ACS ENVR NET 2010 Report for distribution via internet

# A Personal Report of ACS-ENVR-NET 2010 Symposium

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This is a personal report about a scientific progress in CMNS/LENR field which I have felt so by attending the New Energy Technology Symposium (NET) of Environmental Chemistry Division (ENVR) of American Chemical Society. The Symposium was held on March 21-22 at Hotel Parc 55, as one of branches of the 239$^{th}$ ACS National Meeting & Exposition, March 21-25, 2010, San Francisco, CA, USA.

The symposium was organized by Dr. Jan Marwan (Marwan Chemie, Berlin, Germany) and co-chaired by J. Marwan and Fran Tanzella (SRI International, USA). About 60 papers were submitted and scheduled in program, however 10 papers were cancelled. 8 papers from Japan were seen in the program to be reported by A. Takahashi, T. Sawada, A. Kitamura, T. Mizuno, Y. Iwamura, T. Hioki and N. Yabuuchi. However, T. Mizuno, Y. Iwamura and N. Yabuuchi did not show up in the meeting. So, actual attendees from Japan were only 4. This very limited number of participants from Japan motivated me to write this report for informing the major results and progress (as I felt) of the meeting to JCFRS (Japan CF-Research Society) members.

Photo-1: I (A. Takahashi) am presenting paper at the NET meeting on March 21, 2010.

The NET meetings had 4 sessions for two days and I counted about 80 audiences at peak and 60 in average. Foreign participants are 7 from Italy, 4 from Japan, 2 from Germany, 2 from Russia, 2 from Israel, 2 from India, and others. A number of expected presenters listed in the program did not show up. Those are X. Z. Li, Y. Bazhutov, J. Dufour, M. Swartz, T. Mizuno, Y. Iwamura, N. Yabuuchi, X. Jiang, and D. Alexzandrov.

In the following description, I will report brief contents of presentations with my personal impressions (sometimes critical comments).

During the lunch time of March 21, ACS set up a press conference to air by ACS LIVE in its internet web-site, with head-line "*A potential new energy source so controversial that people once regarded it as junk science is moving close to the main stream………*". What is the real status of scientific quality and progress of the condensed matter nuclear science (CMNS) research which is supposed to look for low energy (type) nuclear reactions (LENR) under undefined conditions of condensed matter? My report tries to cut cross sections reflecting essential underlying mechanisms, if at all, by briefly summarizing talks with my personally felt comments.

**Session-1: Theory**

Actually 8 presentations were done, by Y. E. Kim, K. P. Sinha, A. Takahashi, P. L. Hagelstein, T. Sawada, G. H. Miley and R. W. Bass. Every theorist, including above people and others, has own model under developing to solve puzzles of condensed matter nuclear effects (CMNE), commonly known as cold fusion. Many different models compete mutually, exchange information and help comprehensive understanding of CMNE to advance. Someday theory of CMNE will be unified or selected to one through the natural selection (Darwin process).

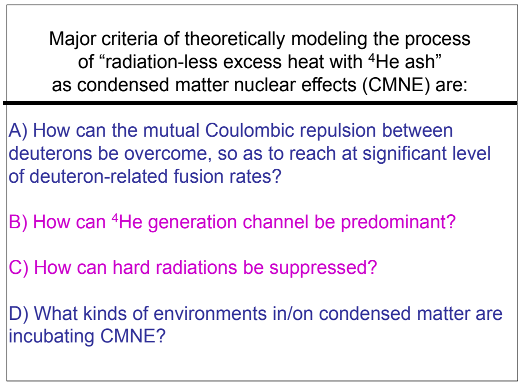



Fig.1: 4 criteria for theorizing CMNE

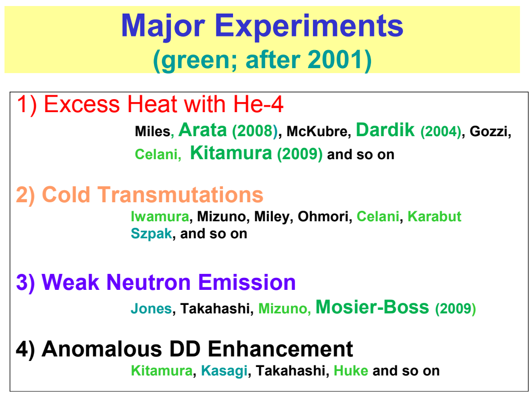



Fig.2: 4 major claims and authors

Although each theorist has unique conjecture and model and always wishes to strongly assert his/her own one the best, there are some common physics background for starting to investigate the CMNE problem, namely to explain consistently major experimental claims as 1) excess heat with He ash, but without hard radiations, and 2) "clean" transmutation reactions. As this is a personal report, I borrow some of my slides from my presentation at ACS-ENVR-NET 2010 to introduce first such common physics background.

Four criteria for theorizing CMNE are issued in **Fig.1**. And 4 major experimental claims and authors are listed in **Fig.2**.

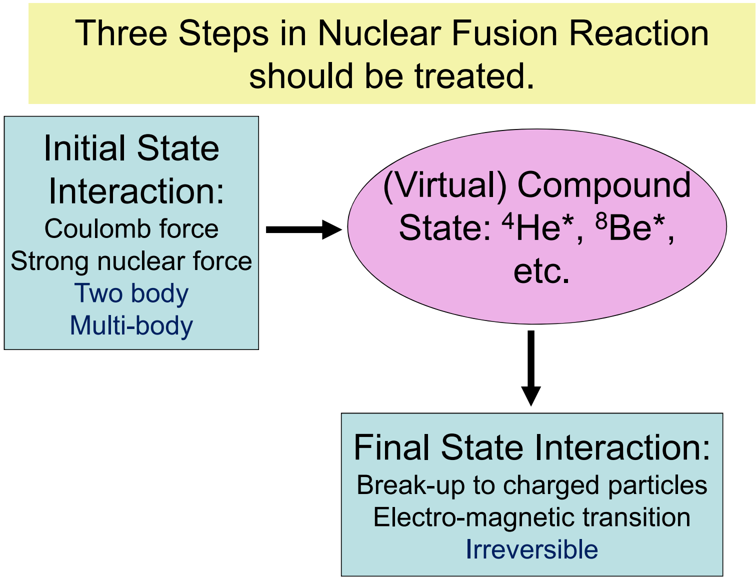



**Fig.3**: Three adiabatic states of nuclear reaction should be explicitly and properly treated for a completion of theoretical model.

As far as the completion of theory for any nuclear reaction is concerned, three adiabatic states of interaction as shown in **Fig.3** should be treated. The flow of interaction from the initial state interaction (**ISI**), the intermediate compound nucleus (**ICS**) and the final state interaction (**FSI**) goes one way (irreversible). And the final state transition or break-up from the intermediate compound state goes on a stochastic process within a life time of ICS. Most proposed models explicitly treat ISI within the scope of Coulombic (or electro-magnetic) interaction, namely the criterion A) of **Fig.1**. Nuclear strong interactions are implicitly thought (not actually treated) there in most models. Quite a few theories treat ICS and FSI.

Quantitative results by theoretical consequences can only validate the feasibility of a proposed model, as the great philosopher Descartes defined for the condition of theorization. Without quantitative consequences, one can say anything arbitrarily, but cannot conclude anything definitely. To quantitatively estimate nuclear fusion rate, we need to solve many steps of QM (quantum mechanics) equations, for instance by evaluating T-matrices (transition matrices) as shown in **Fig.4**. In every step, we need to solve electron wave-functions, particle (deuteron, for instance) wave functions for the initial and final states of every step to estimate T-matrix with an effective interaction Hamiltonian there.

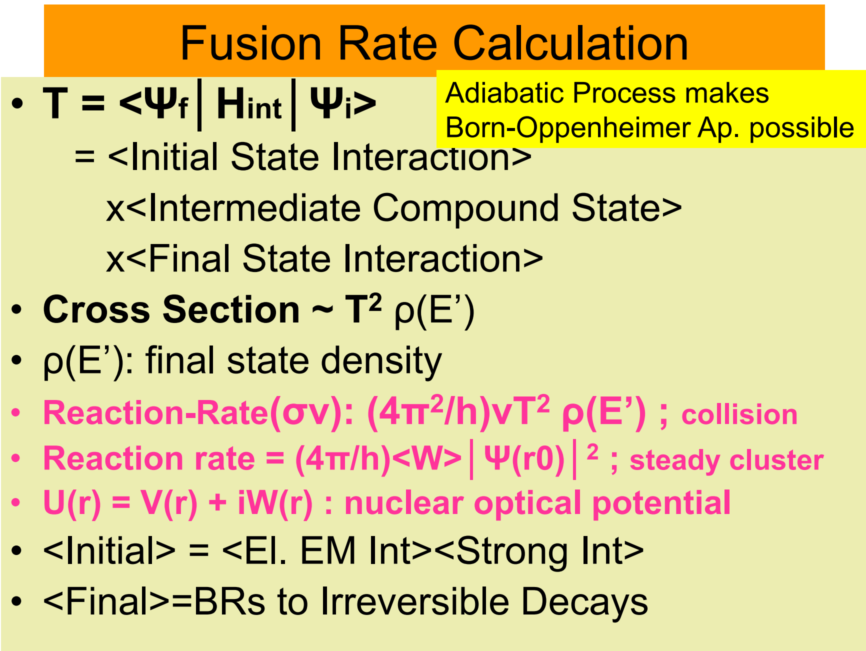



Fig.4: Procedure for fusion rate calculation

We can apply the Born-Oppenheimer Approximation (adiabatic treatment for every step of interaction), due to the large differences between electron wave length and d-particle wave length and between interaction-ranges (gauge boson exchange) of Coulombic force and strong nuclear force.

Especially for a D+D fusion (two body fusion), the criterion B) in Fig.1 is very difficult to be cleared, as illustrated in **Fig.5**. The ICS for a d + d reaction is an excited state of $^{4}$He, namely $^{4}$He*(Ex). The minimum value of Ex is 23.8MeV, since lower state than that is forbidden by kinematics (due to non-existence of negative kinetic energy). Namely, LENR of two body d-d reaction goes to $^{4}$He*(23.8MeV). Out-going channels by FSI are governed by neighboring broad resonance levels (shadow poles) with very large energy widths (QM uncertainty), and always breaks up to n + h or p + t channels predominantly (50%/50% branching ratio). The $^{4}$He (gs:0+) + gamma channel has a very small fraction of branching ratio ($10^{-5}$ %). The branching ratios must be constant for low energy d-d reactions in a range of 0 to 100keV relative d-d kinetic energy.

To change branching ratio or to make $^{4}$He production in dominance, we need a participation of a third field interaction to the d-d two body strong interaction, as shown in **Fig.6**.

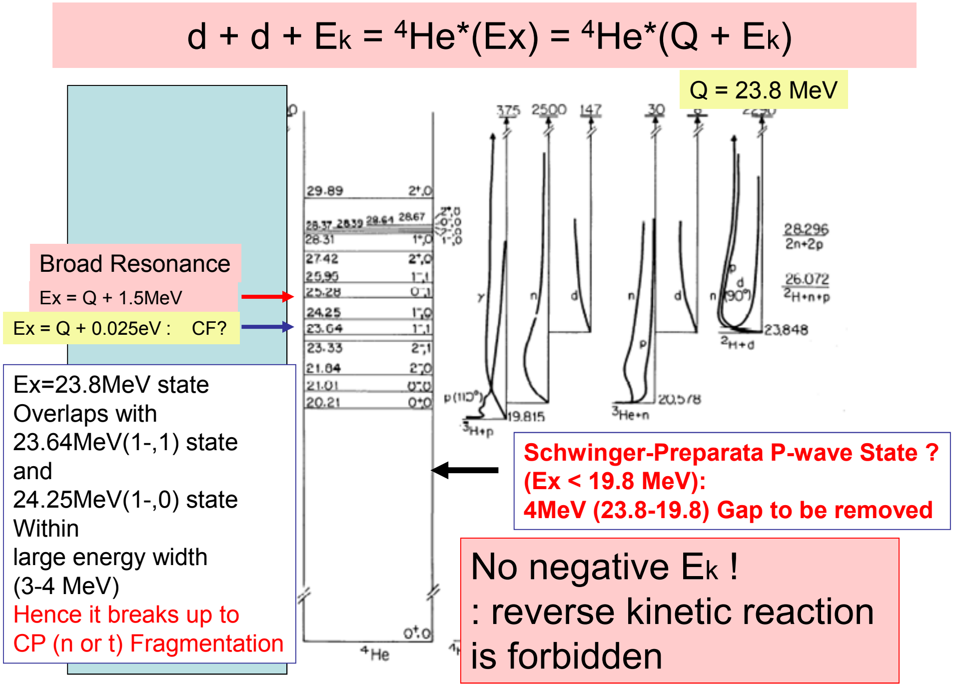



Fig.5: The reason why d+d to $^{4}$He + EM-emission cannot be major channel

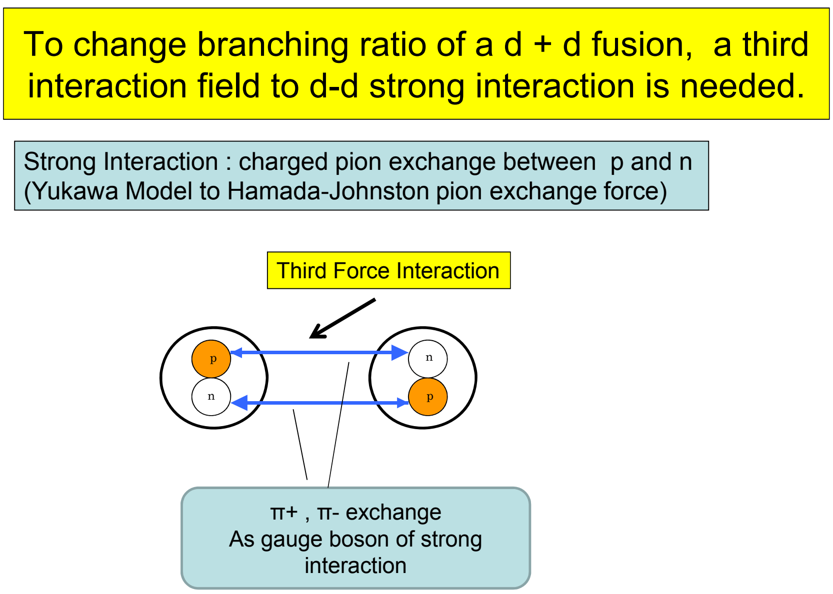



Fig.6: We need a participation of a third interaction during ISI to enhance $^{4}$He emission channel. Only a few theoretical models have treated it explicitly.

As a third interaction field, we have four possibilities, namely, 1) gravity, 2) weak nuclear interaction, 3) electro-magnetic interaction and 4) nuclear strong interaction, as shown their relative strengths in **Fig.7**.

## Relative Strength of Interactions

Comment by A.T.

- Nuclear Strong Interaction: $f^2/hc$ = **1**
- Electro-magnetic Interact.: $e^2/hc$ =**7.3E-3**
- Weak Nuclear Interaction: $(ghc)^2(mc/h)^4$= **5E-14**
- Gravity : $GM^2/hc$ = **2E-39**
- $S_{dd}$ = 1.1E2 keVb vs. $S_{pp}$ = 1E-22 keVb
  (Strong Interaction) (Weak Interaction)

$$\sigma \sim (\text{T-matrix})^2$$

Fig.7: Relative strength (field coupling constant) for 4 known fields of interaction

Since the nuclear weak force and the gravity force are too weak to make a visible effect to change d-d strong interaction, as estimated very small T-matrices, we can consider feasibility for the strong force and the EM-force only.

**Y. E. Kim** studies a conjecture of Bose-Einstein Condensation (BEC) nuclear fusion, BECNF. He assumes highly mobile states of deuterons and protons in condensed matter, as proposed by Coehen-Preparata. He assumes BEC possible for deuterons there, and very large barrier of Coulombic repulsions between deuterons are “avoided” in principle by the BEC state overlapping of deuteron wave-functions. Next, he assumes a cluster of many deuterons (ND) makes a (N-2)D + $^4$He(gs;+) + Q(23.8MeV) out-going channel to transfer Q-value (23.8MeV) to many participated deuterons’ kinetic energies. He published a paper in Naturwissenschaften 2009.

I think, his theory has yet many aspects to be quantitative. First of all, deuteron (+ charge, spin=1) in condensed matter will make very fast (in a fs) charge neutralization by attracting available electron (-charge, spin=1/2), even in the highly mobile state, and an entity of (d+e) should behave as “fermion”, not boson, hence BEC is in question. Secondly, he is yet to treat ICS for many deuterons compound state and its FSI, which is complex and not so simple as the assumed (“desired”) channel for a 100% $^4$He production.  To make a ICS by strong nuclear interaction, a cluster of many deuterons should be condensed directing its center-of-mass point by a large centripetal force in three dimensional domain to become a very small entity with very small inter-nuclear distance (e.g., 1pm or less) and finally to reach in several fm region of strong nuclear interaction. He does not show a model existence of such a large centripetal force. As the BEC is a considerable concept for CMNE, his BECNF model is expected to fill up these points.

**Andrew Mulenberg and K.P. Sinha** presented a paper titled “Tunneling beneath the $^4$He* fragmentation energy”. They have a conjecture of Lochon model. A Lochon is a pair of two 1s electrons for a proton (deuteron) having anti-parallel spin to each other. They model a large screening effect on Coulomb repulsive force between deuteron pair, if four particles (d-e-e-d, for instance) aligned in a one-dimensional line, as a confinement potential by Coulombic interaction is becoming much deeper for the one-dimensional arrangement, compared with two (and three)-dimensional configuration. The assumed trapping potential for d-d pair may become very deep as several MeV, and “therefore”, they consider the two-body excited energy of $^4$He* can be removed greatly as $\Delta Mc^2$ = 3-4 MeV. To realize this, every tightly bound electron state around d-d must be realized in a fm scale domain of nuclear strong interaction. They assume a Lochon can become such a small entity.

I think, their model has several fundamental problems. Again due to the very fast charge neutralization in condensed matter, occupation of two 1s electron states for a deuteron (proton) may not be allowed; the second electron should go to outer orbit (as excited state) to keep $H^-$ ($D^-$) relative state in condensed matter by following the system energy minimum principle. Hence, a formation of Lochon is in question. Secondly, the assumed “tightly bound electron sate” near deuteron in a fm domain requires a very short de Broglie wave length, a few fm, which kinetic energy (relativistic) is on the order of 100 MeV. To trap such a very energetic electron, we need a very much deep trapping potential as -200 MeV which they never proposed a model for. BTW, 200MeV is very close to a threshold energy of pion generation and we remember why the Yukawa model (the meson exchange model for strong force) was proposed to rule out the conflicts and contradictions caused by the old day’s nuclear model “nucleus is composed with protons plus electrons”.

**Akito Takahashi** has proposed the TSC (tetrahedral symmetric condensate) model for the third force interaction, as he wrote two papers in LENR Source Book Vol.1 and Vol.2 from the ACS publication, and many related papers in JCMNS and others since 1989.

His conclusion is shown, from his ACS2010 presentation, in **Fig.8**.

**TSC realizes A) super-screening of Coulomb barrier and B) 4D fusion of $^4$He products**

- ***The ultimate condensation is possible only when the double Platonic symmetry of 4D/TSC is kept in its dynamic motion.***
- ***We have found that 4D fusion may take place with almost 100% yield per a TSC generation, so that the macroscopic 4d fusion yield is given simply by the TSC generation rate $Q_{tsc}$ in the experimental conditions of CMNS.***

Fig.8: Conclusion of Takahashi's TSC theory to explain heat with $^4$He

In his talk, he presented an interesting phenomenological model about a role of PdO surface layer of Pd nano-particles used in the Kobe group experiments (see Kitamura's talk in the next session). When we start a D(H)-gas charging run with nano-Pd/$ZrO_2$ mixed sample in vacuum, reduction of O by incoming $D_2$ molecule (namely a formation of $D_2O$ molecule going out to vacuum) makes a "sub-nano-dip" on surface of a Pd-nano-particle. The formed sub-nano-dip has quasi-free dangling bonds of electrons which arrange a deep adsorption (trapping) potential for incoming $D_2$ molecule. The trapping potential is deep enough (speculated as about 2eV deep by experiment) to trap there a second incoming $D_2$ molecule. An orthogonal coupling of two trapped $D_2$ molecules forms a TSC which condense very fast in a fs interval to generate $^8$Be* as ICS and two alpha-particles break up with 23.8MeV/$^4$He heat generation by FSI. Such a phenomenological process is drawn in **Figs.9-10**.

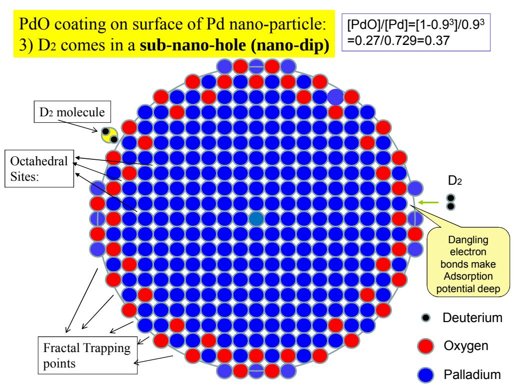



Fig.9: Formation of sub-nano-dip which strongly traps incoming $D_2$ molecule

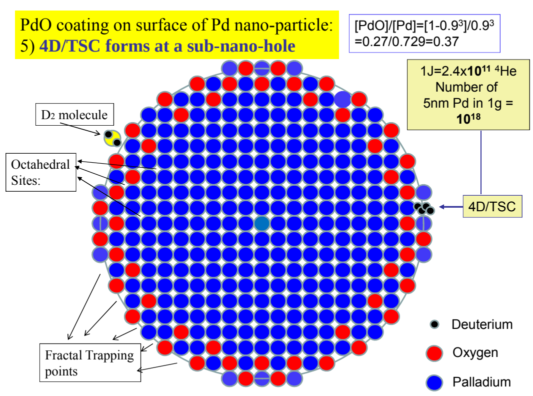



Fig.10: Formation of 4D/TSC at sub-nano-dip

As shown in the insert (upper), a 4D/TSC event per 10 million Pd-nano-particle per second would generate 1 W power per one gram of Pd powder, by this process at the beginning of D-gas charge. To this respect, normalized heat evolution curves of Kitamura et al paper (PLA 373 (2009)3109-3112) for a PZ sample 1st phase are quite interesting as shown in **Fig.11**.

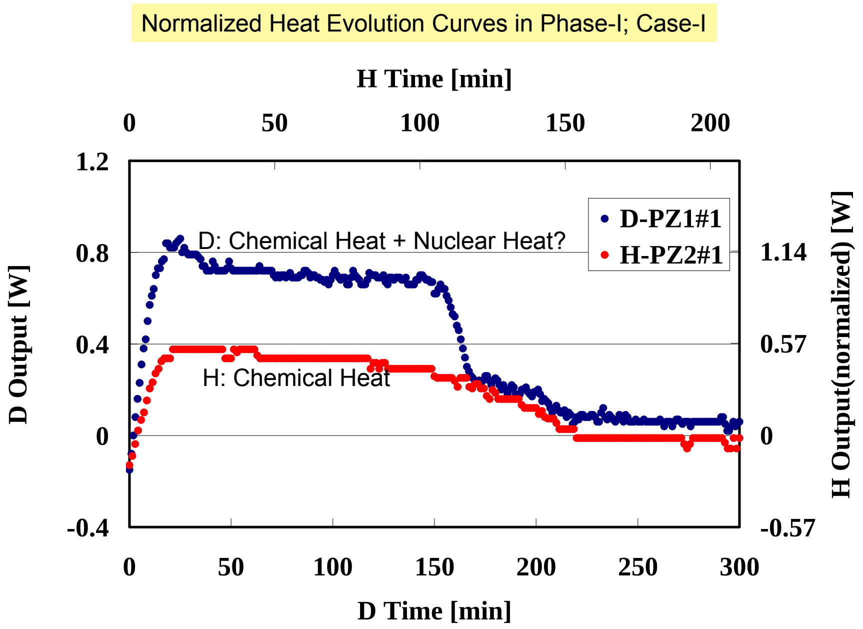



Fig.11: Normalized heat evolution curves for D(H)-gas charging to Nano-Pd/$ZrO_2$ samples, suggesting "nuclear heat component" apparently for D-charging in the 1st phase. D(H)/Pd ratios are about 1.1 at the end of the 1st phase. A peak at the beginning may correspond to 4D/TSC reactions as shown in Fig.10.

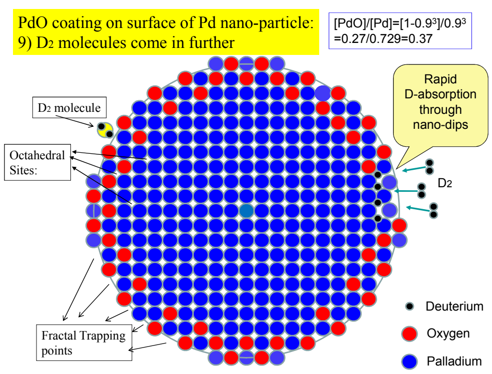



Fig.12: rapid D-absorption through nano-dips

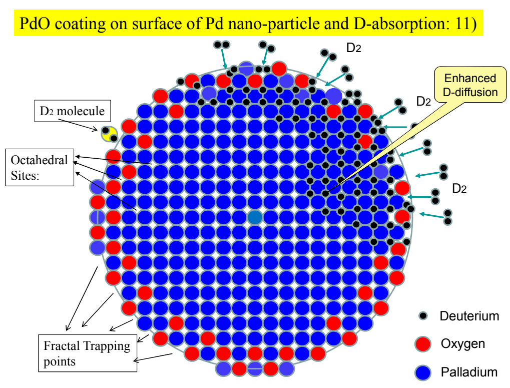



Fig.13: Enhanced D(H) diffusion by back-side pressure

Takahashi also proposed a model for explaining full D(H)/Pd loading in a relatively short time-interval

(10-100min, depending on D(H)-flow rate), by using drawings as shown in **Figs.12-13**.

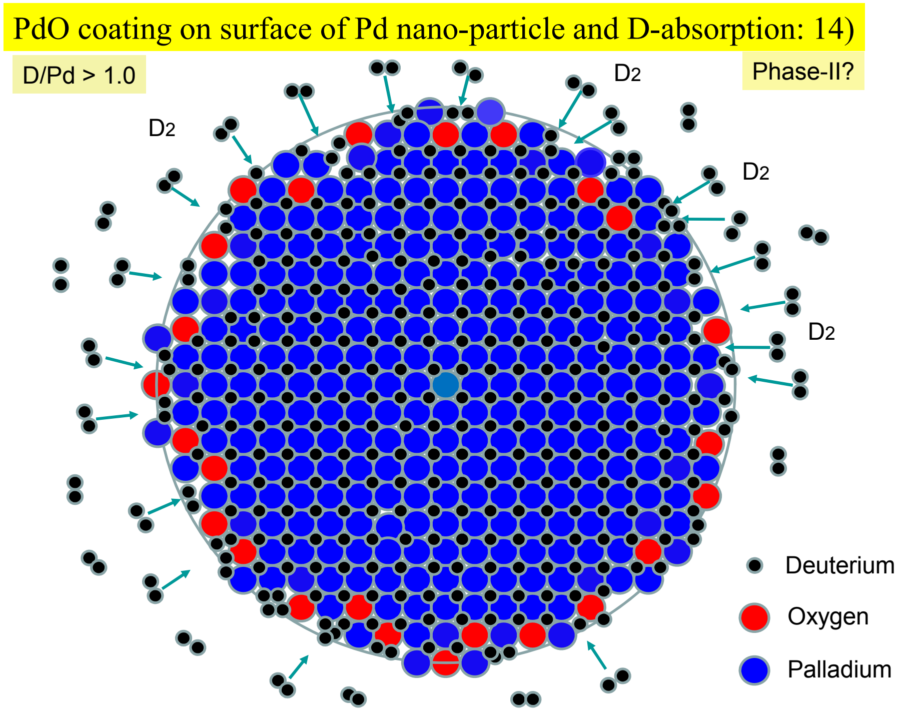



Fig.14: At the end of the 1st phase, D/Pd =1.1-1.2 is attained

The end situation of the 1st phase is drawn in **Fig.14**. All O-sites of inner Pd-lattice zone are occupied by deuterons. This gives lattice full loading; D/Pd=1.0. In addition, we have trapped $D_2$ molecules on surface nano-dips which give a portion of 0.1 to 0.2 for D/Pd ratio.

All together, we would have a value of D(H)/Pd =1.1-1.2 at the end of the 1st phase: this agrees very well with Kitamura et al forced oxidation run (see Kitamura, in the next session).

Takahashi proposes a formation of “global deep shrunken potential” for trapped deuterons in a mesoscopic size Pd nano-particle, as shown in **Fig.15**.

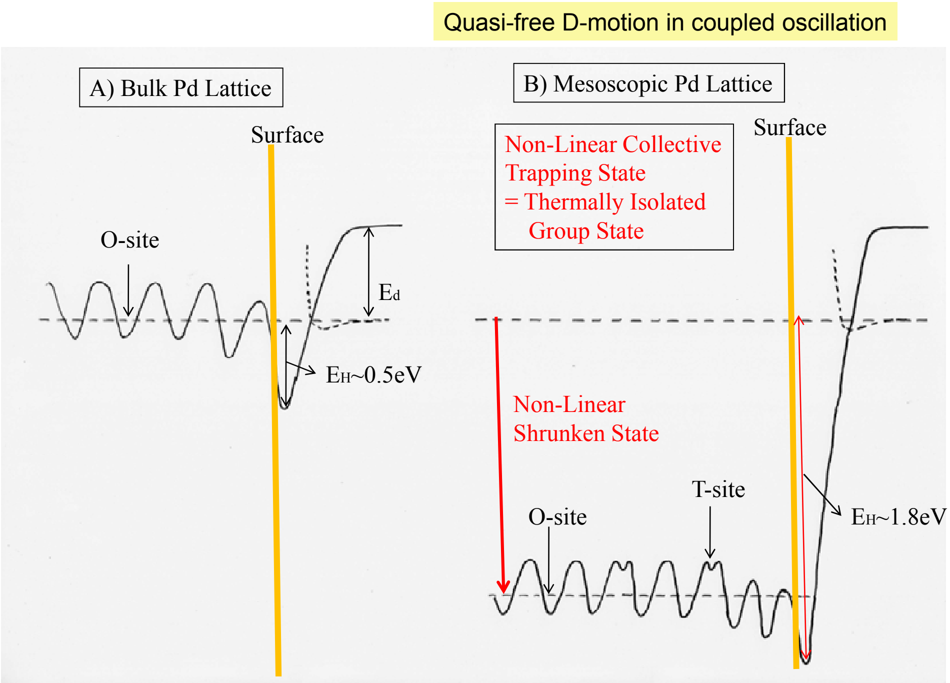



Fig.15: Takahashi proposes a non-linearly coupled "two pendulums" state potential for trapped deuterons in a mesoscopic size Pd-nano particle

He proposes a quasi-free deuteron motion under periodical lattice (Bloch) constraint, by the non-linearly coupled motion of long-short pendulums coupling, strongly enhances TSC formation probability at around T-sites in the local lattice Bloch potential, to induce rather long-lasting 4D/TSC fusion and heat generation; this would be the explanation of anomalous heat observed in the second phase of the Kitamura PLA paper.

**Takahashi's second paper** discussed possible FSI and products by 4D/TSC fusion. Treating even and odd spin-parity states of $^{8}$Be* as ICS and symmetric and asymmetric fragmentations via excited states of composite fragments as $^{4}$He*(Ex) and $^{6}$Li*(Ex), he concluded alpha-particle energies in 2-5 MeV mostly for S-wave transitions. An odd spin parity state of $^{8}$Be* gives a channel of EM transition via multi-photon (QED photons possibly) emission and the lowest alpha-particle energy is 46 keV in this case. A symmetric fragmentation to $^{4}$He*(Ex=20.21MeV) with its succeeding break up to energetic triton and proton may be a source of secondary t-D reaction to emit minor yield fast neutrons in 10-17 MeV region, which may induce $^{12}$C(n.n')3alpha reaction of CR39 detector of the SPAWAR experiments.

His modeling should be criticized by other people than me. A present status of Takahashi theory on CMNE is flow-charted in **Fig.16**.

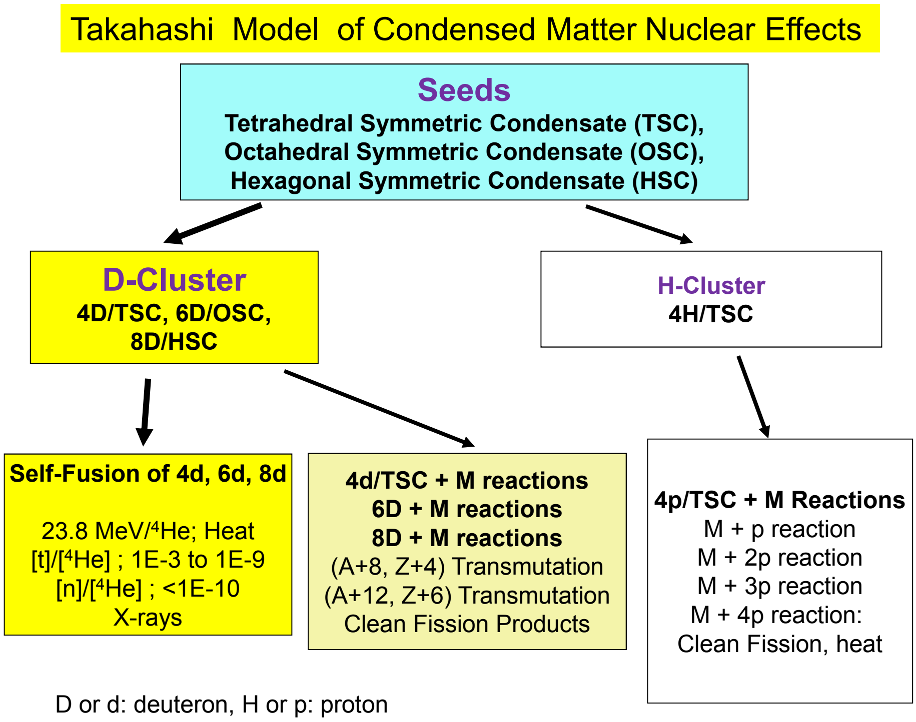



Fig.16: Takahashi model for CMNE; yellow blocks show problem well studies, while others are yet to study deeply.

**Peter Hagelstein** is extending his models since many years. Recently, he published a paper in Naturwissenschaften about yield of secondary reaction neutrons in D-contained condensed matter by energetic alpha-particle injection. This analysis gives an estimate of highest kinetic energy of CMNE/LENR induced $^{4}$He by D-related fusion reactions, by believing experimental neutron yield c.f. observed heat level. He is trying to answer why hard radiations are not seen in experiments, while excess heat and $^{4}$He production exist. He also develops an extension of "spin-boson" model for possible nuclear energy transfer finally to lattice. A new kind of model under consideration assumes a $^{A}$Pd to $^{A-1}$Pd + n like state.

I think, he has very seriously tackled the nuclear-lattice energy transfer problem, which is the EM interaction for the 3$^{rd}$ force interaction to realize major $^{4}$He out-going channel. However, the effort is yet to finish. The $^{A}$Pd to $^{A-1}$Pd + n state, under consideration may have difficulty in nuclear physics point of view as a "halo n" state is hardly happening for around stable isotopes as Pd-104 to Pd-110.

**G. H. Miley and H. Hora** are proposing "Ultra high density deuterium clusters for LENR". He said a cluster in Rydberg matter may realize ultra-high density as $10^{29}$ d/cc.

They are proposing an ICF (laser fusion) target by this idea. To explain, their observed transmutation results by Ni/Pd-H systems, they are considering a peculiar model as a $^{108}$Pd + 156D to $^{126}$X + 38$^{3}$He reaction channel. They assume a "big very condensed cluster" with "pico-meter inter-nuclear distance and mega-second life time".

I think their conjecture is still in the stage of imagination. Reported Rydberg matters for a two-dimensional arrangement of atoms with circular (not QM spherical) orbits makes inter-atomic distance much shorter than usual molecule or 3-dimensional solid lattice. However, to realize a big pm-size D-cluster (156D, for instance), we need a strong centripetal force of system condensation into a system center-of-mass point. They have to show a possible existence of such condensation force for so many particles (deuterons plus electrons). Even if many deuterons would condense so, simultaneous strong interaction to induce very large ICS with very high excited state, which would make a chaotic many break-up channels annoyingly.

At a time of coffee break, George told me, "we will show that by experiment!" Oh, astonishing, I responded.

**T. Sawada** made a similar introductory talk as my introductory why a usual d-d reaction does not got to $^4$He channel. He prefers a magnetic monopole (MMP) induced d-d reaction. A MMP can make a "infinitely deep" trapping potential since MMP mass is infinity and the energy state of trapped d-d pair can be -4MeV lower than usual d-d molecule. Thus MMP can realize a very closely approached d-d pair with deeply shrunken state. He thinks $^4$He* excited energy may be removed by 4MeV in this state and can go out to $^4$He(gs:+) with 23.8 MeV kinetic energy.

I think MMP hardly exists and Maxwell (who denied MMP) was right. If MMP is born somewhere we would see a lot of nuclear reactions never ending as MMP has infinite life. His misunderstanding may be the thought 4MeV energy removal in the very deep trapping state. Even in the very deep trapping state, mutual d-d strong interaction does not change from the very shallow potential trapping case as muonic d-d molecule, hence $^4$He* excited energy by strong interaction never changes and keeps 23.8 MeV Q-value. We with leptons (electrons, in condensed matter) and MMP do not participate the strong nuclear interaction.

**R. W. Bass** gave a talk on "Only conventionally viable cold fusion theory". He recalled an old theory review paper by Chechin-Kim-Rabinobitz (Int. J. Theoretical Physics, 33(1994)617) and reminded "three miracles in cold fusion", c.f. Huizinga. His resonant tunneling model (1991) can, he asserts, explain how to clear the three miracles. He has written a number of papers accordingly.

I have not learnt well, how quantitative results he could obtained for overcoming Coulomb barrier in ISI, what is ICS in his model and how he treated FSI. Late Prof. Peter Hodgson, Physics, Oxford University, who was my friend in fast neutron physics study, once recommended me to make contact with Prof. R. Bass. However I have missed occasions to do so. He sometimes sends long messages with references (his papers) to colleagues in the cmns-google group via internet. I feel respect to him, but have difficulty to read all information given thoroughly taking enough time.

During a pause of lunch time, D. Kidwell (NRL) came to me questioning; "A thought experiment: if a black box contains (confines) all nuclear reaction products (particles), mass defect of the system never happens." I said; Yes, it is so, if so. But it's wrong. Such a state never happens on this planet. In an ICS nucleus excited, photons by EM transitions cannot be confined within nucleus, as an EM coupling constant c.f. strong interaction is too weak (see Fig.7) to confine photons (EM waves). The nucleus is transparent for photons produced. Therefore, the "black box" is transparent for photons to leak out freely. Thus reaction Q-value, namely a mass defect by ISI to ICS and FSI, should be released outside. The same is true for electrons (beta-decay by weak interaction). In a universe, we know, black holes exist confining everything including photons by a super-strong gravitational force of so-huge mass system. T. Matsumoto had imagined; there might appear "tiny black holes in condensed matter" to induce cold fusion as chain reaction. This looks analogous to an appearance (hoping) of MMP. A tiny black hole or MMP would eat everything meeting into its stomach to destroy matter along its tour. Fortunately, we know such events never happen on our planet. Man can make a Gedanken experiment. Free imagination is a given ability for human-beings. But Gedanken Experiment should be cross checked for its logical consequences, if being consistent strictly with known knowledge and facts, and be ruled out of illusions and delusions.

## Session-2: Gas Loading Experiments and related topics

In the afternoon of March 21, we had very interesting reports on anomalies by D(H)-gas loading to Pd-nano powders arranged with various metal-oxide flakes, as reported by T. Hioki (Toyota Central Research Lab., Japan), D. A. Kidwell (NRL, USA), F. Celani (INFN, Italy) and A. Kitamura (Kobe U. and Technova joint work). M. Miles gave a talk on SPAWAR type Pd-D co-deposition electrolysis and a new calorimetry system. W. Williams (UC Berckley) reported a result by fast pulse (ns) electrolysis of light water cell. F. Sarto (ENEA, Italy) reported important material issues for excess heat observation by super-wave electrolysis (See also the part of report on SRI tour and seminar).

**T. Hioki** presented a series of results on D(H)-gas loading for Pd nano-powders incorporated in $Al_2O_3$ and $ZrO_2$ base. He observed saturated values of loading ratios D(H)/Pd around 2.7 for the first runs (virgin sample runs) by D-loading, and released heat levels were over 2.0 eV/Pd-atom. However, repeated use of same samples after evacuation and baking procedure provided very decreased values of loading; D(H)/Pd =0.7; and heat level (0.2eV/Pd-atom). He concluded that "big" heat levels in the $1^{st}$ runs were due to the oxidation (formation of water) by D(H) charging, as original $Pd/ZrO_2$ and $Pd/Al_2O_3$ samples contained 100% PdO. And 2.0 out of 2.7 for D/Pd ratios were due to $D_2O$ formation and liquidation in gas phase of reaction cell chamber. This analysis is in contrast to the forced oxidation experiment by Kitamura group.
A typical heat evolution data obtained by Hioki group using a 54 g $Pd/ZrO_2$ sample from Santoku Co. (same origin sample for Kitamura group) is shown in **Fig.17**. Total heat about 2.4 eV per Pd atom shows a

close value to that by Kitamura group. The observed time-dependent pattern of heat-power (see **Fig.17**) has an sharp peak in the beginning of D-charging and bump later, which resembles with the data by Kitamura group (see Fig.11). Such a heat-power pattern for D-charging may be attributed nuclear heating events by surface 4D/TSC reactions as shown in Fig.10.

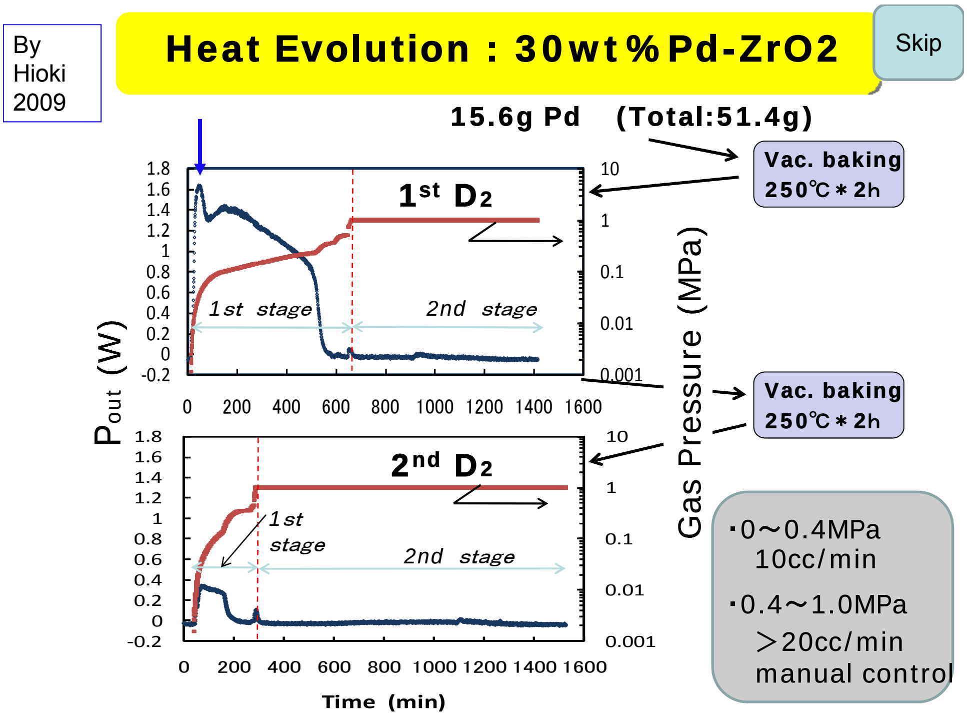



Fig.17: Heat evolution data by Hioki for Pd/$ZrO_2$ sample

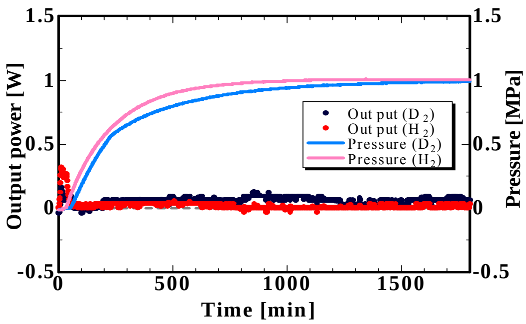
**D. A. Kidwell, et al (NRL)** made an interesting presentation on "Does gas loading produce anomalous heat?" His group used Pd nano-powders of 2-5 nm diameter kept in zeolite base.  Zeolite has porous structure with many nano-holes where Pd nano-particles are trapped. He made D-gas charging and H-gas charging alternately and repeatedly. Heat levels by many cycles showed higher values for D-charging than H-charging. Especially, heat release at initial runs for D-charging gave several times (8 fold at most) larger values than those for H-charging. This anomalously large heat by D-charging, he says, cannot be chemical. He did not say "nuclear" definitely.

I think, the phenomena and underlying mechanisms for giving high D(H)/Pd loading ratios and anomalously high heat levels with nano-Pd particles are common (same) for various trials, as Arata-Zhang, Kitamura-group, NRL group, Case-type (SRI, see the section of SRI Tour), Celani's nano-coated wire, and so on. This is one of most hopeful process for studying CMNE mechanisms at the moment.

**Kitamura, et al (Kobe U. – Technova collaboration)** reported also interesting results by D(H)-gas loading method with various nano-Pd contained power samples (PZ, PNZ and NZ). Here PZ denotes

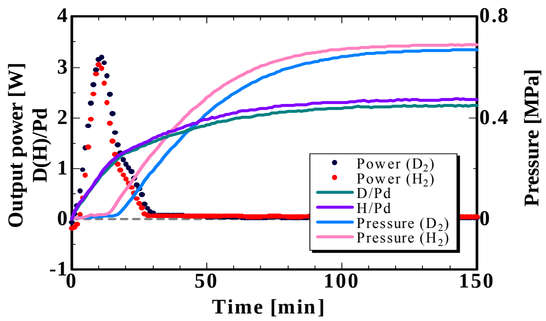
Pd/$ZrO_2$, PNZ does PdNi/$ZrO_2$, and NZ does Ni/$ZrO_2$ mixed oxide samples. They used a twin (A1 and A2) system for simultaneous parallel runs for D-gas charging and H-gas charging. The twin system can provide convincing data if we see difference between D-run and H-run. They observed no apparent loading, namely D(H)/Pd ratios zero, for NZ samples. The results mean that Ni and $ZrO_2$ do not active for D(H)-absorption and accordingly heat evolution. Impurities in $ZrO_2$ base samples did not make affect on D(H)-loading and heat evolution, either. PNZ samples from Santoku Co. gave them no apparent increase of heat level as claimed by Arata group at ICCF15. They observed heat levels proportional to amount of Pd atoms in used PNZ samples, and reproducing similar results as PZ samples (Kitamura PLA paper, 2009). Kitamura reported two new findings this time: 1) Forced-oxidation of used PZ and PNZ samples showed remarkable recovery of D(H)/Pd loading ratios approaching to 1.0 and high heat release levels c.f. virgin samples. And 2) Time-dependent D(H)-loading ratios were first time measured and it revealed the existence of "new second phase" of adsorption/absorption after the 1$^{st}$ phase. Major heat release ends with the end of the 1$^{st}$ phase where D(H)/Pd ratios reach at 1.1-1.2, namely over full-lattice loading. In the new second phase, heat release level is weak (on the level of 1/10 of the 1$^{st}$ phase, or so) but before saturation of loading another 1.0-1.3 D(H)-loading ratios are added. Consequently total loading ratios over the 1$^{st}$ and new second phases exceeded 2.0. I think, claimed large loading ratios as 2.5-3.0 by Arata, Hioki, and others would be attributed to this two components process. The D(H)-loading (over 1.0) in the second phase may be due to some "physical adsorption" process as surface phenomena of nano-particle or P-Z interfaces. I am considering some Van-der-Waals force.

**b) Data by used PZ samples**




PZ3,4#2

**c) Data by used PZ samples after forced oxidation (about 4% PdO)**




PZ1,2#5 (470K-10h $O_2$)

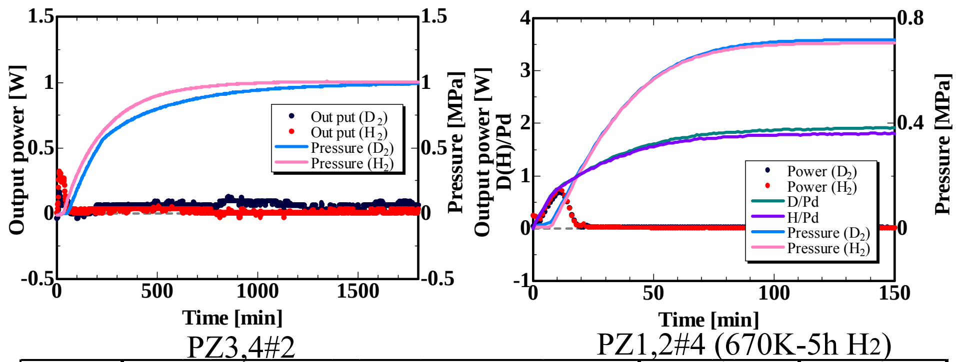
| Run number | 1st phase | | | | 3rd phase | Remarks |
|---|---|---|---|---|---|---|
| | Output energy $E_1$ [eV/atom-Pd] (=$xQ_R+yQ_D$) | $Q_D$ [eV/atom-DorH] | $xQ_R$ [eV/atom-Pd] | $D/Pd$ or $H/Pd$ (=$y$) | Output energy $E_2$ [eV/atom-Pd] | |
| D-PZ3#2 | 0.05 ± 0.01 | - | - | - | 2.49 ± 0.67 | |
| H-PZ4#2 | 0.23 ± 0.02 | - | - | - | 0.62 ± 0.67 | |
| D-PZ2#5 | 0.95 ± 0.01 | 0.85 ± 0.01 | 7.55E-02 | 1.11 | 0.84 ± 0.61 | 470K-10h $O_2$ |
| H-PZ1#5 | 0.84 ± 0.01 | 0.82 ± 0.01 | 6.53E-02 | 1.02 | 2.33 ± 0.71 | |

Fig.18: Typical results by used sample (left) and forced oxidation sample (right). Recovery of D(H)-loading ratios and heat levels in the 1$^{st}$ phases are remarkable. Time-dependent loading ratios have break-points at the end of the 1$^{st}$

phases.

There might be happening in the new second phase an over-loading into T-sites of local Pd Bloch potentials. We need to study the underlying physics there by future research. In **Fig.18,** typical results by forced-oxidation samples are shown. In comparison, they made runs with forced-reduction of oxygen from PdO of used PZ and PNZ samples. The results are shown in **Fig.19**.

**b) Data by used PZ samples** **d) Data by used PZ samples after forced de-oxidation**




| Run number | 1st phase: Output energy $E_1$ [eV/atom-Pd] (=$xQ_R+yQ_D$) | 1st phase: $Q_D$ [eV/atom-DorH] | 1st phase: $xQ_R$ [eV/atom-Pd] | 1st phase: D/Pd or H/Pd (=$y$) | 3rd phase: Output energy $E_2$ [eV/atom-Pd] | Remarks |
|---|---|---|---|---|---|---|
| D-PZ3#2 | 0.05 ± 0.01 | - | - | - | 2.49 ± 0.67 | |
| H-PZ4#2 | 0.23 ± 0.02 | - | - | - | 0.62 ± 0.67 | |
| D-PZ1#4 | 0.19 ± 0.01 | 0.30 ± 0.01 | 0.00E+00 | 0.66 | 1.52 ± 1.42 | 670K-5h $H_2$ |
| H-PZ2#4 | 0.19 ± 0.01 | 0.29 ± 0.01 | 0.00E+00 | 0.66 | 0.28 ± 1.20 | |

Fig.19: Results of forced de-oxidation for used PZ samples. Heat levels and D(H)/Pd ratios decreased to be "bulk" Pd values (0.2eV/D) with component of surface adsorption (0.5eV/D) as 0.30 totally.

As we added only 4% PdO to PZ samples for Fig.18, heat by formation of $D_2O(H_2O)$ when D(H)-charging is less than 10% of observed heat in the 1$^{st}$ phase, even if we assume 100% water formation with O-atoms in PdO. Thus, we can conclude that the forced oxidation induced recovery of anomalously high loading and heat release as those by virgin samples.

Kitamura showed data for time-dependent D(H)-absorption rate which was deduced as difference of saturated loading ratio minus time-dependent loading ratio, as shown in **Fig.20**.

**1)Time-dependent absorption rate in Phase-I is almost same between D-run and H-run.**
**2) In Phase-II, slower "physical adsorption process" exists.**

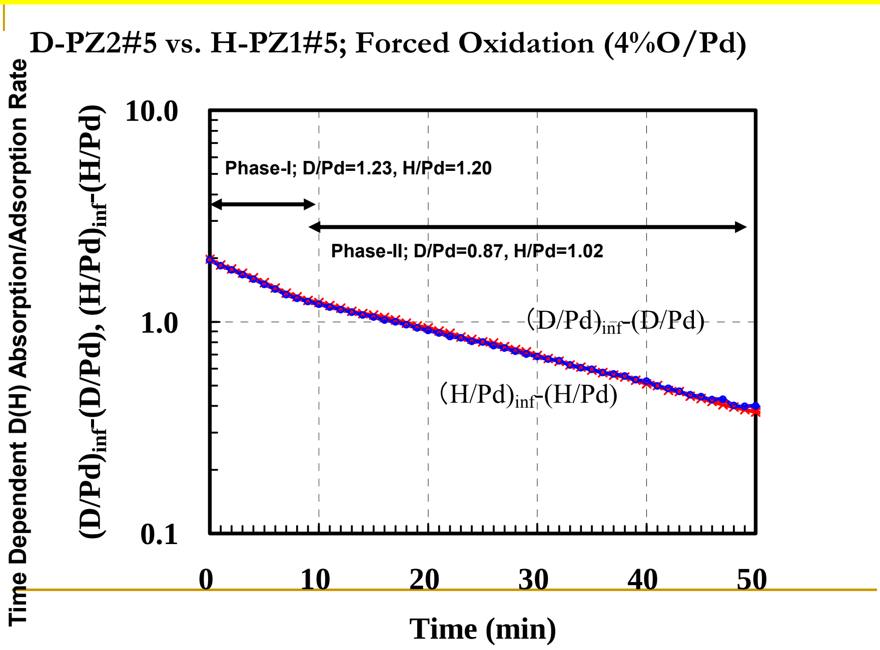



Fig.20: Time-dependent D(H)-absorption rate curves observed for forced-oxidation samples. There appeared two exponential components; faster absorption in the 1st phase and slower absorption in the second phase.

In the first phase, Takahashi proposed a phenomenological model as shown in Figs. 9 through 14.
These two new findings by Kitamura et al provides us a strong tool to study underlying dynamic mechanisms of D(H)-charging to mesoscopic nano-metal samples, so that we will need further systematic study for many other samples with different conditions.

## CONCLUSIONS

1) It was experimentally proved that, under specific conditions, it is possible to generate anomalous excess heat at macroscopic levels (several Watt or up to 400W/g of Pd).

2) According to our experiments, in the Pd-Deuterium system,
the key parameter that controls the amount of anomalous heat generated
**is the "moving" of Deuterium into or from the Pd bulk and/or nano-coating.**
In other words, it is necessary to
maximize the amount of Deuterium moving and its speed.
Such effect, although discovered (by us) since 1993,
was quite difficult to be obtained in practical experiments.

3) The method of coating some specific nano-materials (including Pd at very low dimensionality) over a long and thin **active** (from the point of view of Deuterium absorption) Pd **wire** seems a reliable procedure, at least at Laboratory level, to fulfil the request of reproducible experiments.

4) We experienced that the high temperatures (of the order of 300°C and over), even under mild pressures (4-8Atm), are pre-conditions to get some macroscopic effect.

5) We experienced that high temperatures, by itself, increase the effects, especially **over 700°C**. **Such effect was by us predicted and experimentally found.**

6) We experienced that electro-migration, especially at high current density (>20kA/$cm^2$), improves, in the whole, the effect.

7) We experienced also that the coating of nano-materials is a procedure that allows, in an easier way, the detection of the anomalous effects, thermal included.

8) The power density (up to 400W/g of Pd) and time duration (in principle no time limit, experienced by us over 1 week) of anomalous heat generated pushed us to think that the origin of such anomalous heat is ***out of any known chemical reaction***. Remarkably, the anomalous heat increased over the numbers of ON-OFF cycles, probably because were increased the numbers of fractals at the surface (then further reduced dimensionality). It is the, well-known, phenomenon of both **embrittlement** of Pd (and of several other metals, alloys) and large **changing of dimensionality** (see specific our report at ICCF15) due to Hydrogen/Deuterium loading de-loading cycles. Drawback of such (simple) procedure is the possibility that the wire broke, especially if it is thin. So, we made some specific "developments" to reduce such catastrophic final result.

Fig.21: Conclusions by Celani for his ACS2010 presentation

**F. Celani**, et al (INFN, Italy) gave two talks on D(H)-gas loading experiments with nano-coating of thin Pd wires operated at some high temperatures as 400-500 C. When we will apply the CMNE heat release for power producing devices for industry, we will have to use as high temperature as possible because of conversion efficiency to electricity. In this regard, the approach by the Celani group is important. I borrow his conclusion in **Fig.21**.

**F. Sarto and E. Castagna** from ENEA, Italy made very nice presentation about material issues for improving reproducibility of excess heat events. More detail contents are shown in the part of Tour to SRI, later.

**M. H. Miles** gave two talks on co-deposition electrolysis method. He said, now reproducibility of excess heat by Pd-D co-deposition method is 100%. In addition, he explained a new isoperibolic calorimeter which can make a very precise and stable calorimetry, by using a double tube of copper between which thermal insulator is packed and inside an inner tube he sets up co-deposition electrolysis cell.

**Jan Marwan**, the organizer of ACS-ENVR-NET 2010 kindly invited participants to an evening (19-21:00) reception at the same hotel Parc 55. Participants got together and exchanged information on CMNE research and fun.

**Session-3: Transmutation and related topics**

**P. A. Mosier-Boss** reported a further progress in their SPAWAR group research by Pd/D co-deposition method. They are also trying runs by the Energetics super-wave electrolysis method. They are repeatedly observing triple track events by CR39, as regarded events of $^{12}C(n,n')3alpha$ reactions by incidence of high energy (more than 9.6 MeV of that reaction threshold) neutrons, probably by DT neutrons "specifically" induced in the PdDx metal-lattice of condensed matter. In addition, they are using a neutron bubble counter which is known very noise-insensitive. They observed a slight count increase by the bubble counter over natural neutron background events. They also used a NaI detector for gamma-ray spectroscopy and observed eventually short lived "strange peaks".

**M. Srinivasan (BARC, retired, India)** prior announced the on-going preparation situation of ICCF16, February 2011, Chennai India. He presented executive summary of BARC cold fusion efforts in 1989-1990. Especially, he stressed claims of anomalous neutron emission and tritium generation, sometimes as burst events. Deviation from stochastic Poisson distribution of signals would have told, he argues, anomalous neutron emissions. BARC is the first observer of anomalous n/T yield ratio around $10^{-7}$. We know n/T ratio for d-d fusion is 1.0.

I think most neutron measurements in 1989-1990 were done by $BF_3$ and/or He-3 counters which have no good capability against noise reduction. If we repeat again similar experiments, we need more sophisticated techniques as the n-gamma pulse shape separation using a liquid organic scintillation detector as NE or Bicron products. LSC (liquid scintillation counting) and imaging-plate method are also useful for tritium detection.

We all miss late **Andrei Lipson**, who died in last November just after the ICCF15 conference in Rome. G. H. Miley, who was the host of Andrei during his research stay in University of Illinois, made tribute to Andrei and briefly introduced what he planned to talk at this meeting. Especially, electron beam irradiation experiment with PdO/Pd/PdO/Pd multi-layered target, by charged particle detection of CR39 detector which was his special favor for last 20 years.

**L. I. Ultsukoev, et al** (Moscow State University) presented a paper on "Observation of abnormal quantity of hydrogen under electrical titan explosion in liquid". He claimed anomalous amount of $H_2$ gas production by the explosion where they did not have much $H_2$ gas. He claims it was by nuclear (LENR) effect. Strange results were reported, as Mizuno's who did not show up this time.

**John C. Fisher** is now near 90 years old and still so active to present a paper. People have to respect his strong mission. He believes a line of "neutron-isotopes" along Z=0 and N=N line of the chart of nuclides. If it were so, neutron isotope as "poly-neutron" state can make freely-of-Coulomb-force nuclear reactions with meeting nuclei of condensed matter and most experimental claims by CMNS/LENR people could be consistently explained. He has extended models of possible reaction channels for many cases for many years. He assumes such reaction as $^{A+4}n$ to $^{A}n$ + $^{4}He$ is typical for 4D to 4H + $^{4}He$ + 20MeV.

I have exchanged discussion with him for several years, as I do not believe there exists any sticking force to confine many neutrons in a nuclear scale potential well. Between neutrons there are no charged pion exchange (iso-spin exchange) processes for sticking, but scattering by exchanging neutral pion. Between n and p states of nucleus, we have strong exchange force by exchanging iso-spin, namely Hamada-Johnston pion exchange potential as written in standard nuclear physics text books, which makes a global optical potential (Woods-Saxson) well for many nucleons system and the state can be treated as a "liquid drop" or a "independent particle state" under the global potential well confinement. There is no reason for neutrons to have similar confinement potential to make a "neutron-liquid drop". Once I asked him; what is sticking force between neutrons, how much is the inter-neutron distance of the "liquid drop", how much mass-defect happened to form a "neutron liquid drop", and so forth. Many questions are reserved non-replied. No definite answers came back to me up to now. But he can imagine and he goes his way.

**John Dash**, who worked with Bockris and Fleischmann before the cold fusion saga, is continuing experiments at PSU, Portland USA. He made a presentation titled "Anomalous elements on the cathode surface after aqueous electrolysis". He studied cathode surface structure by using an atomic force microscope (AFM). He also used SIMS for analysis of isotope distribution of cathode before and after run. He claims some anomaly in isotopic abundance ratios.

**V. Vysotskii** (Kiev National Univ., Physics) reported on "Observation of radiation and transmutation process of bubble cavitation in free water jet". He claimed to have observed anomalous X-ray (in 1.0-1.5keV) and optical light emission, probably induced by shock waves of water jet. Radiation was so penetrative to pass through 1cm thick iron plate-shield. Gamma-rays were not measurable. What was the penetrative "radiation". I wonder if shock wave caused some affect to films used.

**R. Stringham** presented his paper using a nice DVD narrated by himself. Model of sonofusion was his title. We well know he has been claiming D-cluster fusion by BDS (BEC plus electrons) to produce heat and $^{4}He$ ash. His model is a primitive phenomenology, but is easy to watch and listen.

**Session-4: Innovative approaches**

I omit several non-CMNS papers.

**V. Violante (ENEA, Italy)**, chairman of ICCF15, presented a paper titled "Material science behind the Fleischmann & Pons effect". He made analysis on the change of chemical potential in PdDx lattice, using Yenyo equation with trace of stress tensor. He analyzed mass-transfer at grain-grain boundary, effect of crystal orientation, [100], [110], etc. He found PSD (power spectral density) of surface structure had sharp peaks for Pd materials which showed excess heat, while non-excess heat material had rather flat PSD spectra. This may mean some periodical surface structure favored.

**F. Tanzella**, co-chairman of NET, presented an interesting experiment titled "Cryogenic calorimetry of "exploding" PdDx wires". Axial current through PdDx wire induces high loading ratio as claimed by Mengoli, de Ninno, Celani, Tripodi, and others. His group is also trying a case-type gas loading experiment using a $SiO_2$ plus nano-Pd. They are observing a change of $^{3}He/^{4}He$ ratio for sampled gas in a cell chamber to be analyzed by a dipole type mass spectrometer. I later visited, on March 23, SRI to learn the apparatus. They are seeing about 0.4J heat bursts in a second peak in this experiment. The heat level 0.4 J corresponds to $^{4}He$ ash atoms of $10^{11}$, which is a difficult level for mass analysis by usual techniques, but his group can resolve this. Good!

**V. Vysotskii** presented a theoretical paper on "dynamic resonant screening of Coulomb barrier", using a time-dependent Gaussian wave-function with resonating relative coordinates form parameters. I did not understand the detail in so short time of presentation, but his direction of approach intending transient process looked going on good direction.

**Denis Letts and P. Hagelstein** are doing collaboration in Texas for "Observation of excess power and isotope effect using D-Pd co-deposition method". They used a new calorimetry system with about 10 min time-resolution (inditial response). They observed 200-800 mW excess heat-power. It is interesting that the SPAWAR co-deposition technique is employed for replication experiments in several places in USA.

Together with several activity groups in USA using gas-loading nano-Pd system, this is a new trend of our CMNS field encouraging. Mike McKubre told me, The Energetics group is moving now to U. Missouri, c/o Prof. R. Duncan, for further R & D. Interesting!

**M. McKubre** gave us his "traditional" talk on his discovered empirical formula of excess heat evolution with three conditions; namely, current density (electrolysis) over a threshold, loading ratio over a threshold and a deuteron flux. He stressed negative and discouraging reports from MIT, Bell Lab, Cal Tech, and so on of leading institutions in 1989 were all wrong as those were not satisfying above three conditions and the "long waiting time" for incubating necessary conditions.

**J. Marwan**, the organizer of NET, gave lastly his interesting work for fabricating Pd nano-structure designed materials and its electro-chemical performances. We would expect, someday, an established recipe for producing and controlling "nuclear excess heat" using designed materials will be provided for customers.   A dream or reality!

Not joining a LENR dinner on that evening, we Kitamura, Hioki and me were wandering through Powel Street with cloud of people, through Union Square and got to the main gate of China Town. In front of the China town main gate, I found Hotel TRITON still there, which I stayed sometimes with my fusion neutronics Japanese colleagues, in 1980s, for launching to LLNL (Lawrence Livermore National Laboratory, beyond the Bay) for US-Japan Workshops on fusion neutronics and related nuclear data.
For our three old Japanese, "San Francisco's China Town, so wet with a dense night fog, and I am waiting for my dear…..", a lyric of Japanese popular song, maybe in 1950s, came to humming in our ears. This is a love song so far, but we three aged Japanese were wandered through shops, nothing to buy, got merely hungry to drop in a Chinese restaurant Xi-Hai (four sees). We were satisfied with good taste genuine Chinese foods. The restaurant was full with young Americans.

## Tour to SRI

Taking a local train, CalTrain, we went south to get off at Menlo Park Station. We enjoyed walk from the station to SRI along a nice avenue of Ravenwood. Menlo Park city is a beautiful and peaceful area with full of trees. On March 23, we saw blooming cherry and plume blossoms. Mike said, here we have 90% days of a year "mild summer", so far never wants to move to other cities. Yes, it's easy to understand that words and feeling by pleasant atmosphere there. A shot in his laboratory of Physical Science Building where Mike McKubre is involved in CMNS/LENR works is shown in **Photo-2**.

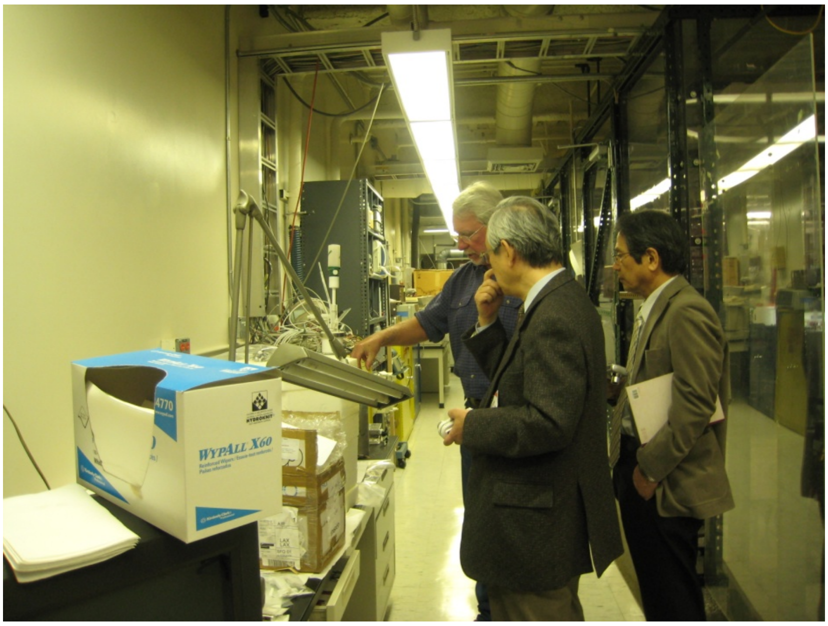



Photo-2: Mike McKubre is guiding his laboratory to Takahashi and Hioki, photo taken by Kitamura

We were impressed with seeing plural experimental systems are running in the laboratory. A super-wave electrolysis, a Case-type gas loading cell for excess heat and $^3$He/$^4$He ratio observation (by a dipole mass spectrometer), Miles-type dual tube calorimeter, an anti-Compton gamma-ray spectrometer, and so on. The SRI activity is still one of vital places in the world.

There we eventually met the ENEA Violante group, visited SRI also, V. Violante, F. Sarto, E. Castagna, and two technicians. At lunch time with out-door table, so comfortable with mild wind through green trees and warm sun-shine, P. J. King (Ireland) joined us.

In the afternoon, we enjoyed an "informal" seminar. Kitamura first presented what he did at ACS-NET starting our previous results published in PLA 2009, as typical data are shown in **Fig.22**. I followed him with my presentation at JCF10 "Role of PdO surface coating of Pd nano-Particle for D(H) charging and cluster fusion". Main results are already shown by Figs. 9 through 20. My nuclear and condensed matter physics view, for underlying mechanics with nuclear and atomic-molecular-solid-state potentials, from a few body interaction system, via D-cluster system and mesoscopic nano-particle system and a bulk lattice system, is flow-charted in **Fig.23**.

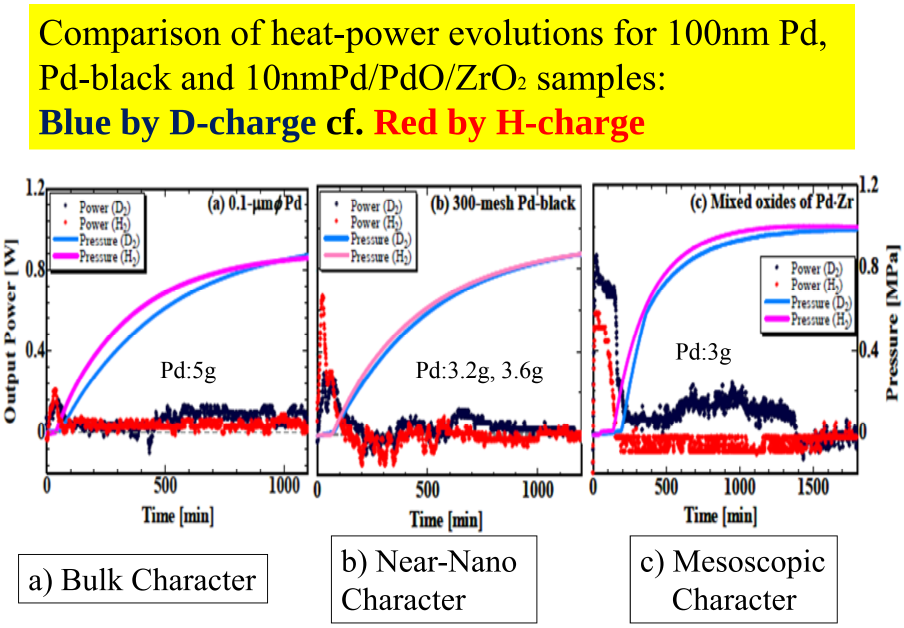



Fig.22: Typical data of D(H) charging with Pd three kinds of powders in twin cell

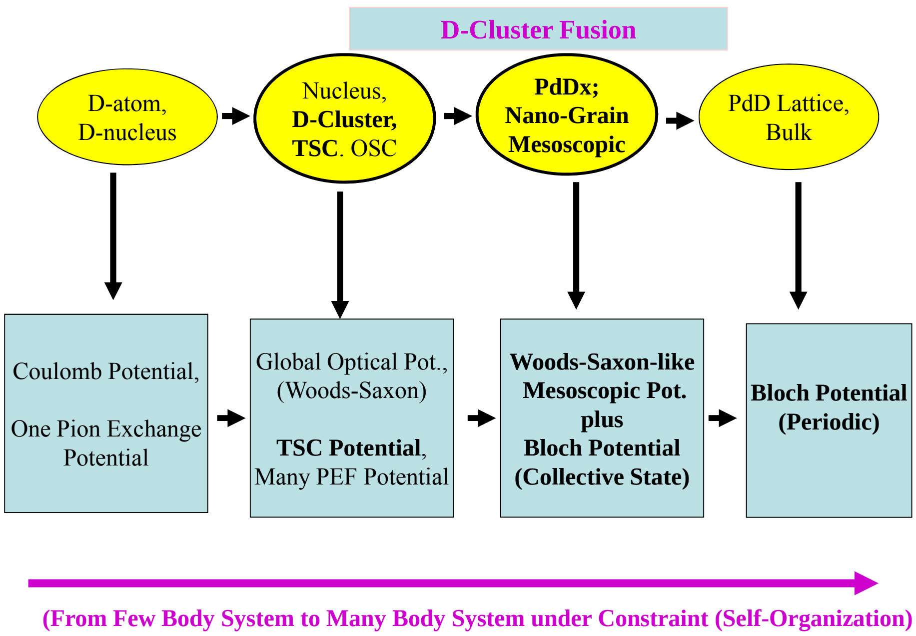



Fig.23: Variation (speculative) of nuclear and atomic-molecular-lattice potentials from a few body deuteron system to cluster, mesoscopic nano-particle and bulk lattice.

Specific nature of condensed matter, compared with gas and plasma, is the ordering process by constraint (or self-organization), dynamics of which may induce specific transient BEC conditions to induce D-clusters on surface and at lattice, and simultaneous D-cluster fusion reactions emitting cleanly released nuclear energy without (almost) hard radiations.
Hioki made a presentation using same ppt he used at ACS.
Mike, Fran, Vittorio, Francesca,….. all looked interested in Japanese works for gas-loading methods.

Francesca Sarto and E. Castagna also repeated same talks as ACS. As Mike had no time to listen our presentations at the occasion of ACS-NET, he said he was enjoyed much our talks, as well as Fran and a Spanish lady (Mike's technician).

P. J. King was so kind to take us three by his hired car to drive back to San Francisco down town.
PJ said, if your theory hits right target, CMNS.LENR/CF is very hopeful to develop clean portable high density energy devices. Conversation continued on hot fusion projects as ITER and NIF. PJ and I agreed with a view that hot fusion DT reactors will be very difficult to commercialize as electricity producing power plants, because of most troublesome tritium treatment and radiation damage and long-lived activation of reactor materials by 14 MeV neutrons. However, hot fusioners will survive as far as big money flow continues. Our human-beings like to live in an easy niche, and a few people try to go through a "narrow gate".

Having said thanks and good-bye to PJ, we three Japanese aged went to a Japanese sushi restaurant at Hotel Nikko, so as to change surfaces of our tongues.

My plane delayed and arrived in Narita in cold (5 deg C) night rain. I changed a flight to Itami, where a cold night was waiting too. I would be lucky to see a blooming cherry blossom season in Osaka soon.

PS: This report is written in a hurry and no editing was made by English mother tongue. Please forgive me mistakes in English and maybe in explaining what other researchers presented at ACS-NET.