Subject: Lost History
Date: Tue, 17 Jan 2017 20:15:42 -0700
From: Lowell Morgan
To: S.B. Krivit

hi,

[P1]
i'm now reading "Lost History". my original intent was to re-analyze some of the experiments, such as those described in Ch 13. this is my 50th year as a professional physicist having done spectroscopy in the early years and gas discharge & plasma physics for the past 45 years (i've been self-employed for 30 years). you don't provide enough information in the book, however, to really analyze what these people were doing and seeing. several examples:

[P2]
(1) they used electric discharges in glass tubes, i.e. cathode ray tubes. unless the glass was very pure quartz it would have sodium in it. if, via electron or ion impact, heating, ..., whatever, sodium got into the gas they would see the Na+ emission spectrum that, except for a small wavelength shift, is identical to the Ne spectrum. a prism spectrometer such as that on p. 52 nor most grating spectrometers wouldn't be able to resolve the difference. ditto for K+ & Ar. also, unless they washed their glassware with DI water, which they didn't have then, the water, even distilled water, would have sodium and other alkali metals in it and would leave a monolayer residue. alkali metals are close to impossible to get rid of once they've contaminated an experiment (ditto for He as well). i've observed this myself. we've even made electric discharges in distilled water although we couldn't strike discharges in DI water, even with voltages in excess of 20 kV.

[P3]
(2) you use the word "vacuum" in describing the various experiments. at just what pressures did they work? later in the book you mention a pressure of 3 cm of Hg, i.e. 30 Torr, in connection with an experiment of Thomson's in H2. that's low-medium pressure and 4x the atmospheric pressure Mars. low pressure would be sub-Torr but i doubt that their pumps could draw the pressure down to a Torr. the gas pressure, since you never really have vacuum, is critically important to a gas discharge plasma and its plasma chemistry as is the composition of the background gas. experiments that you mentioned earlier in Chapter 13 used H2. something to note with regard to gas discharges is that any trace atomic or molecular constituent in the background, e.g. Na or K, is likely to be ionized rather than neutral. that comes from the Saha-Boltzmann relation that, although only strictly true in the case of LTE (which never exists in plasmas), still can be used a a guide.

[P4]
(3) regarding the discussion of JJ Thomson's work on pages 126 & 127 you mention T. regarding M=3 you say "There are two possibilities. It was either the extremely rare, stable helium-3 isotope, or it was tritium, the unstable hydrogen-3 isotope". that's a myopic or, at least, an uninformed statement. with likelihood approaching unity it was the triatomic molecular ion H3+. in fact, JJT is credited with discovering it in, if i recall, 1911. it's the most common positive ion in the Universe (& perhaps even in the Multi-verse) and, given that JJT's mass spectrometer was the equivalent of a contemporary Residual Gas Analyzer (RGA), as quadrupole mass spectrometers had not yet been invented, it probably even produced some of its own H3+. i've attached a mass spectrum taken using an RGA in some hydrogen discharge experiments that i was involved in a couple of years ago. notice how close together the masses for He-3, HD, H3+, & T are for species that all nominally are M=3. it would take a high end quadrupole mass spectrometer to sort these out. in a plasma, any small abundances of D & T would end up as DH or TH or their ions or as a component of a tri-atomic molecular ion. the other graph that i attached shows a narrow portion of the spectra of all the diatomic H, D, & T possibilities. this was taken using a *21-foot spectrograph having a 30,000 lines/inch grating*! these didn't exist in the early 1900s and few exist even today. so, for practical purposes, there no spectroscopic means of sorting out all these isotopic species.

[P5]
(4) some people for whom i consult recently put He gas under very high pressure inside an iron alloy anode shell and let it diffuse out. i calculated the diffusion coefficient, which was very large. granted, He in a defect free metal shell might have a very small diffusivity, but defect free machined metal electronic devices, such as anodes, don't exist - kind of like LTE. in addition, once one contaminates with He the environment associated with an electric discharge plasma, one can't get rid of the stuff. it hides everywhere. so, the graph on page 115 has caveats associated with it.

[P6]
these are just a few points. had i the published articles, which i don't have access to, i'd take some time and analyze, in as much detail as my experience & intellectual tools permit me, the early 20th century experiments.

[P7]
these have been merely examples of difficult experimental diagnostics & analyses in this kind of research are and the pitfalls that one might encounter (there are many more) in this research. as exciting as the prospects of LENR may be, people, most of whom i gather are amateurs, should find something different to get excited about. as with other areas that i've been involved in, the LENR people are, on the whole, too credulous.

[P8]
there are other such things. i spent this past weekend working through the Widom & Larsen 2006 article. it's not even weak-interaction physics although it begins with a reaction that has never been observed in any particle accelerator experiment colliding electrons & protons. it's solid state physics and is based upon a fallacy (as is their patent) that heavy electrons, i.e having mass m* > m, actually exist. they don't. the concept, which began with the Drude model in 1900, is to have a simple means of getting the electron transport correct in k-space without having to deal with details of the band structure of the solid. there's a Reviews of Modern Physics article two or three decades old that lays out how one does this. you can do a similar thing with electron transport in dense plasmas (i did it in liquid CH4 in the early 1980s) or motion of colloidal particles in liquids by scaling out the elastic collision frequency or viscosity respectively using an increased mass. it's a *simplifying model* (in most solids, if you want to do it right, m* is actually a tensor) that makes calculations & understanding easier.

[P9]
another very recent item along that line is the November publication of an experiment purportedly demonstrating an Electro-Magnetic Drive using the quantum vacuum. over the holidays another physicist & i looked into this for a person who was interested in investing. the WWW was, of course, full of news of this development written by credulous observers. the work was attributed, wrongly, to NASA. it was just a couple of guys working for a NASA contractor in Houston. it was a badly designed & executed experiment with terrible analyses. maybe something like that would work - as much as anyone else i wish it would & ditto for LENR - but i'm not going to hold my breath.

[P10]
there was a similar popular free energy uprising a decade ago due to Rustum Roy & John Kanzius that became a meme all over the Internet. it had to do with "burning salt water" & net energy release. it took a colleague & me 5 years to get to the bottom of it and explain what it really was, which was actually very cool in its own way.

[P11]
you has asked how it is that i know so many people mentioned in your first book. well, 35 years ago i worked with Peter Hagelstein on H-bomb driven x-ray lasers at LLNL. i'm acquainted with Dick Garwin from JASON meetings, but that was in the 1980s as well. i doubt that he remembers me. i got acquainted with Freeman Dyson through the JASONs as well. i knew the late-George Miley for 30 years dating from our work on nuclear reactor driven gas lasers in the 80s & 90s. there were a number of others as well but i'd have to go back and thumb through the book to recall who they are.

anyway, i'll try to figure out as much as i can from your book sans the original early 20th century articles. i'm enjoying reading your books.

best wishes,

Lowell Morgan

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Subject: Re: Lost History
Date: Thu, 19 Jan 2017 13:41:15 -0800
From: S.B. Krivit
To: Lowell Morgan

Dear Dr. Morgan,

Thank you for reading my books. And congratulations on your 50th year as a professional physicist! I do appreciate your contacting me with your questions and comments. I will respond to the relevant points in your letter by paragraph number.

1. You wrote: "You don't provide enough information in the book, however, to really analyze what these people were doing and seeing."

Yes, that is correct. You have to read the original published scientific papers for that level of detail. You'll find that the text is supported by 260 references, as shown in the bibliography.

2. You wrote "unless the glass was very pure quartz, it would have sodium in it."

I do not make an assertion about Ramsay and his sodium claim. I direct you to page 78, where I wrote,"After an extensive review of the original scientific papers and half a dozen historical accounts of this claim, I have been unable to determine whether Ramsay succeeded in his transmutation attempts." Although I

appreciate your speculation about Ramsay's claim, you and I have no argument on this point.

3. You ask about specific vacuum pressures. That level of scientific detail is beyond the scope of this book. I would encourage you to read the original papers.

4. You wrote that my discussion of J.J. Thomson's observation of a mass-3 element omitted the, in your opinion, obvious certainty that the element was in fact triatomic hydrogen. If you read the original papers, you'll find that this concern was obvious to Thomson, that he did tests to rule this out to the extent possible, and that he found no evidence that the mass-3 was triatomic hydrogen.

5. I appreciate that you've done some of your own tests on the diffusion coefficient for helium, and I appreciate your opinions. The graph you referred to on page 115 is from a published scientific paper, so I have to consider that with greater weight. Additionally, I refer you to Appendix B — Helium Permeation in Metals Analysis. There, you'll find seven additional scientific reports on this matter.

6. I do agree with you that, in discussing specific published scientific papers, it is advantageous to read such papers. I encourage you to do so and to avail yourself of the reference information in my bibliography for *Lost History*.

7. I accept your comment "as exciting as the prospects of LENR may be, people, most of whom I gather are amateurs, should find something different to get excited about" as your opinion. Although I strongly disagree with your opinion, I appreciate you sharing your perspective with me.

8. You wrote that the reaction mechanism described by the Widom and Larsen theory "has never been observed in any particle accelerator experiment." I would certainly expect that to be true. The reaction process I described in my book *Hacking the Atom* — which you read — occurs in a different environment, condensed matter. Additionally, the LENR reactions are many-body, not two- or few-body, as occur in high-energy accelerator experiments.

Your comments and questions are intriguing, however, my personal time constraints may limit my ability to continue further conversations.

Again, I'd like to thank you for reading my books and for reaching out to me.

Warm regards,
Steven

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Subject: Re: Lost History
Date: Thu, 19 Jan 2017 15:11:00 -0700
From: Lowell Morgan
To: S.B. Krivit

Thanks for taking the time to comment. I don't have access to the original papers and, as I have no particular interest in this field, it's not worth it to me to spend the money to purchase the articles. They were of particular interest to me because they come under the heading of gaseous electronics. I'll finish reading book 3 but I can do no more than just accept what you have to say about what you've read.

I too am working more than full time so I need not make a hobby of analyzing century old work. They were, however, world class experimentalists and were a couple of decades before the Era of quantum mechanics and nearly a century before contemporary experimental diagnostics.

Thanks & best wishes,

WLM