

A HIGH-SCHOOL LEVEL EXPOSÉ OF THE MISTAKE UPON WHICH THE ERAB REPORT IS BASED

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I have already written a “Junior-College Level” or “Advanced High-School Level” exposition of the mathematical blunder upon which the Establishment academic opposition to the possibility of Cold Fusion’s reality is based. [Since many High Schools now have beginning calculus courses for college-aimed students, I have referred to that paper in the past as a “High-School Level” exposition; however, this one will be *far more elementary*.] That paper is 26 pages long, plus it is accompanied by 10 drawings, making a total of 36 pages.

For the benefit of those familiar with graduate-level Theoretical Physics, I have also condensed the main point down to *exactly one page* (involving understanding of the WKB solution of Schrödinger’s Equation). However, no one in the Establishment (or the DOE or the PTO) is paying any attention; they are standing pat on the ERAB Report.

There is a book out, by a reporter for *Science*, called *The Brain Bank of America*, which with the benefit of hindsight, proves painstakingly that ten (10) major reports by the National Academy of Science (NAS) to the US government on technical matters related to public policy were proved later by the march of events to be **dead-wrong** in each instance! The book’s author concludes that “no matter how eminent someone may be, he can be as pig-headed as me and thee!”

The real problem is that, as Caltech Prof. of Physics and Provost, David Goodstein chillingly concludes in his article on “Pariah Science” in the *American Scholar*, to quote his final sentence, is that “**Even if cold fusion is true, no one is listening**.” [Emphases added.]

I suspect that is why no one has paid any attention to my 36-page paper exposing the mathematical blunder underlying the ERAB Report’s dogmatic insistence upon a fatally flawed conclusion. The ERAB Report, and Huizenga, both in his book and private conversation, pretend that their main points are based upon experimental evidence. To the contrary, their main points have been demonstrated experimentally only in high-energy experiments *in vacuo* (and in almost equally high-temperature experiments in fusion plasmas), but they have NEVER been tested in or on the surface of a solid-state lattice!

Moreover, their dogmatic extrapolation of these results into *parameter-regimes* in which they have never before been studied is based upon **mathematical incompetence** which is so blatant that it can be understood by an advanced High-School Student (who has had elementary physics, elementary algebra, and beginning calculus).

Accordingly, I shall try once more.

The *very best* argument that I have seen in print to believe that Cold Fusion (CF) is inherently of such low probability as to be, for all practical purposes, impossible, is that contained in the 9-page section at the end of the first chapter of P.J.E. Peebles’ otherwise admirable book on *Quantum Mechanics* (QM), published by Princeton University Press.

Peebles makes two demonstrably blatant mistakes.

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PEEBLES' FIRST MISTAKE

His first mistake is to assume that the problem is *local* rather than *global*. Using an accepted simplification of a 3-dimensional (3D) analysis in a crystal which is adequate to predict the Nobel-Prize winning (but stunningly surprising) Mössbauer Effect, I replace the 3D lattice by a 1D lattice, namely a straight line. And when I add up the effects of all of the other bound deuterons, including making the line electrically neutral by placing an *averaged-location* electron between every pair of bound deuterons (except those on the right and left of the line-segment of interest), I get a much-modified Coulomb potential affecting the excited (or "free") deuteron of interest. This is called a Madelung potential. Peebles sent me a private letter saying that he thought that my summation of these potentials in a closed form expression was interesting and publication-worthy, but that "Madelung forces" had "nothing to do" with the issue at hand. With all due respect to Peebles (who should have received a Nobel Prize for proposing a search for the cosmic background radiation before it was accidentally discovered by others), he is simply mistaken on this point, and I have proceeded to prove it, as follows.

The one flaw in my original model (that I sent to Peebles) was that the *central* segment, between the bound deuteron on the left at $r = -L$ and that on the right at $r = L$, was not electrically neutral, because I had not included three (3) electrons to balance the charges of the bound deuterons on the right and left and the free deuteron somewhere on the segment $-L < r < L$ between them. I finally learned how to do this by reading a pro-CF paper, printed in the *Proceedings* of the NAS, by Parmenter and Lamb (where Willis Lamb is the Nobel Laureate who, along with the late Nobel Laureate, Julian Schwinger, was [according to Huizenga's book] responsible for persuading Harvard University Nobel Laureate, Norman Ramsey [the only Nobel Laureate on the ERAB panel] to state publicly that he would *not* sign the final ERAB report unless there were included in its preface a sentence which essentially *retracted* and *nullified* the whole report's major conclusion). When the DOE and the Patent and Trademark Office (PTO) stand pat on Huizenga's summary of the ERAB Report, they are actually guilty of *ignoring* the most important sentence in the entire report!

What I learned from Parmenter & Lamb was the technique of augmenting my Coulomb-Madelung potential $V_{CM}(r)$ by a Fermi-Thomas/Mott potential, added only in the central segment $-L < r < L$, which was an attractive potential *quadratic* in r and which was multiplied by an exactly correct coefficient to make it correspond to a *cloud* of three (3) electrons smeared out evenly over the whole line segment $[-L, L]$. I then derived proof that my final Coulomb-Madelung/Fermi-Thomas/Mott potential $V_{CM/FT/M}(r)$ was exactly correct, by deriving a theoretical formula for the Schwinger-Ratio based upon that potential.

VALIDATION BY PREDICTION OF THE EMPIRICAL SCHWINGER RATIO

Schwinger had conjectured that, for CF, the size of the Schwinger Ratio is all-important, and in my advanced papers I have derived a rigorous proof that his conjecture is true, namely that the *Spectrum* of Resonant Transparency Energy Levels of the alleged "Coulomb Barrier" is a function of *nothing* but the Schwinger Ratio! This ratio is the reciprocal of the ratio of the rms (root-mean-square) *amplitude*, say Λ , of the vibrations of a bound deuteron at *absolute zero* temperature (where it still fluctuates, according to the QM theory of Zero Point Fluctuations [ZPF]), to half of the potential well-width, L . This ratio $(\Lambda/L)^{-1} = (L/\Lambda)$ is a purely empirical number, because L is determined by crystallographers (e.g using x-rays), while the number Λ can be determined independently by either the size of a certain blur (vibration-width) on an x-ray photograph or from neutron-flux studies. Therefore (L/Λ) is a strictly *experimentally-measured* result.

In the case of a deuterium lattice inside of a palladium lattice, the Schwinger Ratio is known experimentally to be 28.275 . Now by using my closed-form expression $V_{CM/FT/M}(r)$ for the potential, I derived a *first-principles* formula for the Schwinger Ratio which depends *only* upon the basic constants of physics and pure mathematics; but upon inserting these numbers and evaluating my formula, I came up with the experimentally measured value to within one-third of one percent!

I believe that this validates my $V_{CM/FT/M}(r)$ potential as well as could possibly be expected, and so I am justified in using this potential (and not the local potential $1/r$ used by Peebles).

PEEBLES' SECOND MISTAKE

The second mistake made by Peebles is a corollary of his first mistake. If he had treated the problem as *global* problem (rather than purely *local* problem) he would have run up against the fundamental theorem of QM in Solid-State physics, namely Bloch's Theorem. This theorem states that no solution of Schrödinger's Equation inside a periodic lattice is valid unless the solution's logarithmic derivative is spatially periodic of *exactly* the same period as the lattice. In the case at hand, since my potential is *periodic* of period $2.L$ on the straight line $-\infty < r < +\infty$, then no solution of the fundamental equation of QM is relevant unless it is also of period $2.L$ in the displacement r .

ELEMENTARY ANALOG TO EXPLAIN ERAB MISTAKES

I have thought of an analog to explain Peeble's (& so ERAB's) mistakes which is so clear and readily understandable that an advanced High-School Student can follow it.

The famous Schrödinger Equation is a generalization of Laplace's Equation, which is much easier to solve. Let $\phi = \phi(x,y)$ be a solution in the (x,y) -plane of

$$\nabla^2 \phi = 0$$

where, as usual, the operator

$$\nabla^2 = \partial^2/\partial x^2 + \partial^2/\partial y^2$$

denotes the sum of the second partial derivatives with respect to x and y . Suppose that we are interested in the solution mainly near the origin $(0,0)$, where $x = y = 0$. Suppose also that we want the solution and its first partial derivatives to vanish at the origin. Then by an elementary exercise in differentiation, the reader may verify that

$$\phi = (2.\pi/L)^2 .x.y$$

is such a solution. Let us call this the ERAB solution.

Now I blow the whistle and say, "Wait a minute; the solution which you want is:

$$\phi = \sin[(2.\pi/L).x].\sinh[(2.\pi/L).y]$$

which you may readily verify *ALSO* satisfies the required boundary conditions." The ERAB committee comes back and says "What's the difference? If you expand the trigonometric function $\sin(x)$ and the hyperbolic function $\sinh(y)$ in power series, then the two solutions agree up to joint **FOURTH ORDER** in the two small variables, so that for all practical purposes, when you are near the origin, the arithmetic difference between your solution and our solution is truly negligible."

This is the fallacy of believing that the *local* solution is the only solution, just because it gives excellent agreement with experiment when the variables are near the origin.

But now along comes a Solid-State physicist who says: "The (x,y) plane is not the right *domain* upon which to be solving the problem. You should cut a vertical line at $x = L$ and another vertical line at $x = -L$ and then discard everything except in the vertical strip containing the segment $-L < x < L$. Then roll up the strip into a *cylinder*, wherein points on the vertical line $x = L$ are conceptually *identified* with points on the line $x = -L$ which have the same y -coordinate.

Then the first, merely *local* solution fails miserably; but the second, *global* solution, which satisfies the *periodicity* required to be a point on the cylinder, is the *only* correct solution!

MESSAGE OF THE ANALOG

As long as they were dealing in a situation of collisions between particles in a vacuum (or in the near-vacuum of a tenuous fusion-plasma), the ERAB panel's strictly local solution of the Schrödinger Equation was adequate to give tremendously accurate results, which led them mistakenly into the complacent but false belief that they knew everything which is to be known.

However, when they started to deal with nuclear reactions inside of a *periodic lattice*, they overlooked not one but THREE (3) major differences:

1. With so many lattice particles in a fixed position (as for ions) or in an effectively averagable pseudo-fixed position (as the net result of the circulating electrons [which need be taken to be a probability-cloud only locally and not globally]), then the fact that electrostatic forces are *strictly additive* (and Coulomb forces are *long-range* forces) means that it is *physically incorrect* to use a *local* Coulomb potential (as in the usual Gamow-factor derivation of reaction-rate), one *MUST* use a *global* potential (like my closed-form Coulomb/Madelung/Fermi-Thomas/Mott potential) which misleadingly seems to be close to the Coulomb potential locally, but globally is very, very different.

Otherwise stated: For two isolated particles in a vacuum, the Coulomb potential is correct; for two colliding particles in a tenuous plasma, the other particles are not only distant but in randomly changing positions, so that their averaged net effect would be zero, and again the Coulomb potential is acceptable. But inside of a *rigid* lattice, the effect of all of the other particles is unchanging and does not average out to nullity, and so *MUST* be included for physical correctness. Therefore the Madelung forces *are* relevant!

2. Even using a correct global potential is not enough! One has to take into account the *periodicity* of the domain of definition and find a *periodic* solution of the wave equation in order to take into account the well-known optical phenomenon of *resonant transmission*, as clearly explained in a chapter of Bohm's classic book on QM.

3. Actually, even that is not enough! Sophisticated critics of the preceding two points, such as Rabinowitz and Worledge, and, independently, Jändel, have looked up Bohm's derivation and said, "Well, OK, we'll agree that there is a finite probability of a free deuteron 'leaking' (or QM-tunneling) through the Coulomb Barrier of a bound deuteron, leading to a fusion reaction, but if you calculate the uncertainty in the time, it will take several billion years to have a 50% probability of occurring!" However, I have rebutted that with an answer so solid that they have not replied: "Just include in the calculation the ZPF of the bound deuterons, and the concomitant uncertainty of the position of the bound deuterons, and then the uncertainty in the time of tunneling *reduces* from *eons* to *picoseconds*!"

CONCLUSION

The sad *fact* is, that the ERAB Report's conclusions are *fundamentally flawed*, at the most *basic* theoretical level, by errors so stark and so flagrant that they can be explained to an advanced High-School Student!

But the DOE (which, together with its predecessor AEC, as noted in a recent front-page major signed Editorial by the Editor of the *Las Vegas Sun*, Brian Greenspun, has a "*45-year record of lying to the public*") just repeats the *irrational* mantra "the consensus of the scientific community has been obtained by our usual procedure; so as Civil Servants we must respect the scientific community's consensus and not substitute our own private judgments until the scientific community as a whole agrees that a mistake has been made; and so, accordingly, the ERAB report *stands* as the position of the DOE and the question of

its validity **will not** be re-opened no matter how strong the arguments any one individual like you may make!"

APPENDIX: a letter

Subject: My allegations of two "Mistakes" in your QM book in the section purporting to prove Cold Fusion (CF) so unlikely as to be essentially "physically impossible."

To: Dr. Philip J. E. Peebles, Princeton U

You say you cannot follow my reasoning in my shortnote alleging to point out mistakes in your 9 pages on CF in your QM book, so I shall try to clarify my critique. There are two utterly unrelated senses in which you are mistaken to consider only the electrostatic potential $V(r) = K/|r|$ near a bound deuteron at $r = 0$, and therefore it may be that I am confusing you by discussion of "local" versus "global." The basic mistake you have made is in assuming that if an approaching deuteron is reflected by the electrostatic potential near $r = 0$, that is the end of the story. But suppose that the reflected deuteron bounces back from the Coulomb barrier of the next adjacent bound deuteron, with **exactly** the right energy level for Resonant Transparency of the Barrier? Read the chapter on this in Bohm's book on QM! If you want to think physically about a wave-function building up between two Coulomb barriers, one on the left and one on the right, you will see that there is a clear possibility that the amplitude may eventually become such that the probability of "resonant transmission" is much greater than 50%.

You did not ask to see my 36-page version (with 10 pages of printed drawings, several to scale, others schematic), much less my 100-page version [wherein I have rigorously generalized the Bohm criterion for resonant transmission of a particle through two potential barriers between which it might normally be seen to be trapped, to resonant transmission through **quadruple** potential barriers (because near $r = 0$ the Coulomb potential gets modified by the strong nuclear force and on the left and the right of $r = 0$ there are barriers that do **not** go to infinity [as would $1/|r|$] but are about 23 MeV in height [if my memory is correct]).

Anyway, several experts who have studied carefully the Resonant Transmission argument [such as Rabinowitz & Worledge, and, independently Jaendel] have **admitted** that there is a non-zero probability that the deuteron will QM-"leak" through the Coulomb barrier, but they claim that this is merely of academic interest, because if you use the computation in Bohm's book for the time that the leaking takes, then you get several billion years. However, I have rebutted this by including the ZPE uncertainty of the position of the bound deuterons, and then repeating the Bohm calculation, and get the leaking-time to be pico- seconds!!!

So the **FIRST** mistake you made is to assume that the problem is "local" rather than global, in that Resonant Transmission is a global process, but by mistakenly **assuming** that a local analysis [reflection by Coulomb barrier near $r = 0$] is adequate, you **ignored** the much more important question of whether or not Resonant Transmission through the Coulomb barrier is possible, which it most certainly is!

Another approach to this matter has been published by a Prof. of Nuclear Physics at Purdue, Yeong Kim, whose "optical theorem" shows that even if one ignores the lattice, but just starts the potential on the surface of a nucleon and goes outward locally, then [by analytic continuation] one can **prove** that among all possible solutions there is at least one which finds the Coulomb barrier completely transparent! (I keep challenging Dr. Kim to include the periodicity in the rest of the lattice, and predict that when he does that, his "optical theorem" solution will be found to be a more rigorous version of my approximate result based upon the WKB approximation.)

Indeed, although the answer won't be accurate, you can use the formula in Bohm's book for Resonant Transmission with the following **over-simplified** model of the potential. Consider only the line-segment between $r = -L$ and $r = L$, i.e. the interval $-L < r < L$.

Suppose the excited deuteron is somewhere on the above- stipulated line-segment of width $2.L$ Now if there is a bound deuteron at $r = -L$ and another lattice-bound deuteron at $r = L$, the excited deuteron sees the Coulomb potential of the two adjacent deuterons as

$$V(r) = K/|r + L| + K/|r - L| = 2.K.\{ L/[L^2 - r^2] \}$$

and so you have a "potential well" on the interval $-L < r < L$, and you can use the formula in Bohm's book to compute the energy levels for Resonant Transmission.

Ignoring this possibility (which you should have remembered, since you discuss Breit-Wigner later on as I recall) was **Part A** of your First Mistake. **Part B** was in not summing up the potentials of all the other particles in the lattice in order to get a more accurate formula for the Potential Well in the region $-L < r < L$ of interest. In this I assumed that there are bound deuterons at $r = -n.L$ and at $r = n.L$ for every positive integer n . Then to make the line almost electrically neutral I place an electron at the mid-point between each bound deuteron [except in the interval of interest] because **on average** that is where the circulating electrons will be. This is my Coulomb-Madelung Potential; but it is still not accurate enough, because it omits the 3 final electrons needed for strict neutrality (one each near the bound deuterons at $r = -L$ and at $r = L$, and one somewhere between) and I did not know how to include them until I read the pro-CF papers by Parmenter & [Nobel Laureate] Willis Lamb which augmented the above potential well by a potential quadratic in r on the central interval with a coefficient to make it what would be produced by 3 electron-charges distributed **evenly** as a smeared-out charge-cloud on the entire central interval. (This is the improvement of my potential from a Coulomb-Madelung potential to a Coulomb-Madelung/Fermi-Thomas/Mott potential.)

Next I **proved** the accuracy of this potential by **predicting** the strictly empirical Schwinger Ratio within one third of one percent of measured reality! How much better could I check the validity of the theoretical potential?

Note that the potential is so far defined only on the central interval. It is necessary to include ALL of the distant electrical charges in the lattice, because (unlike in a plasma) the positions [of all but the central 3 electrons & the central excited deuteron] are **on average** sufficiently fixed that they do NOT cancel each other out, and therefore it is **physically incorrect** to ignore the Madelung forces (despite your contrary assertion). The further proof is that by including the effects of all of the long- range Coulomb forces in the entire lattice, I got a central well which is 40% deeper than in the simplified potential above, where it bottoms at $V = 2.K/L$, and it is precisely in this NEW and deeper part (which you won't get if you ignore the Madelung forces, and that I would not have found if I had accepted your dictum that "the Madelung forces are irrelevant") that I find the **lowest** energies of Resonant Transparency of Coulomb Barrier! (In fact, I have computed quite rigorously [using a slight improvement of the method in Koonin's book] the lowest 600 such energy levels, and find that this line-spectrum goes from about 6 eV to about 150 eV, which is so low that conventional nuclear physicists cannot comprehend it and declare Low Energy Nuclear Transmutations (LENT) to be an absurdity.

Your **second** Mistake was not to have made the potential on the central interval $-L < r < L$ into a periodic potential on the entire line from $-\infty < r < \infty$, in order to invoke Bloch's theorem and get a solution of the wave equation which is relevant inside a periodic lattice. (This same argument leads to the correct answer in the case of the Moessbauer Effect, as you can read in David Park's book on Classical & QM). This is just another approach to deriving the Bohm formula for Resonant Transparency. The bottom line of my **correction** of the CF analysis in your QM book is the following (which has an obvious wave-mechanical interpretation, as to whether or not the de Broglie wave-length of an excited deuteron has an odd or even number of waves to just fit within the potential barriers, as drawn clearly in a picture in Bohm's book), and can be derived **rigorously** from the WKB approximate solution of the wave equation in the case of all energy levels except the lowest few (at which WKB is not accurate enough). Here is my Patent-Pending CF criterion based on this:

The Schwinger Ratio depends on the ratio of the lattice period length L to the rms amplitude Λ of the Zero Point Fluctuations of the bound deuterons. The host lattice determines L ; however the particle mass (e.g. deuterons or protons) of the **embedded** lattice determines Λ . Empirically, for beta-phase deuterons in a palladium lattice, the ratio L/Λ is about 29.

Now the final criterion for Resonant Transparency of the Coulomb Barrier says that a particular host-lattice/particle pair will be suitable for CF **if and only if** the Schwinger Ratio divided by pi is closer to an **odd** than an **even** integer. (The "Bass Criterion for Quantum Resonance Triggering [QRT] of a phonon-mediated chain- fusion reaction.")

Consider host lattices of palladium and nickel, and consider embedded lattices of protons or deuterons. Then there are **four** (4) distinct possibilities. However, putting in the numbers, I find that the Bass QRT Criterion predicts that deuterons will work in palladium, but that protons will NOT! This is why Fleischmann & Pons were OK in using ordinary water as a control on their heavy-water experiments. This is a basic point which CERN's arch-critic of CF, Douglas R. O. Morrison, seems not to understand, because he is always wrongfully accusing pro-CF people of inconsistency in using ordinary water with nickel cathodes and yet taking it to be a control when working with palladium cathodes!

In other words, when I divide the mass of a proton in my formula for the Schwinger Ratio by two, to change from heavy water to ordinary water, the non-linearity in the criterion causes the criterion to jump from nearly an odd number to nearly an even number!

Thus "my" theory passes the "Rabinowitz Acid Test" which Rabinowitz had opined NO theory could pass! Now replace the host lattice by nickel, and you get the exact opposite results! This explains why ordinary water works in Patterson Power Cells, as well as the excess energy in Piantelli's hydrogen-nickel cells.

In his 1990 ICCF1 paper, and in some others of which 2 or 3 out of 4, but not all, were published in Germany, Schwinger had conjectured that what I call the "Schwinger Ratio" in his honor was all- important in the CF business. I have verified his conjecture by proving the THEOREM that the nth Energy Level in the Spectrum of Resonant Transparency Levels ($n = 1, 2, 3, \dots, 600$) is a function of NOTHING but the integer n and the Schwinger Ratio!

Around 1992 I sent a Nobel Laureate at the Cavendish Lab in Cambridge, Brian Josephson, a set of Schwinger's papers, and he sent me a FAX saying that in his opinion Schwinger was "cheating" by extrapolating the validity of a certain series beyond its radius of convergence. I have now made everything in the Schwinger paper rigorous and in the process have discovered the fatal errors in your QM book's treatment of CF alluded to above.

I am very puzzled by your statement that you are "content" with the analysis in your book and see no need to pay any attention to my alleged correction of its errors, since you "cannot follow" my arguments. I will send a copy of this to Brian Josephson and see if he thinks that in the QM analysis of CF I am as much in need of basic "tutoring" as you seem to think.

Please let me know if the preceding attempt to further explicate my short note makes my critique of your alleged "mistakes" more comprehensible.

Sincerely,

Bob Bass