Unleashing the Quark within:
LENR, Klein-Gordon Equation, and Elementary Particle Physics

(Preliminary report)

F. Smarandache, V Christianto
University of New Mexico
200 College Road, Gallup, NM 87301, USA

Introduction.
Recently we've read that there is an excellent Cold Fusion experiment performed by Prof Arata, showing that the promise of CF/LENR (Low Energy Nuclear Reaction) is rekindled.

With regards to this experiment, in our opinion part of the problem is to explain how the intraatomic interactions happen in low temperature. A hint on this issue is that perhaps what we know about QM is flawed under the fact of antihydrogen, see Van Hoydoonk [1]-[5]. And considering topological quantization, then can we expect to observe Bohr-Sommerfeld quantization inside the quarks too?

Of course, we don't mean to say that focusing on CF/LENR is because we're inclined to this kind of fusion, but because of our conviction to the idea that deep inside the nuclei, the structure resembles condensed matter physics (or superconductor), either using Ervin Goldfain's CGLE model, 'compressed hydrogen' (Rutherford), or Wilczek's theory [6]. Furthermore, one can find another hint by studying the Klein-Gordon equation for elementary particles, which suggest that deep inside the hadronic interaction is governed by boson. Similar conjecture can be found from Interactive Boson Model.

Here are comments from some fellows physicists on how this elementary particle can be understood via Klein/Gordon or condensed matter physics. They address these simple questions:

- Do you think that we can further extend your KGE to become quaternion Klein-Gordon equation? (see Nottale et al. [8]).

- Is it possible to replace Higgs field with boson field (reminiscent to Schwinger model)? See for instance the paper by Fujita et al. [7]

Comments by:

(a) Takehisha Fujita [7]

You may try to think of any possibilities of constructing fundamental scalar fields (complex fields, of course) in some way or the other. But I believe that this should not be a proper starting point for the scalar field. Simply there exists no fundamental scalar field which can couple to the electromagnetic field. I believe you may find a good explanation of these theoretical points in the textbook "Symmetry and breaking in quantum field theory." The Higgs mechanism itself is physically not acceptable. Unfortunately, people have been pretending that they understood the symmetry breaking theory, without examining its physics in depth. But in reality they did not understand the basic point of the vacuum structure in the symmetry breaking physics. The success of the Glashow-Weinberg-Salem model is entirely due to the final version of their Lagrangian density which has nothing to do with the gauge theory.

(b) M. Apostol

Dirac equation can be derived from Klein-Gordon equation by using quaternions. However, in curved spaces, this may raise problems, and fractal geometry seems to be needed in addition. A convenient covariance seems to be a prerequisite with quaternions, and this is not known to me. The difficulties reside in noncommutativity. As regards the Higgs, I incline to think that it should be a real scalar, after breaking, not a boson. After all, Schwinger model is essentially one-dimensional.

(c) Ervin Goldfain

My explanation is that Wilczek and other theorists from his generation belong to a school of thought that is no longer effective in explaining many experimental observations and "anomalies". This fact is one of the reasons progress in particle theory has been so slow. This generation has been trained primarily in perturbative Quantum Field Theory (QFT), Feynman diagrams and Path Integrals. These methods are mainly applicable in equilibrium QFT but fail almost completely when used in critical phenomena, nonlinear dynamics and chaos, complex behavior, phase transitions in extended systems, self-organized criticality, non-extensive statistical physics and so on. The problem is that many of such "old school" theorists are not ready to acknowledge that these traditional techniques simply do not work when studying nonlinear, open and irreversible systems and processes. I am not the only one that says that: there are studies that have reported this "unwillingness" or lack of training in modern analytic tools.

There are indeed many opportunities for developing condensed matter theory to a point where certain cooperative phenomena (such as cold-fusion) become better understood. Quantum phase transitions (phase transitions at low temperatures) and the physics of strongly correlated quantum systems in different dimensions are two prime examples of topics that are under active investigation.

A similar type of issues are present when talking about phase transitions in Quantum Chromodynamics, a theory with an unexpectedly rich spectrum of behaviors. Understanding quark-gluon plasma, restoration of chiral symmetry, formation of strange bound states of quarks (quarkonia) and gluons (glueballs) and so on, may also help explaining room-temperature collective phenomena such as cold-fusion.

The physics (and the spectroscopy) of macroscopic states involving anti-matter (anti-hydrogen and the like) are also far from being completely understood. It is my view that, until one is able to explain the mechanism of CP symmetry breaking in field theory, one is not in a position to comprehend the underlying physics of anti-matter. It is here where complexity theory (so called emergent physics) and approaches using CGLE may be of practical value.

With regards to CMNS/LENR experiments, it would be fascinating to come up with a sound theoretical model explaining these CF experiments. There are at least three avenues to such a model:

1) quantum phase transitions at sufficiently low temperatures (above 0 K). See work by Subir Sachdev and others.

2) mesoscopic non-equilibrium thermodynamics for quantum systems. See work by D. Bedeaux and P. Mazur et al.

3) non-equilibrium phase transitions (by analogy with reaction-diffusion processes). See work by Lubeck, Hinrichsen and others.

Hope this discussion will be found a bit useful.

FS, VC

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Appendix A: Cold Fusion experiment performed by Prof Arata (from [9],[10],[11])

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On 23 March 1989 Martin Fleischmann of the University of Southampton, UK, and Stanley Pons of the University of Utah, US, announced that they had observed controlled nuclear fusion in a glass jar at room temperature, and — for around a month — the world was under the impression that the world's energy woes had been remedied. But, even as other groups claimed to repeat the pair's results, skeptical reports began trickle in. An editorial in *Nature* predicted cold fusion to be unfounded. And a US Department of Energy report judged that the experiments did "not provide convincing evidence that useful sources of energy will result from cold fusion."

This hasn't prevented a handful of scientists persevering with cold-fusion research. They stand on the sidelines, diligently getting on with their experiments and, every so often, they wave their arms frantically when they think have made some progress.

There is a reasonable chance that the naysayers are (to some extent) right and that cold fusion experiments in their present form will not amount to anything. But it's too easy to be drawn in by the crowd and overlook a genuine breakthrough, which is why I'd like to let you know that one of the handfuls of diligent cold-fusion practitioners has started waving his arms again. His name is Yoshiaki Arata, a retired (now emeritus) physics professor at Osaka University, Japan. Yesterday, Arata performed a demonstration at Osaka of one his cold-fusion experiments.

Although I couldn't attend the demonstration (it was in Japanese, anyway), I know that it was based on reports published here and here. Essentially Arata, together with his co-researcher Yue-Chang Zhang, uses pressure to force deuterium (D) gas into an evacuated cell containing a sample of palladium dispersed in zirconium oxide ($ZrO2$–Pd). He claims the deuterium is absorbed by the sample in large amounts — producing what he calls dense or “pynco” deuterium — so that the deuterium nuclei become close enough together to fuse.

So, did this method work yesterday? Here’s an email I received from Akito Takahashi, a colleague of Arata’s, this morning:

> “Arata’s demonstration…was successfully done. There came about 60 people from universities and companies in Japan and few foreign people. Six major newspapers and two TV [stations] (Asahi, Nikkei, Mainichi, NHK, et al.) were there…Demonstrated live data looked just similar to the data they reported in [the] papers…This showed the method highly reproducible. Arata’s lecture and Q & A were also attractive and active.”

I also received a detailed account from Jed Rothwell, who is editor of the US site LENR (Low Energy Nuclear Reactions) and who has long thought that cold-fusion research shows promise. He said that, after Arata had started the injection of gas, the temperature rose to about 70 °C, which according to Arata was due to both chemical and nuclear reactions. When the gas was shut off, the temperature in the centre of the cell remained significantly warmer than the cell wall for 50 hours. This, according to Arata, was due solely to nuclear fusion.