AN OUTLINE OF POLYNEUTRON THEORY

(with applications to "cold fusion" phenomena)

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First I accept some of the experimental work beginning with Fleischmann and Pons. I accept their observations of energy release in electrolysis experiments far in excess of what could be caused by any conceivable chemical reactions, and replications by many others including Dick Oriani. I accept generation of helium by McKubre and others in amounts proportional to the excess energy and exceeding the concentration of helium in the atmosphere. I accept generation of tritium as reported by Fritz Will and others. I accept generation of energetic charged particles, most convincingly in the giant shower that Oriani observed and I analyzed. I accept transmutation of elements, as for example cesium to praseodymium in the Iwamura experiments. To me these all are sound experiments that any "cold fusion" theory must be able to explain.

Second I accept that conventional nuclear experiment and theory are correct, and that reactions between charged nuclei cannot take place at room temperature. The coulomb barrier forbids it. This rules out fusion of ordinary nuclei as a cause of any of these phenomena. In consequence the new phenomena, if indeed they reveal aspects of new types of nuclear reactions, require the participation of neutral particles of some sort. We are familiar with this when the neutral particles are neutrons. They are able to spread the reaction from nucleus to nucleus, supporting chain reactions that are capable of amplifying the power of a single neutron to provide electricity for a city. But neutrons are rarely (if at all) present in cold fusion experiments. Hence some other neutral particle is required. This might be a new fundamental particle as suggested by Teller, or a neutron droplet (neutron isotope or polyneutron) as suggested by me.

In light of the above I asked the question: Is it possible to assign properties to polyneutrons such that they can play a role analogous to but distinct from that of neutrons in facilitating room temperature nuclear reactions? That is what I am trying to ascertain. The polyneutron model must account for many observations (and non-observations) and must give satisfactory answers to many secondary questions. Here are a few.

(1) Do neutrons actually stick together to make stable (except for beta decay) droplets? I assume that they do in spite of the fact that two neutrons do not bind, nor do three. From the beginning I have assumed that larger droplets are strongly bound by the BCS pairing symmetry. A couple of years ago a team at CERN reported evidence for weakly bound tetraneutrons, which was encouraging. My theory requires that the binding per neutron in large droplets (a hundred or more neutrons) is about half that per nucleon in ordinary nuclei.

(2) Since polyneutrons are radioactive (beta decay) they cannot long exist in nature. Where do they come from? Here there are two possibilities. It may be that a small one can be knocked off lead or uranium or some other heavy nucleus by cosmic ray spallation. Or it may be (my preferred alternative) that a polyneutron is occasionally emitted by decay of a radioactive precursor that is present in air and water. The theory

suggests massive oxygen or helium nuclei whose nuclear structure will be described below.

(3) Even if a polyneutron does appear in an electrolysis experiment, how can it be responsible for the large quantities of energy that are observed? Here I have to assume, in parallel with neutron-mediated reactions, that a chain reaction is responsible. The simplest experiment in which a chain reaction may have been observed is the shower of 150,000 or so alpha particles generated in the O_2+H_2+H_2O vapor above the electrolyte in an electrolysis experiment. It must be that polyneutrons interact with at least one of the isotopes ^{16}O, ^{17}O, ^{18}O, ^{1}H, ^{2}H in such a way that they grow in size until they are big enough to fission. Here I focus on ^{18}O, which is more common than ^{2}H, although the theory admits both of them as nuclear fuels. The mass of an ^{18}O nucleus exceeds that of a ^{16}O nucleus by 3.956 MeV. Consider the reaction $^{18}O + {}^{A}n \rightarrow {}^{16}O + {}^{(A+2)}n$. It will be exothermic provided the mass of $^{(A+2)}n$ is not more than 3.956 MeV greater than that of ^{A}n. I assume this to be so because of the necessity for exothermic growth. Repetition of the growth reaction, accompanied by occasional polyneutron fission, then leads to an exponential chain reaction.

(4) Why didn't Oriani's experiment blow up? Why doesn't liquid oxygen explode? Polyneutron theory seems to suggest they would. Here I suppose that the chain reaction is quenched by buildup of reaction products that absorb polyneutrons and poison the reaction. Analogous phenomena occur in ordinary nuclear reactors where reaction products build up and absorb neutrons, quenching the reaction unless they are removed. The time for poison buildup in an ordinary reactor is usually measured in months or years, while that for polyneutron reactions (such as the vapor in Oriani's experiment) is probably less than a microsecond. (His reaction volume is probably only a fraction of a cubic mm.)

(5) What can the poison be in the alpha shower experiment? Analysis of all possible reactions between ^{16}O, ^{17}O, ^{18}O, ^{1}H, ^{2}H and a polyneutron shows that none of these can generate a poison when the decay products are restricted to ordinary nuclei and polyneutrons. But further consideration of possible reactions shows that the composite nucleus $^{16}O\,{}^{A}n$ where a polyneutron ^{A}n is stuck to an oxygen nucleus ^{16}O (like a drop of oil stuck to a drop of water) is stable against strong decay. Transfer of nucleons between the two members of the composite is endothermic in both directions. One potential reaction is beta decay of the polyneutron $^{16}O\,{}^{A}n \rightarrow {}^{16}O\,{}^{A}H$ where a neutron in ^{A}n has become a proton in ^{A}H and the pairing symmetry has been preserved (the isotopic spin has not changed). But this reaction is endothermic because of the increase in coulomb energy. The first exothermic reaction is double beta decay and associated alpha emission, $^{16}O\,{}^{A}n \rightarrow {}^{16}O^{(A-4)}n + {}^{4}He$. Hence the composites last long enough to quench a chain reaction. They can absorb neutrons from polyneutrons in reactions such as $^{16}O\,{}^{A}n + {}^{B}n \rightarrow {}^{16}O^{(A+B-20)}n + {}^{20}n$, the shrunken polyneutron then being too small to fission. (The 20-neutron size is my best estimate of the optimum polyneutron spin-off, smaller or larger spin-offs being less exothermic.) In time the small polyneutrons undergo beta decay, become charged, and no longer can participate in nuclear reactions. So composites such as $^{16}O^{A}n$ and $^{4}He^{A}n$, and others depending on circumstances, can act as poisons.

(6) How then can a reaction be sustained for days in an electrolysis experiment, producing kilojoules of energy where Oriani's shower produced only about 10^{-8} erg? Here the answer lies in the extreme agitation in the bubbling regions near the electrodes

in electrolysis experiments. The rate of fluid shear is very high there and poisons are carried away and fresh reactant brought up so rapidly that a chain reaction can be sustained at a bounded rate determined by the rates of poison removal and fuel replenishment. So it turns out that simple fluid agitation is the key contribution of electrolysis.

(7) If fluid agitation is adequate to sustain a reaction, why bother with electrolysis? Why not use mechanically driven agitation? Several such experiments have been done, including one by Oriani and me, suggesting that mechanical agitation does sustain a reaction, but the quality of this work needs to be improved.

(8) A critical question remains about the origin of the initial polyneutron. Where does it come from? As mentioned above I prefer the idea of a radioactive precursor that emits a polyneutron. What could such a precursor be? The poisons $^{16}O\,^{A}n$ decay relatively slowly because the only available channel is $^{16}O\,^{A}n \rightarrow {}^{16}O^{(A-4)}n + {}^{4}He$ which requires double beta decay associated with splitting off ^{4}He and tunneling it through the coulomb barrier. The coulomb barrier gets higher as the composite shrinks. Smaller composites can have very long lifetimes. They also have a secondary decay channel in which a polyneutron is emitted: $^{16}O\,^{A}n \rightarrow {}^{16}O^{(A-24)}n + {}^{20}n + {}^{4}He$. For this reaction to be exothermic the droplet surface tension must be small, another constraint on my model of polyneutron properties.

(9) So where does the required composite come from? Because mechanical stirring will sustain a chain reaction, we can expect that such reactions have been ongoing for billions of years in turbulent streams and under waterfalls, transiently in any one location but with new locations being ignited as fast on average as others are extinguished by changes in stream flow. All natural reactors produce composites that find their way into air and water and keep igniting new natural reactors wherever conditions are favorable. Chemically they are oxygen of very large mass, 100 or more compared with 16 for common oxygen. Searches for heavy oxygen have not revealed anything in this mass range, so we know that the environmental concentration must be low. But concentrations low enough to have escaped detection are adequate to trigger chain reactions in laboratory experiments.

(10) The theory seems to have been contrived to fit the data by making new assumptions about reactions and assigning new properties to polyneutrons and composites as required. It may have been successful so far, but has it been useful in suggesting new experiments that might throw more light on the supposed cold fusion phenomena? Yes, to a limited extent. The theory suggested to Oriani that he expose detector chips in his electrolyte to see if he could detect energetic particles there. He did find particle tracks, and we presented papers on these observations. Then I suggested moving the chips to the vapor, where I expected to see alphas from decay of composites that managed to get there from the bubbling regions. Oriani did the experiments, found such decay alphas, and beyond my expectation (but not beyond my hopes) he found giant showers indicating chain reactions. I spent six months photographing and analyzing the tracks in one such shower that emitted about 150,000 alphas, decay products of perhaps a thousand composites that formed in the chain reaction and drifted along with convection currents as they decayed. (See “Energetic particle shower in the vapor from electrolysis” in markfisher.net/johnfisher.) The theory also suggests that a few polyneutrons can diffuse through the nickel cathode that forms the base of the detector cell and can lead to

generation of energetic particles on the back side. Such particles have been sought and found. Similarly a few polyneutrons may diffuse through the glass walls of the cell. Energetic particles have been detected there but analysis of the observed patterns of tracks is complicated by the presence of nitrogen, argon, and other atmospheric constituents, many of which react with polyneutrons and make the situation quite complex.

Most recently Oriani has been addressing the question of reproducibility, which has been a serious problem for everybody. My theory predicts that initiation of a chain reaction is a stochastic process, depending as it does on an unlikely radioactive decay or spallation event in the apparatus. Analysis of a year's worth of experiments has provided preliminary confirmation of the stochastic nature of ignition, suggesting a frequency of about one triggering reaction detected every three days in Oriani's electrolysis cell. He plans more experiments of longer durations (his usual durations have been 1-3 days) to put the evidence for stochastic ignition on firmer ground. (The 3-day mean time for ignition is probably appropriate only for Oriani's lab, where he has been generating composites for years. They have been adsorbed here and there on various lab surfaces from which they desorb and become available. In a startup lab where only the natural composite concentration is present the wait might be much longer.)

Other predictions have been made but not yet fully checked. The theory was applied to Iwamura's transmutation experiments with fair success. It predicted a few transmutation products that had not yet been observed. Then at ICCF12 in Marseilles last year it was reported that two of these predicted products had been found. Suggestions were made for three new transmutations that, if verified, would represent a triumph of the theory. (See "Polyneutron Theory of Transmutation" in markfisher.net/johnfisher.) These are clear-cut predictions that, if checked, will either please me a lot or will send me back to the drawing board or perhaps to the trash can.

(11) Theory accounts for helium as a product of polyneutron decay. But where does tritium come from? Polyneutrons can interact with deuterium in two ways. They can grow and support a chain in the reaction $^{2}H + {}^{A}n \rightarrow {}^{1}H + {}^{(A+1)}n$ and they can generate tritium in the reaction $^{2}H + {}^{A}n \rightarrow {}^{3}H + {}^{(A-1)}n$. We must check to be sure that the formula we employ for the mass excess $\Delta({}^{A}n)$ makes these and all other supposedly exothermic reactions actually so. In general the mass excess is a function of A that equals approximately $\Delta({}^{4}n) = 4\Delta(n) = 32$ MeV for the smallest polyneutron. It probably declines to a minimum near A = 20 as the strength of the BCS interaction increases, then rises again after the volume of the droplet has grown beyond the coherence volume. For a droplet containing hundreds of neutrons, expected to be the most prevalent size in chain reactions, I approximate $\Delta({}^{A}n)$ by the linear function $\Delta({}^{A}n) = \Delta_0 + \alpha A$ (+ β if A is odd). The parameters Δ_0, α, and β are assumed constant. As yet I see no way to determine Δ_0 from experiment, but α and β can be partially determined. Transmutation experiments suggest $\alpha = 1.143$ MeV. The polyneutron growth reaction must be exothermic for all A, requiring $\beta < 4.704$. The tritium production reaction must be exothermic for odd A, requiring $\beta > 0.671$. The relationships $0.671 < \beta < 4.704$ and $\alpha = 1.143$ summarize my current knowledge of $\Delta({}^{A}n)$. This relationship assures that for large polyneutrons all reactions claimed to be exothermic are exothermic.

(12) If reaction $^{2}H + {}^{A}n \rightarrow {}^{1}H + {}^{(A+1)}n$ is exothermic as claimed, then $^{2}H + {}^{A}n \rightarrow n + {}^{(A+1)}H$ must also be exothermic with exactly the same release of energy. Why are so few

neutrons observed? Since neutron release cannot be ruled out by energetics or quantum selection rules it must be kinetically inhibited by a potential barrier. The proton wave function corresponds initially to a localized proton at the surface of the polyneutron and finally to a distributed proton that is paired throughout $^{(A+1)}$H. It must tunnel through high-energy configurations where it is paired with only a few neutrons. In consequence the neutron reaction is highly improbable.

(13) What are the most troubling aspects of the theory as it now stands?

• Are the required masses of polyneutrons reasonable? Binding strength of about 7 MeV per neutron is required, about half of that for ordinary nuclear matter. So far there is no theoretical or experimental basis for deciding if this is reasonable. Nuclear physicists generally feel that it is not, but they are accustomed to short-range neutron interactions in ordinary shell-model nuclei.

• The theory requires that an odd neutron be bound with about half the strength of that for a neutron in a fully paired polyneutron. BCS seems to suggest the odd neutron would not be bound. It will have to turn out that there is an attractive force between a fully BCS droplet and an extra neutron that is strong enough to bind the extra neutron.

• Transmutations result almost entirely by transmutation of the ordinary component of a composite, after beta decay (or electron capture) in the polyneutron component makes possible exothermic transfer of nucleons from (or to) the ordinary nucleus. In these reactions there is no baryonic particle release; only electrons and neutrinos. Remarkably there is little if any detectable gamma radiation. This poses a problem. It may be that the electrons and neutrinos manage to carry away all the energy, leaving the ordinary nucleus in its ground state. Or (this is my current thought) the polyneutron may have a sufficient number of vibratory modes of excitation that it can absorb the residual energy before a gamma ray can be emitted. Then it can cool down at leisure by emitting soft X-rays or ultraviolet rays. (There is limited evidence for soft X-rays.)

• Is the idea that a correlation barrier can suppress neutron emission plausible? Neutron suppression is a vital requirement. It seems intuitively plausible to me, but a sound quantum mechanical treatment is required.

• The Iwamura experiments involve flow of deuterium gas through a Pd/CaO complex. There is no bubbling or mechanical stirring to carry away poisons or bring up fresh fuel. By what mechanism can a reaction be sustained in these experiments? This is a very important question to which as yet I have no answer. The situation is complicated by the physical configuration of the complex, by the flow of gas through it, and by the simultaneous presence of Pd, O, Ca, and deuterium.

• And of course there remains the interesting and practical question: What reactor design would work best and scale up? Here I have no good idea.

I am focused on refining and testing the theory. I would like to prove it wrong, or preferably to provide more evidence that it is on the right track. Time will tell.