows a maximum at $x_o \simeq 0.7$. The explanation of this latter well known fact is in the new approach quite simple and natural: the extra resistivity that, due to H incorporation, rises up to x_o and falls to zero for $x \to 1$, reflects the different scattering cross-sections that the H-ions have for the conduction electrons; in particular the decrease for $x > x_o$ indicates that in the γ-phase the H-ions are in a tightlier correlated state.

In the talk I have given at the ICCF4 [13] I posed two sets of questions, relevant to the H-loading of Pd (α) and to the CF phenomena proper (β). Let me recall them.

First the questions about Hydrogen (D) loading:

(α_1) Why and how does the process

$$D_2|_{gas} \to D + D|_{lattice} \tag{3}$$

proceed?

(α_2) why and how, in the lattice

$$D|_{lattice} \to D^+ + e^-, \tag{4}$$

D^+ and e^- enjoying a different, independent dynamics?

(α_3) why is D^+ so mobile at high x?

(α_4) what are the phases of Pd/D?

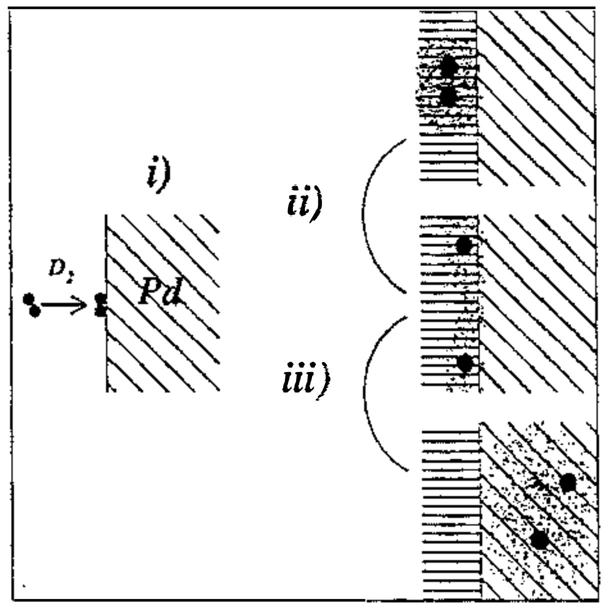

Figure 2: The steps of D_2 absorption by the Pd-lattice.

(α_5) what happens during electrochemical loading?

(α_6) why one observes "Heat after death" [16]?

As for the mechanisms of CF in the highly loaded Pd/D system, the relevant questions appear to be:

(β_1) how can one overcome the Coulomb barrier?;

(β_2) which is the main DD-fusion dynamics?;

(β_3) can we understand rarer processes: n,T,X-rays...?

Let me now briefly recall the answers.

(α_1) The process consist of two steps (see Fig. 2):

(i) the D_2-molecule is brought in contact with the Pd-surface; this step is governed by the gas chemical potential:

$$\mu_{D_2} = -\frac{T}{2}\left[19.3 + \frac{3}{4}\log\frac{T}{T_0} - \log\frac{p}{p_0}\right], \tag{5}$$

(ii) the D_2-molecule interact with the evanescent e.m. field associated with the different Pd-plasmas. It is the dispersive interaction of these coherent e.m. field that causes the tunneling of the D_2-molecule through the high barrier ($\simeq$ 30 eV) that separates it from the "shattered" state

($2D^+ + 2e^-$) in which it starts penetrating the Pd-lattice. Incidentally, the same kind of interaction must be at work when the coherent e.m. field of water [17] induce the NaCl (ionic) molecules of a crystal of salt to tunnel through their barrier (about 5 eV high) to become ions: Na^+ and Cl^-.

(α_2) The D^+'s and the e^-'s of the "shattered" deuterium surface state penetrate (almost) **independently** the Pd-lattice, for they belong to different plasmas, and there is an energetic gain to be part of the plasmas instead of remaining in the atomic configurations.

(α_3, α_4) It all depends on the different phases α- β- and γ- the D^+'s are in with increasing x. The α-phase is a disordered (gaslike) phase existing at $x < 0.1$; in this phase the diffusion coefficient is expected, and found, very small (see Fig. 3). The β-phase is an ordered (coherent) phase with the D^+'s oscillating in the octahedral sites. The (calculated [14]) x-dependence of the chemical potential μ for the β- and the γ-phase (the D^+'s in the tetrahedral sites) are reported in Fig. 4, which shows a cross-over at about x_0, implying that for $x < x_0$ the stable phase is the β-phase, while for $x > x_0$ it is the γ-phase to be stable. Recalling now that the diffusion coefficient is given by

$$D = \sigma x \frac{\partial \mu}{\partial x}, \tag{6}$$

where σ is Einstein's mobility, from Fig. 3 we may understand two major features of the D^+'s mobility in Pd:

(i) its fast increase with x;

(ii) its sharp discontinuity for $x \sim x_0$.

These deductions answer (α_3) and (α_4) completely. I should also remind you that the existence of the γ-phase, whose evidence is becoming compelling, was actually predicted on the grounds that CF **cannot** take place in the β-phase for:

(a) the D^+'s are too far away;

(b) only the tetrahedral sites can accomodate more that one D^+;

(c) only in the tetrahedral sites can the d-electrons of Pd effectively screen Coulomb repulsion.

This is another good example of the fact that the remarkable oddities of CF have just shed interesting light upon the dynamics of an ancient, well known system such as Pd/H.

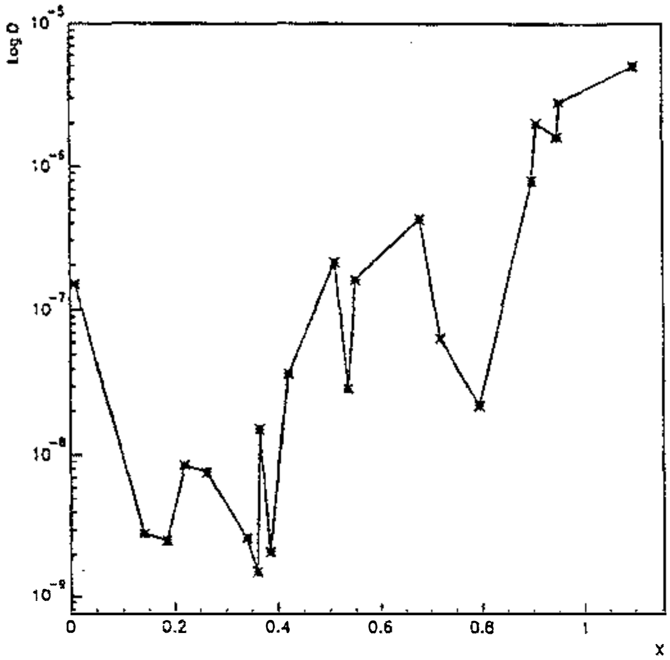

Figure 3: The diffusion coefficient of D in Pd as given by [15]

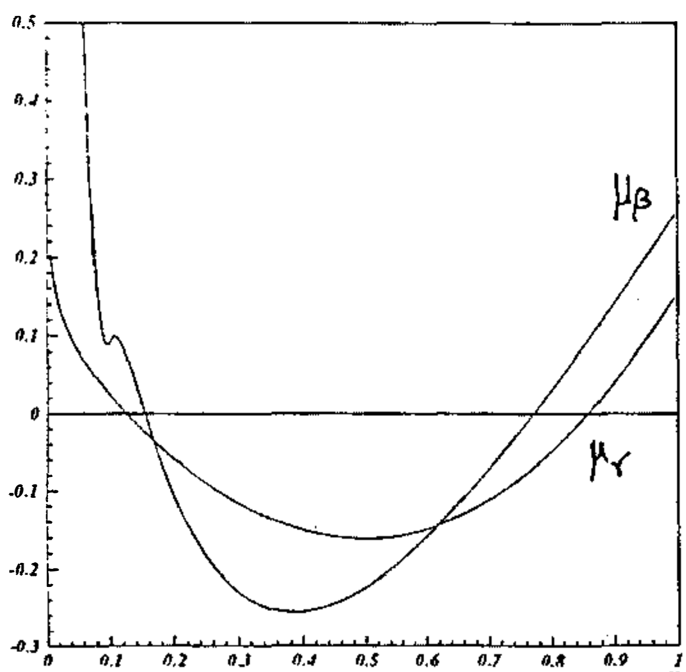

Figure 4: The chemical potential μ_β of the β-phase, compared with μ_γ of the γ-phase, as a function of x.

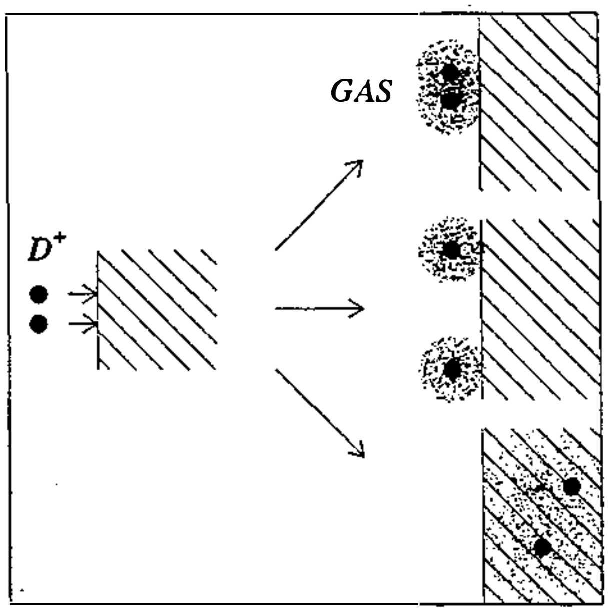

Figure 5: The different fates of D^+'s arriving at the Pd-cathode.

(α_5, α_6) The description of electrochemical loading that our approach affords takes off from the three possible fates (depicted in Fig. 5) of the D^+-ions once they arrive at the cathode. How many of them will be able to enter the Pd-lattice, and load it at high x (the fundamental prerequisite for CF phenomena to occur), it depends on:

(i) how many D-atoms will combine on the Pd-surface and leave the premises as D_2-molecules, forming the well visible gas bubbles;

(ii) the chemical potential of the D's left at the surface.

In order to minimize the number of D-atoms that bubble out as D_2-molecules some kind of "muck", that forms during the electrolysis upon the surface, is certainly useful, for it hinders the free traffic of D-atoms on the surface and its consequent high recombination probability. As for the surface chemical potential a fairly detailed analysis of the ponderomotive effects due to the evanescent e.m. fields of the various plasmas of the Pd/D system has been presented at ICCF4 [13], showing interesting autocatalytic aspects. In fact, the surface, as well as the bulk chemical potentials acquire negative terms, whose absolute values increase with x, thus leading to large equilibrium x-values, quite far away from thermodynamical equilibrium.

As for "heat after death", the subtle interplay of surface and bulk chemical potentials at high x is capable to account for the strange observations reported by S. Pons at ICCF4 [16]

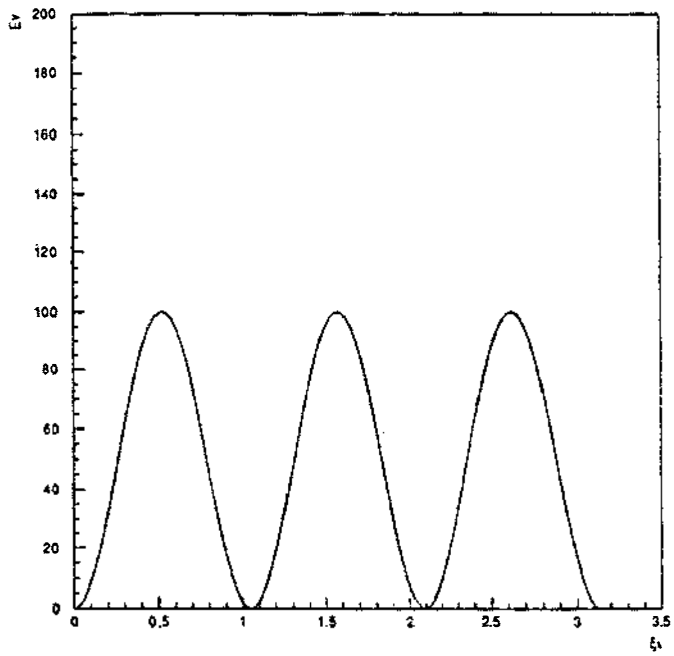

Figure 6: The electrostatic potential as seen by the D^+'s along the tetrahedral disks

Getting now to the CF phenomena in the highly loaded Pd/D system, I will now sketchily[9] answer the questions posed above, in order.

(β_1) The screening that the plasma of d-electrons provides in the tetrahedral sites has been determined by calculating the electrostatic potential generated by them, which is reported in Fig. 6.

Introducing such screening potential in the Gamow barrier penetration amplitude ($\mu = \frac{m_D}{2}$ is the reduced mass of the D-D system)

$$\eta_G \sim \exp\left\{-(2\mu)^{1/2}\int_{r_N}^{r_0} dr'[V(r') - E]^{1/2}\right\}, \tag{7}$$

yields

$$\eta_G(\mathrm{Pd}) \sim 10^{-22\pm1}, \tag{8}$$

some thirty orders bigger than the amplitude in the D_2-molecule $\eta_G(\mathrm{DD}) \sim 10^{-51}$. This very strong screening is enough to yield rates for the purely incoherent processes (the only one that takes place in vacuum)

$$\Gamma_{INC} = |\eta_G|^2\Gamma_{NUCL} \simeq 10(x-1) \text{ fusions/sec cm}^3, \tag{9}$$

where the $(x-1)$ factor takes into account the threshold at $x = 1$, a strict prediction of this approach.

[9] Again, extended answers are found in the ICCF4 talk [13]

(β_2, β_3) The main fusion path is not, of course, the incoherent one, that is the only fusion mechanism in the vacuum, and would be the only one in Pd as well, should AF hold, as dictated by the paradigm.

The new approach, in fact, predicts that the strong e.m. fields that crowd the lattice, once the barrier is penetrated, couple the extra D-nuclei (i.e. those in excess of $x = 1$) to the wave-field comprising all deuterons within a coherence domain[10], of the size of a few microns. Due to the N_{CD} factor, arising from coherence, and the strength of the e.m. fields, the nuclear dynamics becomes strong and fast, leading to the nuclear ground state ^{4}He with high probability and in a very short time. This is why:

(i) the main fusion channel is

$$D^+ + D^+ \rightarrow ^4He + \text{energy in Pd};$$

(ii) the energy released to the Pd-lattice brings the various coherent fields in excited states that relax with their particular times ($\frac{2\pi}{\omega}$, ω is the oscillation frequency) in various forms:

a) e.m. radiations of all frequencies,

b) exotic nuclear photo-reactions, including neutrons from the photofission of D,

c) heat, i.e. phonons.

(iii) in regions where the coherence is partial, we may have different, weaker and longer pathways (Fig. 7), which may explain the odd $\left(\frac{n}{T}\right)$ ratios that have been observed.

(iv) a (semi)quantitative calculation of the main fusion channel on the case of "minimal" coherence (i.e. coherence over only one CD) yields for $x \sim 1.1$ rates of the order of kW/cm^3 Pd. Much higher rates are expected for higher x-values and more extended coherence.

To conclude this Section, I believe I may state with confidence that on the basis of the accumulated body of observations and of the theoretical work done so far:

(α) a good, predictive, sound theory of the "pathologies" of CF can be based on the general laws of QED, liberated at last from the crippling constraints of Asymptotic Freedom;

(β) the science of CF is just in its infancy, other promising systems, besides the Pd/D are now coming to the fore and even in the Pd/D system there

[10] As I have explained, almost "ad nauseam" on several different occasion, the minimum space domain in which the matter and the e.m. fields are coherent is the Coherence Domain (CD), whose size is the wave-length of the e.m. field.

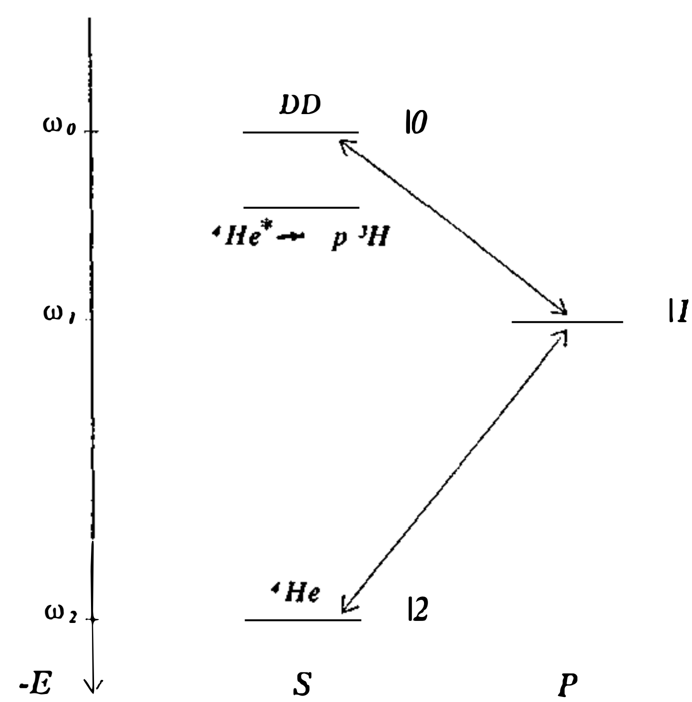

Figure 7: The level diagram for the states involved in the dynamical evolution described in the text. Also reported is Schwinger's $^4He^*$.

are large areas of experimentation where a new chemistry, a new condensed matter and a new nuclear physics are very likely to emerge, fostered by the power and the simplicity of the new, coherent approach.

What's wrong with this?: a reply to my critics

I may concede that I never, since my long scientific militance in the camp of CF, expressed so explicitly to this community the need for the drastic paradigm shift that the CF phenomenology demands. The new approach — QED coherence in matter[11] — just realizes such paradigm shift by identifying in the neglect of the transverse e.m. field in condensed matter a fatal drawback of the dominant paradigm and of its fundamental consequence: Asymptotic Freedom. In hindsight this was a mistake, for very few scientists (fortunately Fleischmann among them) have perceived the logical cogency of the various deductions that form the body of an in principle (to go from principle to practice will require substantially more work) **complete theory** of CF, both with regard to the fascinating problems of Hydrogen loading in a metal matrix such as Pd, and to the CF phenomena in such metal/D systems.

As a matter of fact what in my work most people have incorrectly understood, and criticized, is a collection of more or less "imaginative" models to fix the various "miracles" of CF, that to a closer scrutiny within the frame of prejudices and constraints of the paradigm end up (is it so surprising?) to violate one or other of its tenets.

I shall illustrate this important point by analyzing and answering the most explicit critiques[12] that have been levied against my work, which appear in the ICCF4 Proceedings [18], and in a review article published in a recent issue of the International Journal of Theoretical Physics [19].

The hostile attitude of the authors toward my work is betrayed by their complaint that Dicke, the inventor of Superradiance, is never mentioned in the CF papers. As mentioned in a footnote above, though Dicke's Superradiance leads to ordered quantum states of matter and radiation that have some common feature with those of QED coherence in matter, their distinctive feature of producing coherent e.m. radiation (like in a laser) is just orthogonal to the e.m. field trapping within matter, that gets realized in the new approach when the density of the atomic/molecular systems is

[11] In a first stage I gave this approach the name of Superradiance, as a due homage to the pioneering work of R.H. Dicke. This was a mistake, for my generosity has been maliciously taken as a plagiarism. But more of this in a moment.

[12] To be fair, I should credit these critics the basic honesty of speaking out their disagreement: a practice that is less and less popular in the contemporary scientific world, for it exposes the critic himself to critiques. The more frequent and safer practice makes use of gossips and of anonymous referee reports.

large enough and their temperature low enough. It is just for these reasons that Dicke has been fully mentioned in other papers and lectures, his work being totally unrelated to the subtle phenomenology of CF.

Their hostility is further displayed in their accusing me of conceptual errors, that they do not care to argue, and of "a number" of numeric and analytic mistakes, of which they discuss only a minor one (about a factor of 5 in a Gamow amplitude $\sim 10^{-21}$, whose evaluation can clearly not be more accurate than one order of magnitude!).

But, enough with such pettiness; their really strong critique has to do with the large screening potential ($V_{screen} \sim 100$ eV at the bottom of the well) derived in Ref. [9], and further refined in subsequent calculations [14]. Incidentally, without such screening there is no (known) way to understand how CF phenomena could happen.

Having taken the original calculation to mean that the Z electrons of the Pd atom surround the D^+-ion at distances of the order of the Fermi-Thomas radius for a Z electron atom, they conclude that all this violates basic physics, for "a solid would collapse if such close equilibrium screening were possible".

Their turn of arguments would not be blatantly incorrect if the screening electrons (only the d-electrons, as realistically done since march 1990 [9]) were in static orbits around the Pd-nucleus. But, as stressed time and again, such electrons comprise a plasma performing wide plasma oscillations, that realize an energy gain. In such state, due to the strong correlations among them, the electrons do not behave incoherently and individually, as they do in the atom, but follow their collective dynamics, which endows them with much sharper localization properties, as if their effective mass (their inertia) were in fact larger than in the "asymptotically free" limit. For the sake of visualization only, one may think that the electrons tend to mimic the physics of μ-catalysis. Clearly, the misunderstanding has its origin in the critics' lack of perception that QED coherence in matter does change the paradigm, whose conceptual schemes are totally inadequate to penetrate collective behaviour, that, instead, is at the root of the new approach.

Finally the critique [18] that I violate the laws of physics "in trying to insure a large enough time for the existence of a large charge density on the sides of a crack". Again it is their poor understanding of the paradigm shift that is realized by the new approach that blurs the critics' perception of what I am trying to do in the different field of "fractofusion". The coherent e.m. fields that in the form of evanescent waves fill the crack of a mechanically stimulated solid may have rather large lifetimes, and it is they, and not a large density of electrostatic charges that are responsible for the acceleration (under appropriate conditions) of the charged particles that move across the cracks.

These are the only critiques that I find in the literature to the body of my work on CF, and so my reply should end here.

But, in the CF community, there are other more serious critiques, or better (worse!) the widespread belief that we do not, after six years, possess the correct intellectual means to understand CF. This refrain occurs over and over in all published literature, which most of the time ignores all that has been done in the new approach since March 1989. What kind of answer can I give to such vast majority of believers of CF, but skeptics of CF theory? I have thought a lot about ths problem, that I have always seen as a major obstacle to quick progress in this fascinating field. Philosophy, or better epistemology, clearly has not worked, for the conceptual superiority of the new approach should have convinced my colleagues to invest some of their time to try and understand it, and contribute better critiques to the development of the associated research program. The discussion it affords of general different and subtle mechanisms that give a completely coherent picture of the CF phenomena in the Pd/D system also has had little impact due, I presume, to the philosophical barrier just described. Let me try a new (though at this time subliminal) way. I can tell you with a reasonable degree of confidence that proceeding on the road built by the new approach through CF land one should be able to reproducibly achieve powers in the order of 100 kW/cm^3 Pd.

It is reasonable that all this will be common knowledge by the time of ICCF6.

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