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310) Alchemy versus CMNS?

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This unit is an update on what has been described in unit #186.

In what follows I am using blue to quote myself and green to quote others.

Introduction:

On Friday October 13 of 2006, Roberto Monti, an independent alchemy researcher, posted a message on a restricted Internet list. He thinks that some ideas, usually attributed to John Fisher, should be attributed to him. A dispute about priority is not worth discussing unless the idea becomes part of generally accepted science. This message was prompted by Roberto's website that was given as a reference.

<http://www.lowenergytransmutations.org/papers.htm>

A real alchemist among us

In a subsequent message he wrote "my website has had more than 9000 visitors and more than 22000 pages have been downloaded (since 2004, when we decided to put there a counter)." I went to the website and downloaded his second file. After reading it I posted the following comment on the restricted Internet list for CMNS researchers.

. . . Referring to the method described in (17) Roberto wrote:

We washed, following Geber's instructions, 500g of Mercury in very good Vinegar, obtained mixing 1/2 liter of Vinegar with 1/2 liter of Acetic Acid for 2 - 3 months. Then we took 100g of the washed Mercury from the 500g. Once dissolved in Nitric Acid 1 to 5, the 100g showed 55mg of Gold crystals. After 2 more months 100g of the same lot of 500g, dissolved in Nitric, gave 88mg of Gold crystals. Analyzed by SEM and ICP the Gold crystals showed to be from 100% to 90% Gold. The most beautiful we have ever seen (see photographs) (18).

17) I. Filalete, *Open Entrance to the Closed Palace of the King*. Phoenix, Genova 1987, p.11.

18) R. A. Monti, G. A. Cesarano-Monti, *Metallic Transmutations induced by Acetic Acid*. J. N. E.

What is not clear to me is who are "we" in the first sentence above. Is it Roberto et al. or is it Filalete et al.? The J.N.E. probably stands for the Journal of New Energy. When was the (18) published? Was it discussed at earlier ICCF gatherings? What did other CF researchers say about it? Roberto also wrote:

On April 1992 John Bockris invited R. A. Monti to join and witness experimental tests suggested by Joe Champion (The Philadelphia Project). The first test showed immediately what was going on: it was clearly an attempt to replicate the *Twelfth Key of the dry way to the Philosopher stone* of Basil Valentine (19).

Bockris and Champion knew very little about Alchemy. Fortunately April is *the right season* for Transmutations. Consequently the experiments showed definitely - during April and May - the production of Gold and other Noble Elements from the ignition of a mixture of Metallic Salts and other Elements (20).

In September 1992 the Philadelphia Project finished.

J. Bockris is often described as a leading electrochemist of the past century. Ed, I think that his alchemy experiments, and those described by Roberto, should not be ignored in your upcoming book about cold fusion. These people, like Piantelli, Patterson and Case (mentioned by Peter Gluck this morning), were part of cold fusion history. Let me again suggest that you write a special chapter devoted to their ideas (even if you disagree with them). Likewise, the commercial applications project described at ICCF11 by E. Anderson, or that described at ICCF12 by S. Krivit, should not be ignored. Let me end by quoting Roberto's paper again, for those who found it difficult to download the 52 Mb file.

After ICCF-5 (Monte Carlo, 1995) it was necessary to have similar experiments repeated in independent laboratories for the *validation* of the process . In 1996 an industrial reactor was built in Canada and sent to Italy for a new series of independent tests at ENEA, Saluggia (Italian National Laboratories). In these tests the production of Silver from Lead was used as a driver of the transmutation of Thorium and Uranium. Thorium was reduced by 88% . Uranium by 30% (23).

To make a further demonstration of the reality of these experimental results a new series of tests was carried out on May 21 and May 25 1998. The first experiment (May 21) showed the transmutation of 1.32g (30% of the total) of Uranium (23). The uncertainty declared by the laboratory was between 5 and 10% . To avoid any possibility of error we decided to show the possibility to increase this result through a slight change in the proprietary formula used, suggested by the Alpha Extended Model of the Atom (1), (5). Consequently a second test was carried out on May 25, 1998, with the only addition of 50g of SiO₂ (powder) in the same composition used for the test of May 21. The result was the transmutation of 2.07g of Uranium (45% of the total). An increase of 15% (+ 50% compared to the test of May 21). . . .

In 2002 (October 9) we made another demonstration at the Royal Institute of Technology (Stockholm, Sweden). It was the first time we tried a test, with thorium, in the second window of the year. Three ignitions were made (see the videotape and History section). The samples obtained from the first ignition were taken, for analysis, by G. Godowski . The results never returned (disappeared). The samples from the second and the third ignition were taken by J. Coleman and analyzed by R. N. Barnes (26). A very interesting result came out: using the mixture with 50g of SiO₂ also the thorium in the slag was destroyed (about 100% of the total). We need, obviously, more tests made in the second window of the year, to verify this experimental result. . . .

A comment:

If highly radioactive wastes from nuclear reactors could be turned into non-radioactive substances then the main objection to nuclear energy from fission reactors would disappear. That would be a totally unexpected, and highly appreciated, gift to mankind from CMNS community. I do not think that this is possible without using highly intense sources of neutrons, as suggested, for example, by Carlo Rubia at CERN. But I would be happy to change my mind. Godowski, who you mentioned, was (is?) deeply committed to Rubia's project.

Replying to the above Dr. Roberto Monti wrote:

Filalete published his book in 1645. Or you are kidding or you show deep ignorance of the history of alchemy. My website is enough. I do not need any further publicity. There is even too much Information. I know that many of the experiments that I suggested, have already been repeated successfully by many people. The French, in particular, have a peculiar attitude: whenever one of my tests is successful, they put on it: "Classified." My reply: "Yes, I had no idea who Filalete was. What confused me was the date at your reference #17"

Is alchemy part of CMNS?:

Then, addressing Ed Storms, who is writing a book about cold fusion, I wrote: "I think that his [Bockris'] alchemy experiments, and those described by Roberto, should not be ignored in your upcoming book about cold fusion. These people, like Piantelli, Patterson and Case (mentioned by Peter Gluck this morning), were part of cold fusion history. Let me again suggest that you write a special chapter devoted to their ideas (even if you disagree with them). Likewise, the commercial applications project described at ICCF11 by E. Anderson, or that described at ICCF12 by S. Krivit, should not be ignored. Why should (or should not) a book about CF inform readers about all aspects of CMNS? Why to focus (or not focus) only on those ideas

which, the author believes, have a good chance of being accepted? Perhaps someone will answer these questions.

In the immediate reply Ed Storms wrote: “. . . My criteria is to discuss that which is done with sufficient skill and described with sufficient clarity to allow an educated person to make sense of the concept and accept that reality is being described, even when the reality is not understood.” Does this apply only to Monti or to Piantelli, Patterson and Case as well? In one of his messages Roberto mentioned that his papers, presented at ICCF11 and ICCF12, were not included in the proceedings. But, as I just verified, Case and Patterson are also not mentioned in the index of authors of ICCF11 papers. I did hear Case’s presentations at ICCF11. I suppose that the issue of publishing or not publishing certain papers was discussed among conference organizers. They probably decided that certain topics had nothing to do with CMNS. Or perhaps they were afraid that publishing alchemy papers would give ammunition to those who claim that all cold fusion researchers are practitioners of voodoo science.

Another voice:

Referring to an experiment performed by a friend, X -- he prefers to remain anonymous -- wrote: "Effectively in 2000 a test of transmutation, according to the protocol of our friend R. Monti has been performed by a friend of mine; he was the chief of a big R&D lab completely devoted in the field of explosives and ammunitions. . . . Two tests have been successful in transmuting Hg into Au. The total of the mixture was in the range of 50 grams. In the best test 0.6 grams of gold were recovered by the classical way of Aqua Regalis. The quality of Au has been tested by a mass spectrometer." Then X described bizarre circumstances under which the report was classified. The alchemically produced gold is now in two wedding rings.

Another comment:

Dr. Monti is apparently not the only one who takes ancient alchemy seriously. In my last contribution to this thread I wrote: “I suspect that most people on this list, like most mainstream scientists, have the same attitude to medieval alchemy as I have. We were educated to think that transmutations of elements by chemical means are not possible. And we all believe in nuclear alchemy. What would I do if I had a simple reproducible-on-demand demo of a chemical transmutation? Taking negative attitude toward old alchemy for granted, I would focus on the demo itself and not on the fact that it is based on what was described long time ago. Any chemist should be able to replicate a procedure that was used in 1645, provided it was properly described. I would say "I do not know why it happens; but it does happen. Please confirm or refute my claim before jumping to a conclusion." That would be more productive than anything else, at least for the time being.

Roberto, I would be happy to make your simple protocol known to science teachers. Please describe it clearly and I will post it on my website. But avoid the case of turning mercury into gold, many people would simply smile instead of performing experiments. You wrote: ‘. . . A very interesting result came out: using the mixture with 50g of SiO₂ also the Thorium in the slag was destroyed (about 100% of the total). We need, obviously, more tests made in the second window of the year, to verify this experimental result.’ Please describe the protocol for destroying thorium by a chemical procedure. And do not mention ‘the second window of the year.’ Most people expect reproducibility at any time.

No matter how strongly one disagrees with Monti it is important to keep in mind that “the proof is in the pudding. ” Suppose that a procedure to transform one element into another, by using chemical means, is confirmed by many experimentalists. That would be a proof and we should be able to accept it. The burden of offering the procedure, as always, should be on those who make claims, not on those who are asked to accept claims. Reality, however, is often more complicated. It has to do with prejudice, with money, with conservatism, with fanaticism, etc. etc.. Will I live long enough to witness rehabilitation of old alchemy? Most likely not. For the time being I will accept general belief that non-nuclear alchemy, like astrology, is pseudo-science.

Appended on 10/19/06:

1) This is about alleged discrimination (not publishing Monti’s ICCF11 report in the proceedings). Prompted by Roberto, the chairman of the conference, Jean Paul Biberian, wrote: “The reason your paper was not published in the proceedings of ICCF11 is simply that it was not a scientific paper. I was very open to publish papers at the border line of CMNS: I accepted a paper on alchemy by Pérez Pariente. . . “ Yes,

many controversial presentations (but not as controversial as making gold from mercury) were published in the proceedings.

2) Gold from mercury or gold in mercury? I know that everything is present in everything, in a traceable amount. Is it possible that tens of milligrams of gold, shown on the photos from Roberto web site, were initially present in mercury, or in the vinegar, used to produce tiny crystals? Why was the question not addressed in his paper? What is obvious to the author is not always obvious to readers.

3) As mentioned in the unit #186, Monti's process of destroying radioactive waste by chemically induced transmutations is at the base of an anticipated commercial project. We know nothing about that project, except that progress is being made. The company wants to keep low profile at this time. This is understandable, they do not want to reveal technological secrets to potential competitors. But there is also a negative aspect in such attitude; discussing scientific aspects with scientists on the CMNS list can be very helpful. That is probably a well known dilemma. Is it possible to discuss purely scientific aspects of a pending application without revealing the "how-to-do-it" details? Assuming that the answer to this question is positive, I want to ask some questions about the proposed transmutations of NORM (Naturally Occurring Radioactive Material). Presumably such materials will be mixed with industrial byproducts (tons of waste?).

(a) Is Roberto's transmutation process selective or does it have about the same efficiency for all NORMs: U, Th, Ra, Po, Rn, etc.? (*) Roberto describes efficiency in terms of the "decrease per ignition." For example, referring to 1998 experiments, he wrote: "The first experiment (May 21) showed the transmutation of 1.32g (30% of the total) of Uranium."

(b) What instruments are used to measure transmutation efficiencies?

(c) How many tests were performed in the last two years? How reproducible are the results?

(d) How certain is Roberto that the decrease in the activity of NORMs is due to transmutations of atomic nuclei rather than to their redistribution (changes in counting geometry)? Or how does he know that the decrease is not due to dispersion of radioactive materials into the environment?

One often hears the old English proverb about proof being in the pudding. But what kind of commercial pudding will convince us that Monti's claims are valid? It has to be a set of reproducible-on-demand experimental data and the description of the "reactor's" anatomy that most of us would consider to be free of artifacts. What else can it be, Roberto?

To make clear what artifacts I would look for, let me describe an ongoing experiment. For nearly 7 weeks I have been measuring radioactivity of a hyperthyroidism patient (my wife) who received 8 microcuries of the I-131. The half-life of that isotope is eight days. But a Geiger counter, situated in the same position with respect to the throat, was showing that the counting rate was decreasing much faster, especially at the beginning. The first measurement was taken about three days after the pill was taken. A naive person might conclude that Linda's body transmuted radioactive iodine into something non-radioactive. But that would be wrong. A more credible explanation is that a significant amount of iodine was biologically removed from the body (dispersion) or that the distribution of the iodine within the body was changing (counting geometry becoming less favorable). The first hypothesis was confirmed by the fact that Linda's saliva, for example, was found to be radioactive. How does Roberto know that dispersion and redistribution were not responsible for what he attributed to pyrolytically induced nuclear transmutations of uranium and thorium?

Scientific papers posted on our web sites should meet standards imposed on papers published in referred journals. Reasonable objections should be anticipated by the authors and addressed in what is published. The same should be true for papers presented at our conferences, as indicated by Jean Paul.

Appended on 10/21/06:

Here is what one reader of the above wrote to me in a private message last night:

“. . . Regarding mercury transmuting to gold: Read pages 540-542 in Strong's Procedures in Experimental Physics. This gives good reasons to be suspicious of claims of "transmutation", especially if one knows

nothing about the history of the mercury used in the experiment. Mercury amalgamates with many metals (as with silver, used for tooth fillings). The more likely explanation of the "transmutation" is contamination. If the experimenter does not know what is in his mercury before the experiment, then the results of the experiment must obviously be in question. Certainly the mercury of 1645 would be suspect. . . ." Yes, this observation is valid. The burden of proof is on Roberto. Perhaps he will share the results of chemical analysis of mercury used in transmutation experiments with other CMNS researchers. Mizuno, who also performed transmutation experiments, was very specific about purity of various materials, including water.

But contamination can no longer play a significant role in experiments during which nearly 50% of the total is transmuted, as in the case of Roberto's May 25, 1998 experiment with uranium (see above). In that case the suspected artifacts are dispersion and changes in the counting geometry. Note that if 45% of uranium (about 2 grams) was transmuted then about 2 grams of something else should have been created. What was it? I suspect that Roberto has no answer to this question. But he is certainly aware that the question is very important -- it is likely to be asked by critical readers. Did you try to demonstrate appearance of about two grams of a new substance, Roberto? That would make your transmutation claim much more credible than it is.

What did John Bockris have to say about contamination in his alchemy experiments? He is a worldwide authority on chemistry and electrochemistry -- a teacher of teachers, as someone wrote. I saw his textbook and I read that Fleischmann was his student. Is it conceivable that Bockris also ignored to address the issue of possible contamination? I do not think so.

Appended on October 24, 2006:

Let me mention that George Washington bridge, admired each day from the balcony of our new apartment, was inaugurated exactly 75 years ago. Was it simply a coincidence that I was born on the same day? An astrologist might think so -- and probably explain the coincidence in terms a stellar configuration . . . In a message posted on our restricted list, Bill Collis wrote:

"Your quite right in raising the question of dispersion and also asking about the instrumentation used to measure any transmutation. The two issues are connected. Both Th232 and U238 decay by alpha emission which penetrate Geiger counters with difficulty or not at all. Some gamma radiation is produced (see PCNUDAT on the ISCMNS CD-ROM/DVD-R) but at low energy and intensity. Put a Geiger counter near thorium or uranium and it buzzes frantically. This is due to gamma radiation from the beta decays of the daughters. You can demonstrate this easily by placing an absorber between the source and the Geiger tube. Elimination of radioactivity by "ignition" probably involves temperatures greater than 1500 degrees. Many of the radioactive daughters of uranium and thorium, such as Radium, Francium, Radon, Astatine, Polonium all boil below these temperatures. Of course we should also realize that the boiling point of their chemical compounds may be different. Nevertheless, the prospects for dispersion into the gas phase look rather high."

That is why a very hot piece of uranium might appear to be less radioactive than the same piece before heating. Was a possibility of such scenario discussed by Roberto? If so then I am not aware of it. Responding to Bill, I wrote: "I would like to know what Roberto thinks about the role of dispersion. Ideally, it should be prevented; less ideally (in a preliminary experiment), it should be accounted for. That is not a simple task. On his website Roberto wrote that nearly 50% of uranium -- about 2 grams -- was transmuted in May of 1998. Such outcome, if possible, would be very desirable. Roberto, what allowed you to conclude that transmutations were real?"

Will he reply to our questions? A well known nuclear scientists once wrote, many years ago, that "researchers have the responsibility to publish their own experimental data. They should be in a position to explain and defend their results to other qualified experts. This is especially true when the reported data are controversial and directly contradict well-established scientific results in the literature. The description of experiments and results should be published in sufficient detail to give the expert reader the possibility of evaluating the significance of the claimed result." How can one disagree with this? Personally, I do not think that Roberto paper contains good arguments for reality of chemically induced nuclear transmutation. And, for some reason, he is not willing to reply. That is not a good sign. Something is not right somewhere.

Appended on October 27, 2006:

A long awaited reply from Roberto was posted this morning. He addressed concerns of several CMNS researcher. But my questions (about dispersion and the counting method) were not addressed. Why is it so? In an earlier message Monti asked Bill Collis to translate his Italian papers. Here is the reply from Bill: “. . . As I wrote before, I'm quite happy to discuss your theoretical works privately. However I suspect they will not pass peer review and consequently any translation work will be in vain. I would suggest instead you write an experimental paper, describing transmutation. It should include proper blanks or controls. It should use nuclear instrumentation to verify claimed nuclear effects. Such a scientific paper, if you can write it, should certainly pass peer review. I will be happy to consider translating it, or correcting it, free of charge. But I'm not prepared to discuss translation of any kind, whether free of charge or for a fee, unless I first see the manuscript. I think that's fair isn't it?” I did not know that Roberto is also a theoretician. His paper about Einstein, shown at:

<http://www.lowenergytransmutations.org/papers.htm>

is just as strange as his experimental report about fabricating gold from mercury. I cannot accept Roberto's conclusion that Einstein goofed on special relativity. We know that equations of that theory have been shown valid many times. High energy accelerators, used in numerous laboratories, were designed with that theory. These complex devices work because the equations are correct. Perhaps someone will post a message evaluating Roberto's theories of transmutations. Let end with the following dead-end speculation about making gold.

It is a well known fact that 10% of natural mercury is the isotope ^{198}Hg . A stable ^{197}Au (common gold) would be created if one proton was emitted from the ^{198}Hg nucleus. What could be more simple than this? Unfortunately, the atomic masses involved in such emission are unfavorable:

^{198}Hg 197.96677 amu
 ^{197}Au 196.96655 amu
proton 1.00782 amu

The sum of the last two masses, 197.97437 amu, exceeds the mass of the ^{198}Hg by 0.00760 amu. In other words, spontaneous emission of protons from ^{198}Hg is energetically forbidden; the minimum energy needed to emit a proton is $m \cdot c^2 = 0.00760 \cdot 931.48 = 7.08$ MeV. Yes, I am using Einstein's famous $E = m \cdot c^2$ equation here. Is it possible that a chemical process can supply a ^{198}Hg nucleus with more than 7 MeV of energy? I do not think so. By the way, the binding energy of a proton in ^{198}Hg , exceeding 7 MeV, is not a wall-like barrier, it is a step-like energy threshold. That is why speculations about catalysts are likely to be fruitless. But what about billions of atoms, each contributing a tiny amount of thermal energy to a single ^{198}Hg nucleus? Something like that would help; if we knew how to concentrate thermal energy of many atoms in a nucleus of one atom.

P.S.

A message posted two days ago (on our restricted Internet list for CMNS researchers) referred to a paper published in the most recent issue of New Scientist. The title of that paper was: "The Cryogenic Model of Nuclear Fusion." Referring to this paper, and to a message about a relevant French patent, Roberto wrote about his much earlier paper on that model. His message contained the following prediction:

“. . . They shall have a bigger surprise when they will come to know that any radioactivity can be easily destroyed in 3 days.”

Here is my reply:

- 1) Are you referring to transmutations induced by neutrons? That can probably be done, but not "easily."
- 2) Yes, easy destruction of "any radioactivity" will be a big surprise. How soon will we experience this surprise?
- 3) Will the issues of possible artifacts be addressed?

Efficient destruction of highly radioactive isotopes, in spent fuel from nuclear reactors, would open a new era of nuclear electricity. By the way, rapid turning of radioactive isotopes into stable isotopes will rapidly

release usable energy. The process may be able to pay for itself. Let us hope for this.

Roberto, may I have permission to quote the above prediction on my website?

Permission to quote was given but, as before, my questions were not answered. Why is it so? Something is not right somewhere.

Appended on 11/6/06:

Apparently, Roberto decided not to answer my question about dispersing. But messages he did post on the CMNS list indicate that his "Cryogenic Model of Nuclear Fusion" (developed in 1987, two years before the discovery of CF was announced by Fleischmann and Pons) played an important role in subsequent investigations. The word "cryogenic" usually refers to temperatures near absolute zero. Roberto's paper is in Italian but it will be translated into English and posted on his personal website, in a month or so. If I understood posted messages correctly, the model claims that the temperature at the center of the sun is lower than at the surface. The surface temperature, only about 6000K, is certainly too low to allow for generation of hot fusion energy, and for conversion of hydrogen into helium. What will astrophysicists say about this model? Will they conclude, incorrectly, that all CMNS claims are as unrealistic as Roberto's? I hope not.

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