K.P. Sinha and Andrew Meulenberg response to New Energy Times Request for Critique of Widom Larsen Theory Paper #4, Revision 2

Widom and Larsen have chosen to ignore our prior warning about their use of "heavy" electrons. They have been confusing effective mass with relativistic mass (or resistance to accelerating forces rather than conversion of mass into kinetic energy). It is comparing apples and battleships. Perhaps a public airing, with a few more details, will clarify the situation.

We will use Krivit's questions #7 and #20 as the platform.

Question 7. On page 1, paragraph 3, Widom and Larsen write, "The purpose of this work is to estimate the total rates ... " Do you have any comment on the power and/or energy rate estimates that they come up with?

20. Do you have any comment on their mention of using a "LASER light beam" on page 1, paragraph 1 and the reference to laser radiation applied to a cathode surface on page 11, paragraph 4? *They cite Iwamura, Itoh, Gotoh, Toyoda, Violante, Castagna, Sibilia, Paolini, Sarto, Dash and Miley. (Letts and Cravens are not cited.)*

Sinha and Andrew Meulenberg answer question 7: Paragraph 2 (or 3) on p. 2 of their latest paper reads:

"An order of magnitude estimate can already be derived from a four fermion weak interaction model presuming a previously discussed [12] electron mass renormalization $m \rightarrow \tilde{m} = \beta m$ *due to strong local radiation fields. Surface electromagnetic modes excited by large cathode currents can add energy to a bare electron state* $e-$ *yielding a mass renormalized heavy electron state* $\tilde{e}-$*, with*

$$\tilde{m} = \beta m. \qquad (7)$$

*The threshold value for the renormalized electron mass which allows for the reaction **…** is*

$$\beta > \beta_0 \approx 2.531. \qquad (8)$$ ***.....***

If there are $n^2 \sim 10^{16}/cm^2$ *such* $(\tilde{e}-p+)$ *pairs per unit surface area within several atomic layers below the cathode surface,**……"***

The field-enhanced electron mass that Widom and Larsen are proposing is more than doubling the normal mass. While we have used the same claim for an **effective**-electron-mass-enhanced means of reducing the D - D (or H - H) interaction distance, we would never assume (as did Widom and Larsen) that this extra mass is relativistic. If this mass were relativistic, the electron energies would approach 1 MeV (the range of energies needed to make up the mass difference between the proton and neutron).

What happens to their argument when this assumption is used? They go on to assume that all of the surface conduction electrons have this energy and are all "coupled" with protons in a PdH lattice ("If there are $n^2 \sim 10^{16}/cm^2$ such $(\tilde{e}-p+)$ pairs per unit surface area ")

There are several obvious fallacies in this scenario.

a. **Laser-generation of high energy electrons**. Widom and Larsen state that intense laser illumination is required. They didn't mention that femtosecond, terawatt pulses, which turn the surface into a plasma, are needed to inefficiently create these energetic electrons. (see for example – picked by title from hundreds of appropriate papers on the internet - A. G. Zhidkov, et. al. "Pulse Duration Effect on the Distribution of Energetic Particles Produced by Intense Femtosecond Laser Pulses Irradiating Solids" Physics Of Plasmas Volume 8, No. 8 Aug. 2001 3718)
b. **Confinement of electrons**. A laser beam cannot confine such energetic electrons. 1 MeV electrons in Pd move 10's of microns as they slow down (in picoseconds). This is no longer a surface effect and the densities mentioned in the paper couldn't hold even if they were created.
c. **Temperature**. MeV electrons represent a temperature in excess of 10^{10} K. Does having a modest percentage of all the electrons on the PdH surface at this temperature compute?

As a follow-on from Krivit's questions:

Q17. If this paper is published, would you care to speculate on its impact?
Q18. Specifically, might this be of major or minor help to the field, if any?
Q19. Specifically, might this be of major or minor harm to the field, if any?

We see a disaster, if this paper were to be published and acclaimed by CMNS. It would certainly confirm most physicists' view of the field. Mostly those looking for flaws would read it. They would easily find and advertise them.

Good equations, grand phrases, and authoritative references (some of which seem misapplied and/or contradict the intent of the paper) do not make an acceptable paper. Glaring errors can only be covered so far by QED. We went down a lot of blind alleys trying to find something that we had overlooked to rectify the "wrong" that we felt. Our instincts said there was a serious problem. Our weakness in QED suggested that we weren't "up to it." We finally saw the "emperor's new clothes." There was no solution in the QED.

It looks like a snow job. And that makes us wonder why/how it was done.

One of their referenced papers (below) was published in a reputable journal. We haven't reviewed it - to see if it also should have been rejected or if it was phrased in such a manner as to avoid the errors. But we would be suspicious of any of their work now.

[12] A.Widom and L. Larsen Eur. Phys. J. C 46, 107 (2006). ***……***
"The sources of the electron mass renormalization via electromagnetic field fluctuations on metallic hydride surfaces and the resulting neutron production are **the main subject matters** of this work. The surface states of metallic hydrides are of central importance: (i) Collective surface plasma [11] modes are involved in the condensed matter weak interaction

density of final states. The radiation frequencies of such modes range from the infrared to the soft X-ray spectra. **…”**