Response to Prof. X's ** comments on
"A test of Aetherometry vs Relativity, Special and Larmor-Lorentz:
the 1938 Ives-Stilwell experiment"

** Prof. X is a well known reviewer for a mathematical physics journal

NB - Since your commentary does not follow defined sections, or have a logical order, we present our response below as much as possible following the order of your comments, but grouped in separate sections. Extracts/quotes from your commentary are shown in red.

1. Preliminary comments

Your commentary begins with a generalization that is *not* correct:

"To begin with, some observations regarding the Ives and Stilwell paper, are in order. The paper main merit is to have been a pioneering one in the field, but this is, in my opinion, the ONLY reason for it to be always quoted by everybody dealing with the subject. Granted the ingenuity in devising the experiment the way they did, the final results are, to say the least, not quite convincing for several reasons."

If a research paper opens up a new field of investigation, it is – or should be – always quoted. The I&S paper, however, most often *fails* to be quoted, even though it initiated a new experimental field that claimed to corroborate the relativity of all motion. Why? This is the question that you gloss over, because the answer simply is – that I&S did not see their results as confirmation of Einstein's SR, but rather of Larmor-Lorentz relativity (LLR). That, and that alone is the reason why the paper so often failed to be referenced at all, since the mainstream reviewers never forgave Ives for his advocacy of LLR…

Your commentary then proceeds with another erroneous generalization:

"The experiment, at least the way I understand it, consists in creating hydrogen molecular ions, accelerate them in the intercatode space to velocities determined by the potential difference and then, supposedly, expecting that, by an undescribed process only H_2+ and H_3+ molecular ions are actually formed. These ions, then, dissociate and neutralize into hydrogen atoms travelling with the same speed of the progenitor molecular ion."

This commentary is both confused and confusing. For, after all, "creating hydrogen molecular ions" in the *interelectrode* space (not intercathode) is already the same, precisely, as "forming H_2^+ and H_3^+ molecular ions"!! The problem, most generally, is that I&S actually failed to propose any reaction for the processes they studied. They proposed no mechanism as to how these two kinds of molecular ions were formed in the *interelectrode* space, but presented spectrographic evidence that the canal rays in their set-up were composed solely of diatomic and triatomic hydrogen ions, and no trace of *protons or atomic hydrogen* was found: "no H_1 particles were found in this work". Yet, I&S made the "assumption that in every case the emission is that of a single excited hydrogen atom to which all the particles [meaning the H_2 and H_3 particles] must be assumed to revert before emitting light". Note the double assumption in their own text, and the implicit contradiction since single hydrogen atoms were *not* found. In section 3.6 below, we will return to this topic and present three possible reactions to account for how diatomic hydrogen particles are formed in the interelectrode space, and how, in the post-cathodic space, the canal rays formed by these particles interact through collision with electrons to generate transient atomic hydrogen (free radical) that recombines to produce H_2 particles (a similar model applies to triatomic hydrogen particles). Thus, from our viewpoint, and as we shall repeatedly explain in the pages that follow, it is apparent that *before* emission there was no singly-ionized diatomic hydrogen, no H_2^+, nor any atomic hydrogen (so that I&S' assumption of "reversion" is totally unwarranted), but rather a proton doublet or its equivalent, an H_2^{++} ion. Further, that *upon* emission of the desired line of interest, ie the photon Balmer line (denoted as H_β), *atomic hydrogen was transiently formed* (formed for the first time in the I&S experiment), but that it is indeed short-lived because it readily recombined with the other proton of the doublet to generate the H_2^+ *that was found*.

2. A note on fundamental values and your discrepancies

As a side note, we should remark that there is no fundamental discrepancy with respect to the fundamental symbols and their definitions employed in your commentary, except that (1) your arbitrary usage of the symbol ee to denote an imaginary aetherometric charge e with the wrong corresponding value that you somehow calculate in coulombs; (2) your writing electron-volts as eυ, whereas we write it in the usual form as eV; and (3) the symbol that you and we use for speed differs, ours being v and yours being at times *v* (which we do not use because it is easy to confuse with the symbol for frequency), or, in error, at other times υ (the symbol for frequency). Aside from this, there are some fundamental differences in values employed as shown below (for the sake of clarity we specify those values *both in the aetherometric and the exactly corresponding conventional* systems of units). Many of these differences are due to the fact that over the years we have stuck with, for continuity's sake, the 1986 Codata values *of some* of the physical constants. We have attached a paper that we have just published on the determination of alpha, which includes comparisons of our results for fundamental constants obtained with both 1986 and 2006 Codata values - they hardly differ, and the difference does not impact in any significant way any of the calculations or results of our paper on the I&S experiment.

NB - Values that differ from those employed by you are shown below in bold.

Invariant speed of light	$c = 299792458$ m/sec
I&S wavelength of emission (measured)	$\lambda_O = 4849.3$Å
Corresponding I&S frequency of emission	$\upsilon_O = c/\lambda_O = 6.18218*10^{14}$ sec^{-1}
Conv. wavelength of emission used **by I&S and us**	$\lambda_O =$ **4861**Å

Note that you employ the erroneous value of 4861.3Å, out of the blue.

Conv. frequency of emission used **by I&S and us**	$\upsilon_O = c/\lambda_O = 6.16$**730**$*10^{14}$ sec^{-1}
Planck's constant	$h = 3.990313212*10^{-9}$ m^3 sec^{-1} = $= 6.626068*10^{-34}$ J sec
Avogadro's constant	$N_A = 6.022$**136736**$*10^{23}$

You employ $6.0221415*10^{23}$, nearly but not exactly the 2006 Codata value of $6.02214179*10^{23}$.

Mass of electron (aetherometric)	$\lambda_e = 5.485799003*10^{-6}$ m = $= m_e = 9.10938$**9646**$*10^{-31}$ kg
Mass of electron (Codata)	$m_e = 9.10938$**215(45)**$*10^{-31}$ kg

Note that here you do *not* employ the 2006 Codata value for the electron mass, but instead $9.10938188*10^{-31}$ kg.

Mass of proton (aetherometric)	$\lambda_p = 1.007192697*10^{-2}$ m = $= 1.672$**483939**$*10^{-27}$ kg
Mass of proton (Codata)	$m_p = 1.672$**621637**$*10^{-27}$ kg

Note that here you also do *not* employ the 2006 Codata value for the proton mass, but instead $1.67262158*10^{-27}$ kg.

Mass of hydrogen atom (aetherometric)	$\lambda_H = 1.007741277*10^{-2}$ m = $= 1.67$**3394878**$*10^{-27}$ kg
Mass of hydrogen atom (Codata)	$m_H = 1.67$**3532575**$*10^{-27}$ kg

Note that you, on page 1 of your commentary, give m_H quite erroneously as $1.672621637*10^{-27}$ kg, when a simple addition of the masses of the electron and the proton, using your own numbers for the electron and the proton, obviously gives:

$1.67262158*10^{-27}$ kg + $9.10938188*10^{-31}$ kg = $1.673532518*10^{-27}$ kg

Elementary charge (aetherometric and conventional) $e = 13.97017654\ m^2\ sec^{-1} = 1.602177\mathbf{330}*10^{-19}\ C$

Note that, because of a typo (dropping a digit), our text gave e in error as 13.9707 m^2 sec^{-1} and not as 13.97017 m^2 sec^{-1}. However, all calculations were performed with the correct value, not the mistyped one.

Note also that the coulomb value of the elementary charge that we employed is based on the 1986 Codata value for the coulomb as $6.241506363*10^{18}$ charges, and thus different from the 2006 Codata value that you employ, $1.602176487*10^{-19}$ C.

1 electron volt $eV = 9.648623*10^5\ m^3\ sec^{-2} = 1.602*10^{-19}\ J$

Beta coefficient β $\beta = v/c$ where v is linear velocity

You then proceed to recalculate the values of Table I from the I&S paper. We assume that you are doing this in order to ascertain whether using in the computations the present-day values for some of the physical constants – values identical, or close, to those which are used in the aetherometric computations – by itself significantly affects the results. But for this purpose, you did not have to recalculate all the numbers – they all use the same constants, so one example would have sufficed, no? And the pointlessness of your computational zeal is amplified by the fact that the results you obtain – which are nearly identical to those of I&S – are actually wrong! We don't have the time to waste on the silly exercise of showing this in all of the cases, especially since we have no *Mathematica* but only the little fingers and a calculator (and yes, of course, I&S made a typo in their equation on page 222), but we will go through a single example just to dot the i's – say, the first entry for H_2^+ in Table I of I&S, which is the second entry in your Table I.

I&S give 7780 Volts as the potential and the computed Δλ as 14.04.
You see fit to correct this Δλ to 13.9984.
You are wrong. To get to this number you had to employ:

1) your wrong addition of the masses of the proton and the hydrogen atom, as shown above;

3) your wrong emission line for H_β

The proof is banal and a waste of our time, but we will type it nonetheless:

Your computation:
$m_{H2+} = m_p + m_H = (1.672621637+1.67262158)*10^{-27}\ kg = 3.345243217*10^{-27}\ kg$
$0.5\ m_{H2+}\ v^2 = e*7780V = (1.602176487*10^{-19}\ C)\ (7780V) = 1.246493307*10^{-15}\ J$
$v = (\sqrt{1.246493307*10^{-15}\ J})/(0.5\ m_{H2+}) = 8.632689567*10^5\ m/s$
$\Delta\lambda = (v/c)\ \lambda_O = [(8.632689567*10^5\ m/s)/c]\ (4861.3Å) = 13.9984$

Note that with the correct value of 4861Å, you would get the still erroneous result of 13.9975, not the erroneous 13.9984 you got. The same errors apply to the rest of your computations in that Table I, making all the arguments about the Diff's totally superfluous. And the same exact shortcomings apply *in toto* to your Table II supposedly correcting Table III of I&S.

Note further that none of this has anything to do with our paper per se. It is, therefore, a strawman. The fact that you go on to apply Diff. tests to these wrong and thus imaginary differences, simply boggles our minds. But, perhaps, it only mirrors the kind of physics that is now done in the academias. And these Diff. tests, by the way, are also thoroughly contemporary, an updated version of the old formula for relative error. Only now, in these politically correct times, does it happen that neither of the two values is to be selected as being more "correct" or more "in error" than the other, and the "correct" value in the denominator is the consensually acceptable conpromise - the average of the two. Yet we cannot but ask: what does this measure mean? And why is 1% selected as the borderline between significance and insignificance?

Moreover, what matters in that Table I of I&S are not the expected displacement values (used by you in your own Table I) computed from the relationship between voltage and velocity, but the observed values from which v can be determined directly according to the relation

$$\Delta\lambda_o = 0.5\ \lambda_o\ (v^2/c^2) \qquad (A)$$

Lastly, it is worth remarking that, as even your equation numbered 0.2 (on page 3 of your commentary) indicates, the difference (<<0.1%) between values computed by equation A above and the SR equation worked from the voltage values is too small a difference to be relevant. You draw a blank conclusion from this – that "the effect is too small for the experiment to be "really" meaningful". This is an unqualified conclusion. Indeed, for what the experiment claimed to show, that the effect is NOT classical but relativistic, it is ample resolution. What it is not sufficient for is to establish a difference between the predictions of SR and LLR. That is, in fact, the reason why I&S could not buttress their case for LLR, and the reason why fans of SR were never willing to cite the I&S paper! But once the aetherometric treatment of the experiment is applied and electric collisions ARE TAKEN INTO ACCOUNT, the experimental data (the *observed* shifts) appear to have sufficient resolution to permit one to say that the aetherometric method yields results substantially closer to the experimental data than are the approximations of SR and LLR (more on this below).

On one point we agree with you – that the p. 226 expression, and thus the LLR model, is not able to make correct predictions (whereas the SR one is) even when no electrical collisions are present. But that is because the LLR formula is in conflict with the derivation of the Doppler effect from the law of the geometric composition of velocities, not because of the I&S results.

Now, onward to your commentary on our paper:

3. Your commentary on our paper

3.1. The problem with new concepts, and in particular re. photon emission

We can agree with you on the noxious effect of the idiocies of String Theory – the stifling of research in particle and theoretical physics. But we do not believe that Smolin and his sidekicks are less stifling or idiotic in their approach(es). In fact, your complaint that you cannot understand the aetherometric concepts of equivalent wavelength, electric wavespeed, magnetic field wave and so on, is somewhat spurious. Every new theoretical approach has proposed new concepts and functions, and has enunciated them mathematically. We've done nothing different in this respect, since each new concept or definition is accompanied by the exact function or equation. Reading the latter is sufficient for understanding what is meant by the words without turning them into spurious terminology. It is the reader who must make that effort - not the writer, who has, within the space allocated, effectively defined each of these new concepts or functions. And if you made an effort to read any of our publications in Aetherometry (say, the first monograph of Exp Aeth Vol I for a definition of "mass-equivalent wavelength"), then it must be even clearer to you what is meant by these new enunciates.

But we will also agree with you on this: that some (not all) of these concepts ARE in disagreement with the concepts, or shall we say, with the presuppositions made by conventional physics that you were taught in school, and that you yourself have taught.

Little wonder! If it were otherwise, there would be (from our viewpoint) no reason for our aetherometric approach. Indeed, let's just look at a single one of these concepts. It is an unproven tenet of accepted physics that massbound charges radiate photons whether under acceleration or deceleration. But Aspden has shown repeatedly and extensively how this contention, inherited from Larmor, is unproven and erroneous (see for ex. *Lett al Nuovo Cimento* (1982) 33:213). Aspden tries to salvage the Larmor formula, but it is, from our viewpoint and as per our claim, indeed a useless formula. Our work, experimental and theoretical, has consistently shown that emission of *nonionizing* (ie *blackbody*) photons only occurs upon deceleration of massbound charges, and is an energy phenomenon connected to both the maximal value of the accelerating field and the modal kinetic energy captured from it by an accelerated charge. Please consult the entirety

of Vol.s IIA and IIB for a series of systematic proofs of these statements, including the balanced enthalpy solution of the basic allotropic cycle of the atmosphere (presented in our monograph AS2-09).

Photon emission is discussed at length in monograph AS2-08, where we prove experimentally that there is an error in the Hallwacks' experiment and the current understanding of photoelectric emission. This opened new vistas with respect to the understanding of Planck's constant, the definition of a particle (including a photon), the equivalence between the electromagnetic and the electrical treatments of the rest mass (mass-energy or self-energy) of a particle, etc. All these are explored in monographs AS2-12 to AS2-17B, as well as in our Labofex monograph LS1-25.

You think as a mathematician when you claim that every photon is the same as any other one. But this is an abstraction! If you thought either as a physicist, a chemist or a biologist, you would find that photons have very different properties according to their frequency or energy. Some ionize molecules, others do not, and still others which do not, nevertheless induce free-radical homolysis of molecular substrates. These different classes of photons have different physical chemistries and different biological effects. We have published hundreds of pages on this subject (in the series *Experimental Aetherometry* and the book *Nanofunctions of Bioenergy*)– including the indirect derivation of the spectrum of blackbody (nonionizing) photons *in toto*, from the kinetic energy of massbound charges.

We have not yet presented our complete model of the hydrogen emission lines (Vol. III of AToS, long due for publication). We have also held back because we have so far avoided publishing our own work in nuclear physics. But in that work, and with our value of the fine structure constant, we found that our theoretical model placed the lines of the various spectra of hydrogen very nearly exactly where they are experimentally found, ie with substantially greater accuracy than any existing model, and with simpler expressions. There is no substantial disagreement with the determination of the excited states of the hydrogen atom from the QM formula (that you gave on page 4 of your commentary) - except that, unlike accepted QM but in parallel to Randy Mills' claim, we also claim that there is one more excited state, essentially of the magnitude of the Hartree energy and thus twice that maximal value of 13.6eV.

But what is important to retain in regard to our paper is that, whereas electrons accelerating in "free flight" do *not* generate any of these lines, H_β included, the production of any of these lines *by accelerated hydrogen ions that lack sufficient kinetic energy to do so*, requires – ie in accordance to our model – that an electron with x energy *collide* with a proton of y energy, the magnitudes of the two energy terms varying in a reciprocal fashion. The unique signature is therefore not a property of an hydrogen atom per se, but more properly speaking a property of a combination of an electron and a proton that reaches the energy required for Balmer emission (in other words, the Balmer line can be emitted from *formed* hydrogen atoms as well as from, or upon, the *formation* of hydrogen atoms). An hydrogen atom is formed upon the emission, and thus the line is post-factum attributed to it. Yet, when present and necessary, the collisional reaction is entirely glossed over and thus misunderstood. Precisely, in our view, this is the pitfall avoided by Aetherometry in its treatment of the emission in the I&S experiment.

That you missed this completely, including all the caveats that were put into our paper, when it is crucial for the aetherometric approach to the I&S experiment, is somewhat astonishing. Moreover, your objection that in Aetherometry the emission would seem not to be unique to atomic hydrogen is a red herring, since *it is characteristic of atomic hydrogen* when certain kinetic energy (-ies) of its components is (are) lost upon their collision. That is what the paper was designed ultimately to demonstrate – the existence of this unsuspected physics of collision that leads to the manifestation of a unique line *for different states of motion of the colliding particles* (the canal rays and the electrons released from the post-cathodic sheath). This is precisely the point that you concur was missed ("the paper of IS left open the problem of how actually the hydrogen atoms were formed out of the originating molecular ions") by the I&S analysis (and everyone else ever since!), even if it is unclear whether your sentence is intended to apply to what happens in the interelectrode space, in the postcathodic space of the canal rays, or in both. Again, it is our view that it is in the physics of the process that the current physical theories come up short.

The aetherometric rules of the combinatory of the different states of motion of the proton and electron are not probability rules, or reducible to such a treatment. They are part of an altogether new approach to quantum processes. They give exact values for the varying combination of two distinct kinetic energy terms, as is manifested, for example, in the relations established in our paper by the values shown in Tables 4 and 10. We would rather say that the probability treatment of modern particle physics is a gross approximation to the simple algebra that determines the combination of these distinct energy states of the two particles composing the hydrogen atom. Thus, when you categorically state that

"the aforementioned probabilty depends solely on the structure of the system "Hydrogen atom", and not on the state of motion of the center of mass or the velocity or the kinetic energy of the eletron",

we must say that this is simply an axiom (and a dogma) of a probabilistic approach, not a physical parameter that has been empirically confirmed. Indeed, it is this axiom that (meta-physically) blocks a dynamic collisional analysis! For, as we have shown in our paper, the better explanation that yields results closest to the observed data is based precisely on the states of motion of the two composing particles, ie their velocity and kinetic energy just prior to collision. Not to see that *this* was the properly aetherometric challenge is to miss entirely the advantage of our new algebra over combinatories of mere probabilities - and thus the point of the paper.

In this context, you ask:

"Is a massbound charge a particle subject to the laws of quantum mechanics ? If yes, how come the velocity is a well defined quantity, given W_m and W_v ? Are wave speeds, in every situation of the massbound charge, well defined quantities ?"

From our simple answers to these questions, you can perhaps begin to realize the enormous distance between Aetherometry and the Heisenbergian legacy of modern particle physics. Yes, it is subject to the basic laws of "quantum mechanics", but these laws are not exactly the same for Aetherometry and QM! Precisely, the very interpretation of Heisenberg's postulate is different (we shall shortly publish two papers on this subject that formally prove the errors of the present theory of uncertainty as applied to physics). Indeed, all the wavespeeds and their resulting linear speeds are well defined and *definable*, or determinable, quantities (on this alone, Einstein might well be proud of what we have accomplished…).

Moreover, since, strictly speaking, the aetherometric treatment is not in conflict with the accepted determinations of the H emission lines – only with Larmor's theory of radiation from moving charges and with Bohr's model of the atom – nothing in it is literally beyond your grasp (though it may be beyond your imagination). Simply put, it is only the treatment of the WHOLE system (proton plus electron) that diverges, and for good physical reasons: after all, there is no I&S result that is not the product of a collision between charged hydrogen ions and electrons!

You proceed as if you needed to teach us very basic tenets of physics (even if you think we already know it, or should know), when our very work has examined at length why a variety of these sacrosanct tenets, including those you mention, are neither experimentally verifiable, nor correct; or worse, they are like the above statement regarding whether or not emission occurs as a function of a probability: impediments to the development of our understanding of natural processes.

In your commentary, you repeatedly act as if we were throwing out the baby with the bathwater – when we are not. This should be pointed out, since it is tied to the general strategy of discrediting by dangling a red herring. In the context of such a strategy, you assume, without reason, that we disagree with the Balmer line being a unique marker of an hydrogen atom, when it is clear from what we have been saying that in any region where there exists hydrogen, the presence of the Balmer line is always indicative of the presence *or* the formation of *atomic hydrogen*, and thus, indeed, is a unique marker. Moreover, with reference to your statement -

"No other emitter can emit H_β, certainly not an elctron which , when accelerated, emits a "continuos" spectrum ."

- we're compelled to say that, according to Aetherometry, an electron *never* emits photons while *under acceleration*! Red herring follows red herring, and with so much herring around, soon one should be able to feed a veritable Godzilla...

We do not think that such reactions are rational responses or that they are elicited

by the contents of our paper. Its succinct section on new methodological principles should suffice plenty for a trained reader to understand how the few new concepts and expressions relate to the traditional ones, and how they are used to get at an original collisional explanation for the results observed by I&S – an explanation that is more accurate in its predictions than either SR or LLR. In fact, the beauty of our paper lies in the algebraic simplicity of the new relations that we introduce. (Please note that the inclusion of a longer section on the new methodological principles or their elucidation would also have been unacceptable to any journal.)

3.2. The passing remark on clocks running slower or faster

You write:

"At page 6 you use the sentence "clocks run slower the faster they move", I think this sentence, which can be found in many papers and books, is "TOTALLY" misleading, Einstein,in his 1905 paper, was very clear about the fact that the Laws of Physics remain unaltered going from one inertial frame of reference (IFR) to the other, therefore it is only when the observer in one (IFR) reads the clock of the moving(IFR) that , FOR HIM ,(or HER as one is,so ridiculously forced to write nowadays)the clock slows down , for the "moving observer " the clock ticks exactly as for the "still" one!"

The sentence "clocks run slower the faster they move" is one of those sentences that is not worth quibbling about. Both comparative adverbs imply in English that the reference of the clock moving faster with a slower time is *not its own inertial frame*, but the inertial frame of some other clock to which it is compared. In its own inertial frame, the time that is running slower (or faster) is supposed to be moving just as slow or just as fast as in any other inertial frame –given the interchangeability of inertial frames.

Of course, for Aetherometry, this entire notion of different rates of time flux is a ridiculous one, the result precisely of misunderstanding what is happening in an experiment like that of I&S (even the title of their paper is evidence for this misunderstanding!). We have shown this to be, in part, the result of an uncritical approach to the coincidence of the SR formula with the simple derivation of the law of the geometric composition of velocities (a paper that has now been released as the 3rd monograph of Vol. I of AToS). It is easy to demonstrate that, as long as SR analyzes an experiment like that of I&S as being collisionless, the second order result that it will predict *can NEVER be correct if collisions are in fact involved*, since it is only the application of the law of the geometric composition of velocities that permits the determination of *what the correct displacements ought to be* from the velocities (plural) of the TWO colliding particles, and with mathematical operations exactly identical to those of Aetherometry. For, in the second case - ie the instance of Aetherometry in an I&S-type experiment - the resulting shift is always the result of two combined motions, whereas in SR it will always be determined from the shift attributed to the motion of ONLY ONE OF THE TWO PARTICLES THAT FORM THE WHOLE SYSTEM! In this respect, both the SR and the LLR treatments come up short or unable to deal with the WHOLE PHYSICAL SYSTEM…

You should have realized that Aetherometry is not disputing the result of the coinciding equations of SR and the law of velocity composition under qualified conditions (note, in fact, that we draw the attention of the reader to this fact in the fifth note of Table 12: the values of Aetherometry for proton doublets BEFORE collision are identical to the values predicted by SR for the singly-ionized diatomic hydrogen in the I&S experiment; see our Table 11). Essentially, what Aetherometry contends is that simple application of these equations to a single particle of a two-particle system is erroneous, simplistic, and can never be as accurate as *the two-particle* analysis proposed by Aetherometry. This, of course, is the point of Tables 10 and 11, which compare the charge carriers BEFORE and AFTER collision, and thus the point of the differences presented in Table 12. Since all these relationships were arrived at without a single invocation of time-dilation or any other LF transform, it seems only reasonable to conclude that neither SR nor LLR have the right of invoking the I&S experiment as a confirmation of the relativity of time flow with respect to states of motion. Such a claim is patently abusive, even if the entire scientific community were to believe otherwise.

Anyway, we have said as much in our explanatory letter to you on 26th June 2008, reproduced below as Appendix A.

3.3. The red herring that our value and employ of charge e is not consistent

You are right in thinking that equation 4 is one of the key equations for the new system of units. But you cannot bring yourself to reason or understand how the proposed conversion is an exact one, thinking rather that it is metaphoric, like those of Geometrodynamics (eg c=1=G).

How, from this refusal to take the proposed conversion literally, you arrive at the confabulation that we are inconsistent in our usage of the symbol e for charge is so preposterous that it is worth examining in detail.

First, succinctly, what we have written and published, what is an integral part of our paper on I&S, and what we consistently hold, is:

1. The charge e can be written either with conventional units (SI, cgs) or with aetherometric units. The two values have a STRICT equivalence. Let's write the exact equivalence between SI and aetherometric units:

$$e = 1.602177330*10^{-19}\ C = 13.97017654\ m^2\ sec^{-1}$$

Often, to detail in which system the equation is written, we use e for the SI system, and p_e for the aetherometric meter-second system. If possible, when the system of units is readily apparent, e alone is used.

2. As Aetherometry alone contends (ie because of the equivalence of mass to wavelength), the correct dimensions of e are interchangeably (as long as the units are properly employed):

$$e = MLT^{-1} = L^2T^{-1}$$

As such, charge has the dimensions of linear momentum.

3. To write electric energy (whether this be a kinetic term or the term for mass-energy) is to write it in terms of charge and voltage; that is the basis itself of the concept of an electron-volt as a unit of energy. Dimensionally and traditionally:

$$eV = (MLT^{-1})(LT^{-1}) = ML^2T^{-2}$$

1 electron-volt is the energy quantity formed by the product of the elementary charge e by the potential corresponding to one volt. Since there is equivalence between mass and wavelength, the dimensionality of the electron-volt can also be aetherometrically written as:

$$eV = (L^2T^{-1})(LT^{-1}) = L^3T^{-2}$$

4. In the specific case of massbound charges, the elementary charge function is also the product of the mass of the particle (ie the 'charge carrier') and its intrinsic (or constitutive, and thus *unique*) magnetic wavespeed, W_m. We can express this in either system of units, and abstractly write (with the sign =∫= denoting that we're changing from the SI system to the aetherometric system of units, or from mass units to units of length, in meters, by employing the exact conversions we've proposed):

$$e = m_e\ W_{me} =\int= \lambda_e\ W_{me}$$

5. For ordinary ("nonrelativistic") velocities the relationship between voltage and linear velocity simply involves the characteristic magnetic field wave. The basic expression for this is equation 5 in our paper.

$$v = \sqrt{(W_v\ W_m)}$$

where W_v is the electrical potential expressed directly as a wave speed.

6. The proposed direct and linear relationship of voltage V to wavespeed W_v can be determined by *a variety of methods*, always giving the same result. We will show a method based on the mass-energy of the electron itself. This is equal to the Jeans-Einstein expression and its aetherometric exact equivalent

$$E_e = m_e\ c^2 = e\ W_{v511k}$$

Where W_v is the wavespeed equivalent to 511 kilovolts. Thus we can say that the mass-energy of the electron is equivalent to

$$E_e = 511 \text{ kilo electron-volts}$$

or

$$E_e = 8.1870*10^{-14} \text{ coulomb*volts, or joules (by definition)}$$

or, still, in aetherometric terms, to

$$E_e = \lambda_e\ c^2 = p_e\ W_{v511k} = 4.93*10^{11}\ m^3\ sec^{-2}$$

It follows that it is easy to determine the effective speed content of W_{v511k} in the above expression (using only aetherometric units, and thus the aetherometric value of e given above):

$$W_{v511k} = m_e\ c^2/e = \int = \lambda_e\ c^2/p_e = 3.529*10^{10}\ m\ sec^{-1}$$

Divide that by the number of volts (in fact, by 510,994.1496 volts), and you get the basic aetherometric equivalent of the volt as:

$$1 \text{ volt} = 69{,}065.86\ m\ sec^{-1}$$

Multiply charge e by that value for the volt, and now you get (for the first time in the darn history of physics) the simple aetherometric, or meter-second equivalent of the electron-volt:

$$1\ e * 1 \text{ volt} = 1 \text{ eV} = 964{,}862.37\ m^3\ sec^{-2}$$

As a good physicist, then note that this has the dimensions of energy and corresponds exactly to $1.602177330*10^{-19}$ joules.

This is the basic aetherometric presentation. But instead, what do you infer from our paper, regarding our supposedly inconsistent usage of the charge e and its magnitude?

You propose a number, the only one that "makes sense" to you - which you nonsensically write as:

$$1 \text{ x e x V} = 964886 \text{ x ev} = N_A \text{ x ev}$$

It seems to us that all that you are doing is multiplying Avogadro's number by 1 electron-volt to get a quantity whose proper designation would be *a mole of electron volts*; with our usual Codata values:

$$1 \text{ mole of eV} = N_A * 1\text{eV} = 96{,}485.3 \text{ joules}$$

This is not 964,886 electron-volts!, nor the aetherometric equivalence that escaped you: 1 eV = 964,862.37 m^3 sec^{-2}!

How you get from one mole of electron-volts (ie your "N_A x ev") to whatever "964886 x ev" is, only to conclude that our "unit of charge would be then 1.14681 x 10^{-19} C" (sic) verily escapes us! You can be derisive of "the mysteries of Aetherometry", but it seems that the mysteries of your reasoning are far more unfathomable...

Check above, there is no way that such a preposterous result (1.14681 x 10^{-19} C) can be obtained from the aetherometric tissue of relations. There is only one charge function e, with the strictly equivalent values that we showed in section 2 above!

3.4. Other *non-aetherometric* speculations: X-rays and UV photons

To prove that you failed to understand the aetherometric relation between the voltage or wavespeed of the accelerating field, and the energy or frequency of photons emitted from *decelerating* massbound charges that were accelerated by the field, you give the following example:

"To give you an idea of how much I am far away from understanding your physics, I give you here a reasoning which I thought up, just to see if I was getting anything. Suppose I consider the nucleus of a radioactive element which decays via emission of X rays (to all intent and purposes the electromagnetic decay of a nucleus is identical, as a physical process, to the electromagnetic decay of an atom), then :

frequency = 10^{17}

velocity = $\sqrt{}$(frequency * e)

velocity/c $\approx$ 4!! "

This shows that you failed to realize several basic aetherometric rules:

1. That the proposed relationship ONLY AND EXPLICITLY applies to the production of blackbody photons, ie *nonionizing photons*, or to the 'continuous part' of the em spectrum, and thus does NOT apply to X-ray or gamma ray photons! We say so in our paper.

2. That no massbound charge can be "inertially accelerated" to speeds above the invariant lightspeed c.

This last point can be shown easily from the computations based on our equations. You misunderstand how the equations work together. You state:

"O.K. Your theory applies only when v/c is < 0.85 (this already, in itself, is a very peculiar limitation)"

But you are totally wrong in thinking that our theory only applies up to v/c = 0.85! That is not the case! What the sentence in the paper stated and meant was that, up to that value, or thereabouts, there is NO NEED to introduce the *complete* set of equations, since the result will be PRACTICALLY the same (much as de Broglie created two sets of equations for the total energy - one set, the classical one, applying approximatively for as long as the motion of the particles was nonrelativistic; this is an analogous situation). In other words, the aetherometric predictions regarding the results of the I&S experiment are not substantially changed in any way if the full set of aetherometric equations is employed, since the speeds involved in the experiment do not go beyond some 0.005c!

That's all that is being said. The notion that our theory does not apply with speeds beyond 0.85c is only in your mind.

For *the complete set of the equations* - and a realization of their distant kinship to Autodynamics - see Appendix B below, which contains an excerpt from AToS, Vol. II, monograph AS3-II.10. There, it is plain to see how the relationships introduced in our paper on the I&S experiment hold firmly in the context of the "wider picture". (To understand the basic relationship between magnetic and electric wavespeeds, as well as the proposed toroidal structure of the electron, one can read the introductory material in LS1-25.)

Lastly, let's take the example of an UV photon, say, emitted from an electron, and how it is treated by Aetherometry. What is the predicted kinetic energy and linear speed of this electron prior to emission? We will assume that the electron was maximally

accelerated by the field, and that it emits upon deceleration. We will assume *your* photon frequency of 10^{15} sec^{-1} for purposes of correcting your wrong deductions and computations. From equation 8 of our paper (and with the strict conversion of e between SI and aetherometric units, see above), the voltage of the charge accelerating field would have to be:

$$W_v = e\,\upsilon/W_{me} = 5.4*10^9 \text{ m sec}^{-1} = 79.4 \text{ kV}$$

It is evident that the kinetic energy will be 79keV (NOT the ridiculous 40 MeV that you pull out of a hat on p. 6 of your commentary!!), and that the linear velocity will be (on a first approximation, ie without employing the equations of Appendix B):

$$v = \sqrt{(W_v\, W_{me})} = 0.394\ c = 1.18*10^8 \text{ m sec}^{-1}$$

Thus your conclusion -

"There exists, to my knwoledge, NO nucleus to which an electron could be bound to form a stable atom to emit the ultraviolet radiation, having THAT kinetic energy."

- which refers to that ridiculous 40MeV figure turns out to be yet another red herring dropped out of the blue. No aetherometrist would quibble with it, since simple algebra demonstrates that the UV-emitting electron DOES NOT have any such ridiculously high kinetic energy.

3.5. On the proton doublets claimed solely by Aetherometry

So we come to the doublets at last - something we will further clarify in the next section when explaining the reactions going on inside the I&S vessel.

Your first salvo in this respect is:

"Now the business of PROTON DOUBLET : to my knowledge, after nearly one hundred years of handling and studying protons, in all the laboratories of the world, nobody has ever mentioned, or even suspected, that an "illegal" marriage could exist between two protons for the two to constitute a happy couple travelling together without any bond !"

That's right, but *not without* a bond - with a *noncovalent* bond! This is precisely an aetherometric suggestion. You would have to understand what is in Aetherometry the relationship between spin and charge. For, after all, the same problem of coupling *like* charges once existed in chemical bond theory, when two electrons had to be stacked in the first known orbital, the 1s pair, and so on. In Aetherometry, we found and have proposed that, in the same way that "orbital electrons can be geometrically stacked, so also can be any massbound charge, whether in relatively stable atomic or nuclear constructs, or in "free motion". Such stacking must be the result of a like-charge alignment occurring when hydrogen ions passing through the perforated cathode form the canal rays. Our suggestion is that the canal rays that generate H_2 particles are formed by stacked doublets, and that emission of the line of interest only occurs when a proton of the doublet collides with an electron (more on this ahead).

You, however, miss this point entirely out of sheer incredulity combined with very poor reading that generates more red herrings still; indeed, you write:

"You must admit that reading the sentence, "we have to treat its (of the doublet) overall W_{mag} as being twice the value for H_2+, ie identical to that of the proton", one wonders what kind of notion must be attached to W_{mag} for it to be the same whether referred to one or two protons."

We admit nothing of the sort!!! In fact, we will formally prove it with equations B, I and J below.

But for now, we should note that you do a particularly specious turn of the screw here, for you have MANGLED the sentence that you quote totally out of context. The FULL sentence, with that which you removed now placed in italics, reads:

"*When we think of a proton doublet as forming a doubly ionized molecular hydrogen ion*, we have to treat its (of the doublet) overall W_{mag} as being twice the value for H_2+, ie identical to that of the proton."

It is when the doublet is treated as an hydrogen molecule, that the part of the sentence quoted by you applies. And it is obvious why - or should be, since in the sentence prior to that one, we write that proton doublets can *either be written as two protons or as a doubly ionized molecule* - "which we can write as $2H^+ = H_2^{++}$ " (page 9 of our paper). In other words, since each proton of a doublet carries a charge, one EITHER treats these particles as 2 protons, OR treats them alternatively as a doubly ionized hydrogen MOLECULE. This is so straightforward that one can easily express it algebraically for the magnetic wavespeeds involved, which are indeed the same:

$$W_{mp} = e/m_p = W_{mdoublet} = 2e/2m_p \qquad (B)$$

The first two expressions apply to single protons, and the last two to proton doublets.

So, your 'biting' irony ("one wonders what kind of notion must be attached to W_{mag} for it to be the same whether referred to one or two protons") is as misplaced as your contempt and ridicule for Aetherometry, which you have judged rather irrationally *by mistakes that are*, after all, without a single exception so far, *your own*. For, indeed, whether we treat the particles in question as singlets or doublets is only a matter of physics (for mathematically they are identical), and not some 'wild reason' to be wondered about! It is a simple fact: in both the singlet and the doublet treatment, the magnetic wave function is consistently employed and has, indeed, lo and behold!, the same value!

Your explanation for your 'argument' is to compute conventionally the same velocities that are shown by us in column 5 of Table 4 as what we claim are the *wrong* computational values of SR or Lorentzian electrodynamics. As you were taught, you use the relation

$$v = \sqrt{[(2e\ V)/m]} \qquad (C)$$

which Aetherometry claims is incorrect, and that the correct formula should be:

$$v = \sqrt{[(n\ e\ V)/m]} \qquad (D)$$

where n is the number of charges.

Let's illustrate this by using a single value from the Table that you present at the top of your p. 7, the first value for the singly-ionized H_2^+. For an applied potential of 7,780 volts, you give, as does SR, a speed of $8.633*10^5$ m/s. Now see what happens (keeping in mind that the mass of H_2^+ is, in aetherometric terms, 3673 times the mass of the electron or its mass-equivalent wavelength, and thus $2.0149*10^{-2}$ meters) with your formula:

$$v = \sqrt{[(2e\ V)/m_{H2+}]} =$$
$$= \sqrt{[2\ (13.97017\ m^2\ sec^{-1})(69{,}065.86\ m\ sec^{-1}\ per\ volt)(7780\ volts)/(2.0149*10^{-2}\ m)]} =$$
$$= 8.633*10^5\ m\ sec^{-1} \qquad (E)$$

That is the speed reported by I&S for the same carrier, H_2^+. By the same conventional formula, and for the same voltage, we would instead have for protons:

$$v = \sqrt{[(2e\ V)/m_p]} = 1.22*10^6\ m\ sec^{-1} \qquad (F)$$

Applying the same conventional formula to the doublets whose existence current physics ignores - and whose mass is 3672 times the mass of the electron or its mass-equivalent wavelength, and thus $2.0144*10^{-2}$ meters - we would then get:

$$v = \sqrt{[(2e\ V)/m_{H2++}]} = 8.633*10^5\ m\ sec^{-1} \qquad (G)$$

Now, what happens *when the aetherometric formula that we claim is correct is applied instead to all three instances*? The value for *singly ionized* molecular hydrogen is:

$v = \sqrt{[(e\ V)/m_{H2+}]} =$
$= \sqrt{[(13.97017\ m^2\ sec^{-1})(69{,}065.86\ m\ sec^{-1}\ per\ volt)(7780\ volts)/(2.0149*10^{-2}\ m)]} =$
$= 6.103*10^5\ m\ sec^{-1}$ (H)

After all, singly ionized molecular hydrogen only carries ONE positive charge! For protons (singlets), which also only carry a single positive charge, the aetherometric correct equation is:

$$v = \sqrt{[(e\ V)/m_p]} = 8.633*10^5\ m\ sec^{-1} \quad (I)$$

And for proton doublets, which (lo and behold!) carry 2 positive charges (ie 2e) for *doubly ionized* molecular hydrogen, and with the magnetic wave expression discussed above now also made explicit to underscore *the complete consistency* of the aetherometric algebra:

$$v = \sqrt{[(2e\ V)/m_{H2++}]} = \sqrt{[(2e\ V)/2m_p]} = \sqrt{(W_{v7780}\ W_{mdoublet})} = 8.633*10^5\ m\ sec^{-1} \quad (J)$$

We can put all these results into a simple table comparing the velocity values given by SR/Prof.X and Aetherometry for a single potential of 7780 volts applied to either singly-ionized molecular hydrogen, doubly-ionized molecular hydrogen (doublets), or proton singlets:

TABLE A

Substrate	**SR/Prof. X** 10^5 **m sec^{-1}**	**AToS** 10^5 **m sec^{-1}**
H_2^+	8.633	6.103
H_2^{++}	Don't exist (8.633)	8.633
H^+	12.21	8.633

It is readily apparent that, from the aetherometric perspective, *if no protons are found* (as I&S demonstrated experimentally in their set-up), the canal rays can only have the speeds claimed by I&S *if they are doublets*, if they are double charge carriers, and not singly ionized molecular hydrogen, as claimed by I&S. Yes, we claim that this is their error, and an error that has remained undetected to this day *because of the use of incorrect formulas both in SR and LLR*. That's right. So, to counterpose to your table at the top of your page 7, we built a table from column 4, Table 3 of our paper on I&S (all the information is there!), which demonstrates the epochal error that we claim to have identified in the interpretation of the relationship between voltages and linear speeds for the relevant hydrogen species:

TABLE B

Voltage	**SR/Prof. X for H_2^+** 10^5 **m sec^{-1}**	**AToS** H_2^+ 10^5 **m sec^{-1}**	**Doublets H_2^{++}** 10^5 **m sec^{-1}**
7,780	8.633	6.104	8.633
9,187	9.381	6.634	9.381
10,574	10.06	7.117	10.06
13,560	11.40	8.059	11.40
18,350	13.26	9.375	13.26

It is evident that for SR/Prof.X, the canal rays can only be H_2^+ (since doublets cannot exist), whereas for Aetherometry *they can only* be H_2^{++} (ie doublets) and NOT H_2^+.

Failing to realize entirely the essence of the aetherometric argument, you persist in attributing to us the (false) claim that proton singlets were involved in the experiment:

"What is stated at the bottom of page 9, after considerations which are based solely on Aetherometry (?), are in definite contrast with the description of the experiment given by the authors,in fact, they did not see any single proton effect and maintain (correctly in my opinion) that the potentials they applied DO accelerate the indicated ions to the given velocities."

And then you adopt an indignant attitude - as if our argument, that the real substrate of their canal rays was H_2^{++} and not H_2^+, somehow had to be faulty a priori because I&S could not be wrong about their "understanding [of] their own work !?!?":

"Now, at this point, one seriously wonders what is the idea of the whole paper: the authors are to be trusted, in as far as they produce numbers coming from the set up of their experiment, but are completely wrong in understanding their own work !?!?"

This is a particularly amusing tone given that you indeed claim that I&S were wrong about the LLR interpretation ("understanding") of their experiment, and that you opened your commentary by doubting even their competence as experimentalists - "the final results are, to say the least, not quite convincing" were your words.

You emphasize this:

"Saying that " only protons or proton doublets can be accelerated to the reported velocities with the voltages applied by Ives and Stilwell" is tantamount as saying that they really did not know what they were doing, so, where do I go from here ? Believe two established physicists, whose work you intend to utilize, or believe your incomprehensible Aetherometry?"

This 'reasoning' is a riot. Here we thought that science, of all 'things', was about demonstration, closeness to experimental facts, theoretical cogency, a qualified belief based on data, etc - not a matter of belief in a "*higher authority*". The only consolation we're left with is that if I&S in effect did not realize what they were doing, you manage to realize even less what you have read in our paper.

You add -

"Well they might not have been perfect, they might have had their own funny ideas about Einstein and relativity theory, but their paper,with all the aforementioned limitations , gives me the impression that they were good experimentalists and definitely knew what they were doing."

This implies that if a scientist is a good experimentalist he cannot, in principle, make mistakes - especially somehow mistakes in understanding... Science would then have to be a static body of dogmas.

These imaginary problems you raise resolve mostly to poor reading. Here is another straighforward example of this fact:

"As regards your Table 4, there are indeed some strange features :
1) I am not able to find anywhere in the paper of Ives and Stilwell the numbers appearing in column 6 !?! Attributed and computed !?!"

Yet, that column 6 states that for the first three values, *the attribution was made by I&S* (the values come from their Fig. 9), and that the repetition of these values on the bottom of column 9 is "where the I&S results belong" according to Aetherometry.

Regarding your second objection to our Table 4: the proton velocities of Table 4 and their correct and incorrect computations (according to us) were already discussed at length above. The SR values shown in column 5, Table 4, are only correct for the doubly ionized molecular hydrogen that is not recognized - and thus are incorrect also for the proton singlet. The conventional formula is wrong for the reasons already cited above.

Regarding your third objection:

"3) The second footnote refers to the fact that you use a ratio m_p/m_e equal to 1836, intead of the conventional one 1836.16, this fact is irrelevant, in the context , unless you want to take into account the mass of the electron , which is certainly not the case."

Yes, it is of course irrelevant. It's noted there for reference purposes. But you should not assume that the ratio can be applied in reverse to the electron. *The electron only comes in one mass species*; the value we use is what we reported above in section 2, and is consistent with the Codata value. It is the proton which, in Aetherometry, *comes in various sizes*, 1836.16 being effectively a kind of mean of these different sizes. But none of this is relevant for what is at hand.

Now for the fourth objection. You state with reference to the formula written in the second footnote of Table 4 - ie

$V = 300\ (m\ c^2)\ \{[1-(v^2/c^2)]^{-0.5} -1\}/e$

- that the formula is wrong:

"The formula for the potential is wrong, as written, witness the numbers which I have calculated for the four following cases."

You are wrong; the formula is correct. You just failed to realize that it is expressed in the cgs system, where the mass to charge coefficient for H_2^+ is $2.075*10^4$ gm/emu and all speeds are in cm/sec. So the Table on page 8 of your commentary, with calculations that you numbered I to IV, is altogether spurious. And instead of wasting your time performing 20 calculations, why didn't you just ask, ie avail yourself of direct communication with us as you had said you would if needed?

In any case, to use the formula in the SI system, all that you had to do was drop the 300 factor. In fact, you made a mistake in the SI formula you wrote in your commentary; you wrote:

$Pot = mc^2(1/Sqrt[1 - v^2/c^2] - 1)$

when, in fact, this is *dimensionally wrong* (it has the dimensions of energy) because it is missing the division by the elementary charge e, so that the correct formula in the SI system of units (not the cgs system), with our notation used in Table 4 of our paper, is:

$V = (m\ c^2)\ \{[1-(v^2/c^2)]^{-0.5} -1\}/e$

or, with your notation:

$Pot = mc^2(1/Sqrt[1 - v^2/c^2] - 1)/e$

We note that you must have used this (ie the correct) equation in your calculations, since your values in the 3rd column of that table on page 8 of your commentary, can only be arrived at by dividing by e. Note that the values in Table 4 of our paper were actually arrived at, not with the 300 factor, but with 299.79 etc, in the cgs system. One could replace these values in column 5 of Table 4 with those in the 3rd column of your table on page 8 without any impact upon what our Table 4 claims to demonstrate, or its consequences in the context of our analysis of the I&S experiment. In other words, 'our' formula which you find so erroneous is simply the formula of SR expressed in the cgs system.

And then, in a particularly silly conclusion to an already silly argument, you exclaim:

"I think you have to agree that, in writing a scientific paper, one symbol must have the same value all over !"

This is a total *nonsequitur* - what symbol are you referring to...???

So, that entire section of your commentary, with its four useless sets of calculations, was a big vacuous hullaballoo made out of nothing.

The most we can think of, is that we could have written the equation on the SI system just by dropping the factor 300 (actually we did this change in the manuscript, but was never typed into the final edit). A good reviewer would have simply recommended something like "drop the 300 factor to be consistent with using only the SI and aetherometric unit systems". Who, after all, does not know that that is the difference between the two expressions in the SI and cgs systems?? This is what truly irks us about careless reviews - the endless useless time one spends correcting negligent readings of the material supposedly being reviewed with care and attention. We have had only too many experiences of this kind to assume that they are either random or justified (more on this ahead in the peer-review section 2.9).

Still on proton doublets, you write:

"Coming now to your description of the process whereby an electron collides with the "doublet",a hydrogen atom is formed, the Balmer line is emitted etc. I just have no words : do you mean to say that nearly eighty years of Quanum Mechanics have just to be forgotten ? Qualitative descriptive terms, like:"bombarding ", "satellizing" etc have no room in a rigourous treatment of the problem. One should talk of absorbtion crossections, probabilities of creating a given excited state etc. The intuitive planetary description of atomic structure was useful in the beginning of quantum theory, but nowadays .. When I was young I had the wonderful opportunity of discussing Quantum Mechanics with Prof. Pauli and he explained to me that our ordinary space time description of the physical world is no longer tenable as a consequence of the existence of the Quantum of Action, our

ordinary way is "anschaulich" but the quantum mechanical description is "unanschaulich" and to be understood only in mathematically well defined notions."

We could not do justice here to our views on this subject. You would have to read (actually *read*) all the various papers we have already published on the foundation of a very different theory of so-called quantum mechanics, and then we might be able to discuss this with you. Neither Pauli, Fermi, or Feynman, etc, etc, are - in our view - some form of last, unquestionable authority on this matter. After all, QED still cannot explain charge in terms of spin, or by any other sensical physical mechanism. Aspden has written some profound pages on this matter (see for ex. the first two chapters of *Physics without Einstein*, or a feature dedicated to the nature of charge in his website), which, even if in partial error, should make every physicist ponder. In any case, you are wrong in attributing to Aetherometry a classical view, and in total error in thinking that our model of the atom is planetary...Could not be further from it! Usage of expressions such as "satellizing" is a concession to other descriptions and, incidentally, a common textual procedure not indicative of any theoretical agreement with the classical view (it is all a matter of scale in a description or in the use of analogies).

As for the aetherometric theory of Space and Time being distinct manifolds (and there being no Spacetime...), we can only direct you to what we have published already (viz. the introductory chapter in the book on nanobiology, the two papers on SR and GR, the introduction to Vol. IIB of Exp Aetherometry, etc), and other papers still forthcoming on the same subject.

Paradoxically, however, when it comes to QM we are on the side of Einstein, and believe that what he was missing was the algebraic theory that we continuously developed and enunciated for now well over a decade. If you want to understand it, you should first become familiar with the material we have already published, and exercise a good dose of good-will to read it with the attention, thought and care it deserves.

Returning to the doublets (to finish this section), you write:

"Quite apart from this, however, even if one accepts the idea of "doublets", one should justify the occurrence of the collisions you need in your description, first of all by showing that the electrons are indeed there and with the necessary velocity distribution, and, secondly, that the probability of such a collision is, in fact, such as to make the whole argument tenable (since the doublets are, to all intent and purposes, as far I can envisage, just two protons, the collision turns out to be a three body collision, and, as such, extremely unlikely)."

No, they are two protons that form a unit, two protons in a stack, no different from the two protons of any diatomic hydrogen! That is what we wrote and are saying, not what you keep trying to put in our mouths.

Moreover, we do not need to prove that the plasma sheath over the glass is able to emit electrons; a reference should suffice. The phenomenon has been known since the 1920's, and has been studied more recently in Hall thrusters (Phys Plasmas (2001) 8:5315; Phys Plasmas (2003) 10:2574), at much lower potentials. We ourselves ran into it (including elastic acceleration of electrons) in our development of the PAGD reactors. It was an impediment to sustaining rates of discharge and favoured the onset of a vacuum arc. We eliminated it by an original and unpatented design, and once built and tested it completely inhibited the development of a plasma sheath over the dielectric. Incidentally, YYY has observed the reactor where the end of the sheath is shown by the end of the metallic deposit that causes mirroring. To actually prove the point, however, we would have to build a canal ray tube with dimensions and geometry comparable to I&S and study it there. Unfortunately we have no funds for that. Institutional frameworks like those that speak through your review, with their closed-door approach, have made sure of that.

If there are doublets that behave as a molecular stack (as we claim there must be), the collision involved will just be a two body collision, not a three body collision. That is what we have suggested throughout the paper. In terms of the morphology, geometry and volume of the electron and the proton in Aetherometry (both are torus-shaped particles, the proton volume being negligible compared to that of the electron), the capture of a doublet stack of protons, the formation of atomic hydrogen and the subsequent recombination to a singly-ionized hydrogen molecule, are processes that can all happen sequentially while still within some part of the large central cavity of the electron. In fact, according to our view, the reason why an electron can be treated as a probability wave is

not that it is a point arbitrarily located on the orbital ring described by the mathematical distribution of such a probability, but that this distribution is an approximation of the physical reality of a continuous torus (or a stack of such toruses). The so-called point location is just an intercept of a 'vast' toroidal ring (see our monograph LS-25).

As for your criticism of Table 5, it has already been addressed above under the discussion of Table A. You think that our proposed proton velocities are wrong (eg that for a 7780V potential the speed should be "1.22 x 10^6 m/sec !! That is in my phyisics ... "). We have already shown that Table 4 presents traditional values such as yours (your own physics being just conventional physics), some of which we think are in error, and contrasts them to the values which according to our work (Aetherometry) *are the correct ones*. We did that above precisely for protons with a potential of 7780V in Table A.

3.6. On the reactions going on in the I&S vessel

We would like to clarify an objection made by you that originates in a sentence of ours which, unfortunately, is not the clearest, though in light of everything else stated in the paper, should have been properly understood. Your objection is:

"On: Bottom lines at page 8 "we will propose that it is ionized molecular hydrogen H2+, that is formed at the time the Balmer line of interest is emitted ", ONLY stable atomic Hydrogen can emit the Balmer line !?! (later on, I will come back to this point because, as I said before, this is CRUCIAL)"

The passage you quote should have read instead something like: "we will propose that it is ionized molecular hydrogen, H_2^+ (and not H_2), that is formed *once* the Balmer line of interest (H_β) is emitted by an atomic hydrogen (transiently formed out of a doublet)". Let us explain and clarify this by writing the reactions involved in the I&S experiment that belong to *diatomic* molecular hydrogen.

The first problem one is confronted with is the double assumption made by I&S in their paper, that the emission is from an hydrogen atom (the free radical H•) to which the emitter "reverts" (their choice of words). Let's call this reaction A, which we can write with traditional notation as:

REACTION A

$$H\bullet \xrightarrow{H_\beta \text{ emission}} H\bullet$$

The second problem is that I&S do not tell us how the singly-ionized hydrogen atoms H_2^+ which they assume composed the canal rays in the postcathodic space (PCS) were obtained from the ionization of hydrogen gas introduced into the interelectrode space (IES). Putting these terms into the reaction, we are obliged to write as follows - while identifying the unexplained transitions with question marks:

REACTION B

$$H_2 \xrightarrow[\text{IES}]{?} H_2^+ \xrightarrow[\text{PCS}]{?} p + H\bullet \xrightarrow{H_\beta \text{ emission}} H\bullet + p$$

What is wrong with this is that Ives and Stilwell claimed to have found no "H_1 particles" in their vessel! That means neither protons (p), nor atomic hydrogens (H•)! Obviously neither reaction A nor reaction B can actually account for what is going on inside the I&S vessel.

Now, enter Aetherometry:

First, according to the aetherometric argument - which our paper claims to demonstrate - that the kinetic energy of the hydrogen ions which move at the correct (ie measured) speed is *not sufficient* to source the H_β emission without collision with an

energetic electron, we would have, at the very least, to write this last reaction very differently as reaction C:

REACTION C

$$H_2 \xrightarrow[\text{IES}]{?} H_2^+ \xrightarrow[\text{PCS}]{1e^- \text{ input} \rightarrow H_\beta \text{ emission}} 2H\bullet$$

Thus, if I&S were correct about the chemical nature of their diatomic canal rays, the real product - from the aetherometric perspective - would have to be two atomic hydrogens (2H•). Yet, Ives and Stilwell found no atomic hydrogen!

Furthermore, our paper also contends that the kinetic energy of H_2^+ ions is *also not sufficient* to account for the observed speeds of diatomic hydrogen (see page 10: "according to Aetherometry, only protons or proton doublets can be accelerated to the reported velocities with the voltages applied by Ives and Stilwell". See also tables A and B above). Thus we contend that the canal rays are proton doublets, because, according to Aetherometry, (1) the observed speeds of the canal rays can only be those of doublets or singlets, and (2) *no singlets were detected as being present*. Consequently, the reaction proposed by Aetherometry is quite different from reaction C: ionization of hydrogen in the IES generates protons that are stacked by passing through the perforated cathode; these protons form canal ray doublets that ultimately generate singly-ionized diatomic hydrogen by collision with sheath-emitted electrons. The aetherometric reaction discussed in our paper then must be written as reaction D:

REACTION D

$$H_2 \xrightarrow[\text{IES}]{(\text{loss of } 2e^-)} (2p = H_2^{++}) \xrightarrow[\text{PCS}]{1e^- \text{ input} \rightarrow H_\beta \text{ emission}} (H\bullet + p) \longrightarrow H_2^+$$

According to Aetherometry, the final product would be either an atomic hydrogen plus a proton (still in association and thus placed above in parentheses), or a singly-ionized diatomic hydrogen molecule - that is, *as* that quote from page 8 of our paper indicates, "H_2^+ (and not H_2)". Note that the final transition will always be favoured by the fact that formation of molecular hydrogen will release energy from the higher energy states of the free radical and associated proton.

Similar reactions with an identical rationale can be written for the triatomic molecules.

There is, therefore, in none of the above any disagreement with the view proposed in our paper that, upon emission of the Balmer line, an atomic hydrogen is transiently produced. What we suggest happens further after the emission is the formation of a singly ionized diatomic (or triatomic) molecule. Besides, the text of our paper states this much on page 11: "the Balmer line is present upon the formation of *atomic hydrogen*, *be it in the transition of a proton doublet to ionized molecular hydrogen*". Check the last reaction above - it is exactly what the preceding italicized text describes.

3.7. Regarding our Table 6

Your commentary on our Table 6 begins with an immediate indication that you did not understand the reasoning behind this table:

"Now Table 6 : Why compute for frequencies which, by your very assertion, have not been emitted and have, therefore, never been measured ?"

The point of that Table 6 - as we say on page 13 of our text - is to demonstrate that if one considers the velocity of the hydrogen ions and the corresponding kinetic energy, and one applies the aetherometric equations 7, 8 and 9 of our paper, it is apparent to every aetherometrist - or any reader who, not being an aetherometrist, is able to follow

the mathematics and physics of the aetherometric method or argument - that these ions simply do NOT have the kinetic energy aetherometrically required to emit the desired H_β line! Yet the Doppler shift at those comparable velocities (and for much lower frequencies of emission if the charge carriers were decelerating) is substantially the same. That's what that Table 6 demonstrates, FROM THE AETHEROMETRIC VIEWPOINT.

Thus, we calculate instead what the maximum photon *frequencies* would be that such hydrogen ions could emit *as a function of their kinetic energy*, and we compute the corresponding $\Delta\upsilon/\upsilon$ that *would be observed at those frequencies* if they decelerated and became photon emitters.

As for your pseudo-pedagogical tirade regarding the relativistic derivation of the Doppler effect of light, this derivation surely ain't more elegant, simple or cogent than the aetherometric perspective; and instead of taking into account the real 5-dimensional world of energy (and thus grasping the energy limitations that we have pointed out throughout this response - including the realization of the existence of doublets, the dependence of photon emission upon kinetic states, and the determination of the correct speeds of protons!) - it moves in the imaginary terrain of 4-D Spacetime.

The tirade is irrelevant for the contents of that Table 6, which, as the text explains, for its columns 1 to 6, employs the relevant equations of our paper (both non-aetherometirc and aetherometric). Moreover, columns 7 to 9 are computed using SR, as you yourself realized after generating yet another useless table on page 10 of your commentary: "Given the closeness of the values, I conclude that you must have used the same formula". And whether the values *are exactly those* we give with computations employing the values given in section 2 above, *or the insignificantly different values obtained by you*, *once again*, *nothing changes to the conclusions to be taken from our Table 6* and already discussed above: the kinetic energy of such ions would not permit them, *according to Aetherometry*, to source by themselves alone emission of the desired Balmer line.

The last question you make re. our Table 6, is:

"What do you mean by "From theoretical values predicted by SR on computed $\Delta\lambda$ "? The computed $\Delta\lambda$ is $\lambda_o(\gamma(1-\beta)]$, given the velocity one can get the $\Delta\lambda$, but to get the velocity one must know the $\Delta\lambda$, or do you get the velocity from the potential, so what is it?"

The straight answer is: as columns 1, 4 and 7 of said Table 6 *indicate*, the determination *is obviously made from the potential*!

3.8. Regarding our Table 7

You also miss entirely the point of Table 7, which is: with the equations presented in our paper (AND SHOWN AS HEADINGS TO THE TABLE 7 COLUMNS), AND THUS ACCORDING TO AETHEROMETRY, a free electron could only produce *on its own* the Balmer emission line of interest upon deceleration or collision, if it were accelerated by that field voltage (48.9kV) - CORRESPONDING TO THE VELOCITY INDICATED in column 3 - just prior to the onset of deceleration!

All that you had to do was to check the calculations with our equations in that paper and compare them with the conventional determinations to see where and when they agree, and when they don't!

Our perception is that you were unable to grasp the basic algebraic language proposed by the paper, and the basic changes in the physical description of what is happening (you failed to grasp the new articulations of voltage, speed and energy that we propose, the process of production of nonionizing photons in Aetherometry, etc). You get carried away by your supposed detection of an error or inconsistency, when these errors or inconsistencies lie, as we have now shown time and again, either *in your misreadings of what is written* or *in your miscalculations*. Then you punctuate this with perfectly run-of-the-mill preachings in accepted physics, with a condescending pedagogy. Certainly not for our benefit.

We should note that the data generated in Tables 7 and 8 are preparatory for their integration in Tables 9 and 10 that clinch our case for collisional causation of the Balmer emission line. If, by Table 7, the reader is not 'with the program', then he or she may as well forget getting any understanding of what this paper is about.

3.9. Why peer-review today is a detriment to science?

A propos of the fear in mainstream peer-reviewed science journals of considering alternative explanations, you chose to educate us (who have published in such venues often enough for our taste, but never in the domain of basic physics) as to the reason for such conservatism:

"Let me, at this point, tell you something about my views regarding Science in general and Physics in particular. (this is partly motivated by an attempt at answering your complaint about the fact that often people do not make reference to alternative theories, which could equally well explain the given phenomena under consideration) I believe that the progress of Science is primarily based on observation, for me the truth is in FACTS , experiment , that is. The progress comes about because, at a certain moment new findings do not fit the theoretical framework that, up to that moment, had worked well in interpreting a given domain of phenomena,and it was just that theoretical framework, used up to that point, that brought to the formulation of the new experiment which did not fit anymore. Now what happens, as a follow up, is that people scratch their head and try to come up with a new, broader, theoretical framework, which, hopefully, interprets well both the more restricted set of phenomena , as the old one did, but is now valid in the larger domain. Just as a purely illustrative example, but with the intent to make my point clear, let me quote the so-called "Caloric " theory of heat of the early nineteenth century, it could explain well all the the phenomena connected with heat, as long as the considered phenomena did not imply transformation of heat into mechanical work or viceversa , now it would have been nonsensical, after the development of Thermodynamics to expect that, when reporting on work related to thermal properties, if it had happened that the particular situation could also be interpreted within the "Caloric " theory, people would make reference to it."

Your choice of "illustrative example" is funny in its non-aptness. You are picking a case where an older theory was replaced by a newer one with a greater explanatory power, and the punchline of your story is that it would be nonsensical to expect people to keep referencing the older theory when reporting on work in an area covered by the newer one. How is this example relevant to the situation, vis-a-vis mainstream science, of a radically *new alternative* theory such as Aetherometry? Surely the reasons why mainstream science refuses to give a fair hearing to the ideas put forth by Aetherometry are not the same as its cessation to reference theories that were once part of the mainstream and were made obsolete by the mainstream adoption of newer theories. Just consider: if Aetherometry's position within science was like that of the caloric theory, surely you would not see fit to construe Aetherometry as too outlandish for your understanding, would you? So, the question that remains untouched by what you say above is: why ***do*** you – and all the "peer reviewers" we have ever encountered - consider it a point of honor to *not* understand Aetherometry?

What you wrote above is a 'nice, clean presentation', but unfortunately only an idealistic (or idealized) distillate of the real historical development of science - which is inextricably shaped by interactions with economic, political, technological and artistic factors (for a good disabuse of such a linear view of science see the 'old' explorations of James Burke in his series "Connections").

Moreover, even in what concerns science taken in isolation, your presentation is also deeply incorrect. We can only hope that you will read the next chapter (due to be published shortly) of our ongoing "*Whither Science*" in the new journal JSPT. We will be succinct here: science does *not* have a consistent single model shared by all disciplines and fields. In many fields, parallel hypotheses exist. And very frequently, a result not expected by any of these models forces a re-evaluation and re-enunciation of the competing theoretical models *without actually being able to distinguish between them* (see the effect of the Michelson-Morley expt. on classical theories, and the fact that different models besides SR can equally account for the null result of that expt.). In such situations, election of one model over others is simply *a political decision* of the scientific and politico-economic establishments. Today, the key to supporting such a political decision is to engender, at the very least, an image of consensus. The resistances of the established peer-review system are not primarily scientific; not at all. They are primarily political and libidinal. In fact, they are psychiatric for the most part.

Another fact to consider is that frequently, experiments are not final, but simply defer the differenda to a new level of investigation.

This much having been said, we can easily concur with the conservatism of preserving any approach that has proven itself, but not at the cost of understanding and verifying NEW FACTS or developing SIMPLER EXPLANATIONS; or worse still, at the cost of INDISTINCTLY MUZZLING ALL ALTERNATIVE APPROACHES, irrespective of their cogency and experimental evidence. Such conservatism is neither rational nor does it serve science. As we have written in our commentary on Dark Energy, "all too often scientists charge in groups, somewhat like vying pack-dogs, in the general direction of some real and essential phenomenon, either unknown or misconstrued by existing science - and yet, they remain singularly unable to precisely pin it down. Typically, this inability to see clearly into a problem is due to the blinding hold that a previous paradigm is capable of exerting upon a majority of peers. To avoid inertia and ultimately complete paralysis, a few may try to break out of the mold, but just a little. For the general rule is that any attempt to move upstream in the midst of a descending stampede, regardless of how hallucinatory its trajectory is, guarantees professional suicide." (www.aetherometry/Electronic_Publications/Science/dark_energy.php)

Indeed, and your presentation above completely omits *the only thing that matters* with respect to the justification of conservatism: that the *legitimate* objective of the conservative tendency is the *elimination of spurious claims arising from trivial research*. It *cannot* be reasonably applied to pioneering research!!

In this respect, we can only quote you the immortal words of C.D. Darlington:

"Scientific discovery is often carelessly looked upon as the creation of some new knowledge which can be added to the great body of old knowledge. This is true of the strictly trivial discoveries. It is not true of the fundamental discoveries (...) on which scientific advance ultimately depends. These always entail the destruction of or disintegration of old knowledge *before the new can be created*" (C. D. Darlington, Conway Memorial Lecture on *The Conflict of Society and Science*, 1948).

Your parable about science omits that basic FACT that, from time to time, there emerge "all at once" more cogent comprehensive views of a field or an entire set of fields, views which deserve the attention of the scientific community, but, time and again, fail to get it in their own epoch, mostly out of apathy, carelessness, dogmatism and envy, but often enough out of vicious repression. We do not believe we need to cite examples of *peerless* scientists that, in the history of science, suffered this fate...

The only virtue of the present exercise, including your commentary, is that it does well illustrate precisely the problem with peer review: while the reviewer is never paid enough (in fact, generally not paid at all) to read through something that he already, from experience, is *predisposed to suspect* is humbug, he also *has no desire to read and explore out of his scientific curiosity* (if he or she ever had any, it was dulled long ago), *let alone any desire to uphold in practice the ethics of adequate reviewing*. For the desire to learn something new, including its errors, is not separable from making a fair evaluation, which requires a patient and careful reading. Rather, since, in fact, most alternative solutions proposed in any given field have fatal flaws, reviewers *feel unspokenly justified* in throwing out the wheat with the chaff. Since they are defensive about what they were taught, the whole combines into a frequently nasty armored reaction against anything that might be new and different. The result is repression that invokes confused and false reasons.

The honest truth is that reviewers cannot be bothered to read the submissions with the care these should deserve. And as we have experienced and studied (in a book on peer-review that we have been preparing for some five years), it happens that when reviewers do catch something innovative that they at last grasp, they are not beyond failing the submission and flying with the idea, or even the data, as if it were their own!

Yet, the peer-review system is all that our social system has at its disposal to select which research should be funded, and which shouldn't. Thus, aside from their ethical responsibility, journal reviewers have a tremendous, though indirect, responsibility (unlike grant reviewers who have a direct responsibility) in determining the direction and even the outcome of scientific investigation.

It is obvious that with such a bureaucratic and unethical system of so-called peer-review, scientific research would evolve rather slowly, if at all, if it were not for the challenges constantly forced by the development of military machines!

Incidentally, our work would never have happened nor seen the light of the day had we not funded it and published it ourselves, with the help of kind donors and volunteers. In more than one way, we can thank peer-review for this. Also incidentally, the equations we have applied to I&S were obtained from our work, theoretical *and* experimental, in plasma physics and electrodynamics. They were not tailored to explain the real physics of collision in the I&S experiment - a physics which nearly everyone would like to agree is there, but no one has succeeded in articulating. Yet, when our equation system was tested with the I&S data, it made such an articulation possible.

3.10. On the usage of words or terms

It is not without precedent that a professor of humanities or science purports to teach us the use of the english language... Rarely were they the professors who taught us something.

You first make a welcome suggestion:

"Back to your use of words : you seem to attribute to words that usually have a definite meaning, another one, which, you seem to assume, should be obviuos to the reader, but this is not the case, perhaps a little introductory Glossary might be of help."

Yes, if we had time, we'd like to create one, but it does not rank high in our priorities. The basic definitions exist already at www.encyclopedianomadica.org.

However, you next impersonate yet another one of those professors we've encountered all our lives:

"Example: Section 3.2 "The physics of the kinematics ..." , in my dictionary the word 'kinematics' refers to that part of the description of a physical system where one studies the elements that characterize its CONFIGURATION , notion such as degrees of freedom, parameters giving its STATE etc., certainly NO dynamical notions."

Well, there is nothing wrong with the use of the expression "the physics of the kinematics...", irrespective of your dictionary! One spoke in this sense, for instance, of the *physics of statics* in Archimedes's thought versus the *physics of kinematics* in Galileo's, in the sense of a branch of mechanics that addressed the motion of bodies (whereas kinetics became the branch that addressed the effect of forces *in changing* the motion of bodies).

We cannot understand why you would object to the expression, or need a dictionary to figure it out.

Likewise, you comment:

"It transforms according to the law (Lorentz transformation,which does not have anything either ontological or phenomenological, see later re. the use of these terms)"

Well, to paraphrase Sklar, the original Lorentz and Fitzgerald view simply assumed that the material objects constituting the laboratory apparatus shrank when placed in motion with respect to the (luminiferous) aether. That was a view that did not have to chose between ontology (they actually shrank...) and phenomenology (they appeared to shrink...). But Relativity further proposed its own "relationist ontology of spacetime" (see Sklar, L "Space, Time, and Spacetime"). At any rate, your comment was made apropos of a sentence on page 17 of our paper, which read:

"In Aetherometry, the second-order effect of the linear Doppler is a mere phenomenological consequence of the law of the geometric mean composition of velocities, and does not entail any ontological or phenomenological Lorentz-Fitzgerald transformations."

By the very fact that without LF transforms one still gets, by the law of the geometric mean composition of velocities, the same Doppler shift *when no collisions are involved*, the LF transforms are *merely* proven to be *phenomenological* in their validity. They are therefore superfluous in their application.

3.11. On Tables 8, 9 and 10

Effectively, your comments on our paper are sparse, but none sparser, more irrelevant AND more erroneous than the single comment purportedly on Tables 8, 9 and 10. Note that you have *so far managed not to perform a single aetherometric calculation correctly*, and this is no exception: you write:

"My way of computing the energy required to emit the frequency of the Balmer line according to your formula (8) is

1/2 me v0 ee /ev = 24495.3

No 48.983 Kev ? Table 8,9,10 require initiation into the mysteries of Aetherometry ..."

First, the frequency of the Balmer line does not appear or belong anywhere in tables 8 and 9, so the statement is sloppily qualified.

Secondly, your formula quoted above is not only virtually indecipherable in your pdf, but is also NOT an aetherometric formula!

Thirdly, if we write your formula with proper symbols, and look at its dimensionality:

$$0.5\ m_e\ v_0\ e\ /e\ V = (MLT^{-1})(MLT^{-1})/(ML^2T^{-2}) = M$$

we can see that it does not have dimensions of energy!! It is *dimensionally wrong*, and so is neither valid nor sound. The imaginary result (24495) would be in mass units not in units of energy!

Finally, if ee is a symbol you *invented* to designate the value of charge e in Aetherometry (along with the *imaginary value* in coulombs that you claim Aetherometry attributes to charge e!), then you should at least remember that you have changed units, and thus that mass would have to be written in units of length; likewise for voltage!, which would have to be written as a speed.

To proceed as you do from *a wrong equation that is falsely attributed to us*, to the constatation that an initiation to "the mysteries of Aetherometry" is for an understanding of our tables necessary, is not only calculated to be demeaning, but is in fact simply slack, since the equations of our paper are straightforward to resolve and do not require acrobatics nor merit falsification. Why do you persist in whipping up, and attributing to us, false equations and computations? One can only wonder how many decent papers you might have rejected with the same unfair and unfounded treatment...

And what is that incoherent exhultation "No 48.983 kev?"?!

The answer as to how we determined the Balmer line shown CONSISTENTLY in column 11 of Table 10 is simply to apply equations 1, 2 and 8 of our paper!!! All the values necessary are presented in that Table 10, and show that the Balmer line *is a unique signature* because in all the collisions obtained with different hydrogen and electron energies, *the total kinetic energy of the emitter* (LITERALLY, the electron of hydrogen) *immediately prior to emission is the same*, 48,983 eV.

You inarticulately add:

"In the end of the day it is not even clear to me if your contention is that Aetherometry EXPLAINS the Doppler effect or not ,that is that Detected frequency is different from Emitted one."

Well at the end of the day, Aetherometry (1) explains the Doppler effect of light by the law of composition of velocities, (2) applies this to the Ives and Stilwell experiment in a novel and original analysis that is the first one to propose a solidly collisional model, and (3) shows that the detected frequencies are per force different from the emitted ones (but not in the wrong sense we surmise you meant when writing the nonsequitur sentence - "that is that Detected frequency is different from Emitted one" (sic) - which turns your alternative into nonsense) because, AFTER ALL, that is what the Doppler effect is (and everyone agrees at least on this) - the difference in the emitted and received frequencies...!

AND, still at the end of the day, none of the 3 appreciations put forth in the previous paragraph could have been obtained from an average, or even above-average, reviewer of official science, mainstream publications. That is the sad truth.

3.12. On Table 11 and your chi-square technique...

We were perversely amused that you left the best for last. This is the analysis that you purport to conduct on the results presented in Table 11. You initiate your commentary with a salvo intended to ridicule Aetherometry once more, while mixing up three different topics into one ball of fur:

"Coming to the Table 11,leaving aside the fact that your miraculous astronomical conjunction whereby the two protons of the doublet get slowed down by an electron ,the elctron is captured by one of the protons , the atomic Hydrogen atom is formed,the Balmer line is emitted, promptly to be followed by the formation of the Hydrogen molecular ion *H2*+ is really ??, one thing,at least should be clear, but it is'nt ! Before collision there is no atomic hydrogen, so no Balmer line, so why compute ?"

Let's dissociate this paragraph.

First, there is nothing miraculous about a doublet being slowed down. If doublets exist, which indeed we alone claim is the case, then the proton stack behaves as a single body, as if the hydrogen molecule was doubly ionizable.

Secondly, let's consider the next (ungrammatical) part of the sentence where you state: "the atomic Hydrogen atom is formed,the Balmer line is emitted, promptly to be followed by the formation of the Hydrogen molecular ion *H2*+ is really ??,". This has already been amply addressed above in section 2.6. It suffices to look at the aetherometric reaction D to see precisely how atomic hydrogen results from the collision, and how its re-association with the proton of the proposed doublet leads to the formation of H_2^+.

Thirdly, *before collision*, there is neither atomic hydrogen *nor singly ionized molecular hydrogen*!

Fourthly, the point of the comparison is to demonstrate that if the Balmer emission was sourced solely by the kinetic energy of the ions before collision (which Aetherometry claims is not the case), then the observed Doppler shifts would be very close indeed to the shifts predicted by SR for the actual emission - and thus as far from the actual or observed I&S results as the SR prediction is! That Table 11 demonstrates therefore that, to get to predictions (such as those of Aetherometry) that are closer to the observed results, it is necessary to assume that collision with electrons MUST BE OCCURRING, and their energy contribution MUST BE TAKEN INTO ACCOUNT. That is said, in so many words, by the commentary on that Table, on page 17 of our paper.

Now we get into the best part. You purport to perform a chi-square test to the data of Table 11. The chi-square test is a measure of the "goodness of fit" of the observed data to the predicted values. The typical chi-square is supposed to determine whether a significant difference exists between an observed number of cases (f_o) in different categories and the expected values (f_e) in each category. The formula is:

$$chi^2 = \sum [(f_o - f_e)^2/f_e] \qquad (L)$$

This chi-square formula cannot be used to compare the values predicted by AToS or by SR against the values observed by I&S, because those values are not counts or frequencies.

There are other chi-square tests. Specifically, in physics, one of those tests is used to determine whether a statistically significant difference exists between observed results in different categories (viz at different applied voltages) and the corresponding expected values as predicted by a specific theory (e.g. SR, or AToS, etc). The relevant formula is -

$$chi^2 = \sum [(x_i - \mu_i)^2/\sigma_i^2] \qquad (M)$$

where it is assumed that the observed values x_i are each taken from a normally distributed data set, that the values _$_i$ are the theoretically predicted values, and _$_i$ is the standard deviation of the i-th distribution of observed values.

The basic idea of the test is that each of the terms in the sum of equation M measures the distance (or more precisely, the square of the distance), expressed in standard deviations of the experimental set, between the representative experimental value

and the corresponding theoretical value. If the theory "fits" the experimental values, these distances should typically be less than 1 standard deviation, and the whole sum should therefore be less than the number of terms it involves. The actual value of the sum can therefore be used to measure the degree of confidence one can attach to the hypothesis that the theory "fits" the experimental data.

However, could we apply equation M to compare either SR or AToS with the I&S data? If one wanted to employ an actual chi-square test in our case (ie in the case of Table 11), the overwhelming obstacle to doing so is the impossibility of estimating the standard deviations – since each of the four separate observables comes from a sample of size 1, because Ives and Stilwell did not publish the entirety of their data, nor any statistics. There is no way one can estimate the mean and standard deviation of a single datum in a sample of size 1. Consequently, one *cannot* in such a case apply the chi-square test of equation M.

Thus, this begs the question of what you were doing when you claim to "define a χ^2" and apply it to Table 11?

Well, the simple truth is that what you were computing has *nothing to do* with the chi-square statistic, even though you grandiosely announce "When I want to compare experimental data I usually define a χ^2 and go by it". In actuality, what you compute is

$$\text{chi}^2_{\text{Prof.X}} = \sum \{(x_i - \mu_i)^2/[(x_i + \mu_i)/2]^2\} \qquad \text{(N)}$$

or, to use the symbolic notation that you employ on page 11 of your commentary (where you forget a closing parenthesis):

$$\text{chi}^2_{\text{Prof.X}} = \sum ((x - y)/((x + y)/2))^2 \qquad \text{(O)}$$

This is not a chi-square statistic, but simply the sum of the (squared) *relativized differences* between two sets of data: each term of the sum calculates the difference between two corresponding data points, relativized to (that is, as a fraction of) the average value of these two data points (and then squares this fraction). In other words, each term is the square of the Diff function that you employed earlier in your commentary.

As shown by your formula on that same page 11 of your commentary, which reads:

chi[a1, 61, a2, 62, a3, 63, a4, 64, a5, 65] = Sqrt[1/5(Scarto1[a1, 61] + Scarto1[a2, 62] + Scarto1[a3, 63] + Scarto1[a4, 64] + Scarto1[a5, 65])]

after computing the sum of equation O above, you divide it by the number of terms – thus obtaining *the average of all the squared relativized differences* – and then take the square root, thus obtaining (more or less) the *average relativized difference* between the two data sets. So the complete formula for the "distance parameter" that you use to estimate how close two data sets are to each other is

$$\text{AvgRelDiff} = \sqrt{\frac{1}{n}\sum \frac{(x_i - \mu_i)^2}{\left(\frac{x_i + \mu_i}{2}\right)^2}} \qquad \text{(P)}$$

You then calculate this parameter for four pairs of data sets:

(1) to "compare" the values predicted by SR with the pre-collision, and also the at-emission, values predicted by AToS; and

(2) to "compare" the experimental values observed by I&S with the values predicted by SR, and also with the at-emission values predicted by AToS.

We simply have no idea to what end you perform the computations of the first comparison - that is, those computing the pairwise distances between the theoretical predictions of SR and AToS. Maybe you are just doing warmups for the computations of the second comparison... It should be obvious why such a procedure is totally meaningless.

As for that second comparison (the only one that could be of import), we are assuming, in what follows, that you regard AvgRelDiff (equation P) as some overall, seat-of-the-pants measure of "goodness of fit".

Well, if you are performing the calculations of the first comparison as a warmup exercise, it didn't help you much – because when you compute the values of the *second comparison* you make a number of mistakes.

First of all, there are only four experimental data points, but when you average the (squared) distances, you divide by 5 (i.e. you use n=5, instead of n=4, in the AvgRelDiff formula).

Second, even assuming n=5, when you calculate AvgRelDiff between AToS and the experimental data, you get the value wrong! You get 0.04797, whereas the correct result (using you own formula) is 0.018073401!

Third, when you then calculate the relative difference between these two AvgRelDiff values, you get that wrong too: assuming your own two incorrect results, 0.04918 and 0.04797, the correct relative difference between them is 0.0249099 (and not 0.000039, as you exhultantly exclaim at the end of your computation)!

Now, we ourselves have no possible idea of what scientific value AvgRelDiff is and what conclusions one may *legitimately* draw from it. Our minds are even boggled by the notion implicit in equation O above, that one should employ *an average of the observed and the predicted* in the denominator: on what basis?

But since you seem to attach a value to AvgRelDiff (equation P), let's get your computations straight. Using the correct n=4, the correct values for the parameters you compute (on your page 12) are as follows:

AvgRelDiff for SR vs observed = 0.054993619
AvgRelDiff for AtoS vs observed = 0.020206677
Relative diff. between the two = 0.925184 = 92.5% !!

We have put two exclamation points after the relative diff. calculation because you did the same after yours ("Notice that the percentage difference between the last two comparisons is Dif f = 0.000039!!").

But really now, what could POSSIBLY be the value of those AvgRelDiffs and of their relative difference? You're proceeding as if it was, in fact, part of the scientific method to ditch one theory in favor of another on the basis of a comparison between their respective AvgRelDiffs towards some set of experimental data. Well, if this is part of your scientific method then you should, in fact, ditch SR in favor of AToS – after all, when properly computed, that precious relative diff between the two AvgRelDiffs is 92.5% in favor of AToS!

It is somewhat comforting that, by your own so-called chi-square test - and your interpretation of the same - AToS would appear, after all, to have a significantly more accurate predictive ability with regards to the Ives and Stilwell experiment than does SR. Too bad this, too, escaped your notice.

3.13. Your closing comments on our paper

Despite the fact that you claimed to us that our letter of June 26 was "very useful", it seems that it went by completely uselessly. There we stated:

"We would like to draw your attention to two facts. The first is that, as you may well know (for instance, from the French lectures), confirmation of a second order shift cannot be construed as unequivocal proof of SR's correctness, because the second-order shift can be arrived at independently (with no invocation of time-dilation) by the consistent application of the law of the geometric composition of velocities. In fact, it is this coincidence which separates SR from LLR - whose formula for "time-dilation" does not yield a result identical to the law of composition of velocities. (...) Aetherometry does not question the correctness of the second order shift derived from the law of velocity composition. On the contrary, Aetherometry fully agrees that the observed shift complies with the second order approach (and that the result of the Michelson-Morley experiment is and should be null)."

And yet you write:

"Finally, in Table 12, the best of the best discovery : Aetherometry and Special Relativity give the same expression for the effect: at this point I really give up."

You clearly fail to realize that the expression (in note 4 of Table 12) is applied by SR solely to a *collisionless* process, and is differently applied by AToS to both collisionless and collisional processes. Thus, the predictions of $\Delta\lambda$ in Table 12 ARE NOT THE SAME for AToS and SR, as is obvious from Tables 10 and 11, because AToS already takes into account the collisions involved in the equations that are written in the text of the paper! Thus the predictions regarding the Doppler shift DO DIFFER.

We believe that this exercise, despite everything, will not be in vain, even for ourselves. It has confirmed, in our minds, that careful review is virtually always a pipe-dream. The reviewer is deemed to be objective, but this deeming is itself an irrational supposition or belief. Based on all the errors in interpretation, algebra, statistics, programming and computing that you committed and that we patiently, if not painfully, dissected in these pages, you concluded:

"Summarizing it seems to me that no referee of any Journal or Magazine dealing in either Theoretical or Experimental physics would accept for publication your paper, and this for two main reasons :

1) The paper is essentially based on a question mark theory.

2) Even if everything in the paper had been clear and acceptable, considering the very marginal improvement in the agreement between experimental and theoretical values, with respect to other theories, regarding data coming from an experiment carried out 70 years ago,using old technologies, the paper would be considered not to have any scientific interest."

You clearly concur with our prediction that our paper could never be published *after review* in any mainstream peer-reviewed publication. But your reasons are the wrong ones. Every theory is a question mark that responds to previous question marks and raises new ones. If only that which does not disturb accepted models was worthy of publication, then science would ossify beyond any usefulness - which actually pretty much describes what is already and effectively happening to a significant degree in mainstream scientific publications. The essence of science is questioning! No questioning, no science.

Realizing the enormity of what you just said, you later add: "At any rate, if I had any say in the matter, I would consider reason number 2 alright but not reason number 1, I believe that prohibitionism in science is not to be accepted,in fact, good ideas will triumph and bad ones will fade away"...

But what you don't seem to acknowledge is that "prohibitionism" operates in many different ways and on many levels, and your carefree and disdaining attitude towards our paper is precisely one of the ways in which it operates. Rejecting our paper off hand with wrong equations and wrong computations surely should not be the way to defend the pristine edifice of science that deserves to be conserved...

The real reason why our paper could never be published *after review* in any mainstream peer-reviewed publication is simply that reviewers are not objective, not even objective on average, and thus the review cannot be objective either. In fact, they are so biased, so un-objective, that there is no desire to actually review the submission but only to exorcise it from further thought or consideration.

These pages of our response are one long proof - with the i's dotted, the t's crossed and the numbers wrenched - of this simple fact.

Based on all the errors that you committed, you then make a firm declaration of your close-mindedness:

"Perhaps if you could have analyzed Aetherometrically all the subsequent experiments, and still come out better on the agreement , you might have aroused some curiosity, not mine, however, because of the complete disagreement on the premises."

Irrespective of what those other experiments that you wanted us to analyze might be (??), we find it remarkable that your curiosity to understand our thought is nil. Yet, if you were a true scientist, would you have arrived at such hard and fast judgements based on so many errors in your understanding and methods? Should you not have backtracked, asked questions, rethought, exercised a greater caution in you review? Or were you so emboldened by apriori certainty that our paper and our work were merely the rantings of

poor souls adrift in the world, that you could afford to appear to be hard and fast, final and severe in your pronunciations, and be fatally in error, time and again, while being indifferent to being exposed for it?

Yes, it is remarkable. And to dissipate any doubt in our minds about the worthlessness of Aetherometry, you write:

"I can assure you, if it had not been my friend YYY to ask me to review your paper, and I care for YYY's friendship, I would not have spent a minute on it, instead I ended up spending hours!"

"Not a minute!" We were tempted to be angered by this, but give you credit for the honesty in stating your position - which, incidentally, further underscores our contention about reviewers' apriori belligerent lack of interest and desire. We confess that we were intent on hearing and responding to your comments, irrespective of our friendship for YYY, as would have also been the case if you had been a reviewer for a journal considering our submission. But after reading your comments and realizing how many gratuitous objections devoid of foundation or effective rationale you raised and, above all, your obvious and avowed total lack of interest in reading and understanding our paper, we reasoned that there was no point in responding to you. We uphold this reasoning - even if, for the sake of the record, we have now responded. We submit that in our view your lack of interest hardly befits a scientific stance.

"YYY often tells me that I should get rid of my (scientific) eye glasses and wear yours, I tried, but,if , with my own, I can see, not completely, not everything, but some substantial something, clearly, when I try yours I become blind."

On this, also, you are totally wrong. You did not try *our* eye glasses! You tried some "glasses" that you yourself concocted and mocked as being ours, hardly a fair appreciation of our 'glasses' or 'eyesight'. Little wonder you were flying blind!

Paulo Correa, MSc, PhD
Alexandra Correa, HBA

December 26th, 2008

APPENDIX A

June 26, 2008

Dear Professor X -
YYY forwarded us your email below, and we thought that maybe we could help out - keeping in mind that you're working hard to understand Aetherometry. We've been aware of Guido Saathoff's work since his dissertation in 2002, and of the current struggle to improve resolution in confirming the second order Doppler effect. This work stands in a long line of post-I&S experiments - with maser, laser and ring-storage variants - that steadily increased confidence in the correctness of the effect. However, Rheinhardt et al are patently incorrect when they state that "the original Ives-Stilwell experiment confirmed time-dilation as predicted by SR to about 1%" (p. 2). It suffices to look at our Fig. 2, to see that this was not the case; neither was it a claim that Ives and Stilwell made in their paper. If it were the case, our results would be worse than those of SR, and there would be no point to our paper.

Furthermore, we would like to draw your attention to two facts. The first is that, as you may well know (for instance, from the French lectures), confirmation of a second order shift cannot be construed as unequivocal proof of SR's correctness, because the second-order shift can be arrived at independently (with no invocation of time-dilation) by the consistent application of the law of the geometric composition of velocities. In fact, it is this coincidence which separates SR from LLR - whose formula for "time-dilation" does not yield a result identical to the law of composition of velocities. Unfortunately, those Max Planck Institute experiments still lack sufficient resolution to rule LLR out and SR in. This is another criticism that can be addressed to the Rheinhardt et al paper that you

forwarded us - the fact that it fails to mention this at all (and yet it mentions such "faddist idiocies" as Camelia's).

All of this, however, is beside the point of our paper. Aetherometry does not question the correctness of the second order shift derived from the law of velocity composition. On the contrary, Aetherometry fully agrees that the observed shift complies with the second order approach (and that the result of the Michelson-Morley experiment is and should be null). In fact, when Rheinhardt et al use the term "ether-independent relativistic Doppler formula" there is an excision or elision being performed on both sides of the history of science (something no peer review really should have allowed) - since the formula is only independent of the "stationary aether theories" (in contrast to dynamic aether theories, such as Reich's or Aspden's, which predict a null result for the Michelson-Morley experiment), and is not the only relativistic formula that is compatible with the obtained results. Indeed, Ives' own preference, LLR, has not yet been ruled out by these modern experiments. In fact, it is an abuse of science (likely an abuse of power) to claim otherwise, given that all one can say is that, among relativistic theories, only SR has a theory of time-dilation compatible with the law of the geometric composition of velocities. But the reverse is not true: the law of composition of velocities is not a proof of special relativity, nor can it be construed as such a proof, given that other theories - Aetherometry, for instance - require such a law without any recourse, physical or mathematical, to the phenomenologically coincident results of SR's theory of "time-dilation. This, of course, with the proviso that no substantial collisions be at work.

This bring us back to our paper, or to the second fact that might be of help to you: the advances made since the 1960's in perfecting the accuracy of resolution of the second order shift might have been all that mattered if - as Ives and Stilwell were themselves inclined to think, but without any proof - there was no substantial plasma collision (ie collision of an electrical nature) at work in their experiment. They explicitly recognized this in their paper. Precisely, our own paper proves that such directed collisions were at work in that still standing, un-reproduced experiment (as opposed to the minimized inertial collisions in the ring-storage experiments), and the point of the exercise is not to dispute the accuracy or the prediction of the second order shift coadunate with the law of composition of velocities, but rather to demonstrate that it is its consistent application that alone permits the correct prediction when electric collisions are present or dominant. That is the breach (logical, mathematical and physical) which, we submit, should justify consideration and publication of this paper, especially when all velocity determinations were carried out with no invocation of LF transforms and the results are substantially closer to those of the I&S experiment, than are either SR's or LLR's.

We sincerely hope that this will be of help to you, in locating the context of our paper.

Best regards,

Paulo & Alexandra

APPENDIX B:
Excerpt from AS3-II.10, section 3.3

The old newtonian theory considered kinetic energy of a body or particle to be given by (see "theorem of the *vis viva*" [9]):

$$E_k = 0.5\ mv^2$$

SR, instead, proposes that the kinetic energy of a "material point" is a function of its rest mass-energy, mc^2, so that -

$$E_k = mc^2/[1 - (v^2/c^2)]^{0.5}$$

This equation becomes infinitized as the speed v approaches the lightspeed c. Thus, Einstein concluded, speed v must always remain inferior to that of c, with the polynomial series expression being

$$E_k = mc^2 + 0.5\ m\ v^2 + (3/8)m\ (v^2/c^2) + ...$$

It is apparent that classical mechanics only considered the second term, and when only v is taken into account - claims Einstein - there is no need to take mc^2 into account. However, when transforming the reference of a movement of "uniform translation" from one inertial frame to another - ie, when referencing a uniform or "nonaccelerated" movement between galilean systems of coordinates - the Lorentz transformation must be invoked (by SR, that is).

By applying these premises to Maxwell's electrodynamics, a "body animated [moved] with speed v upon absorption of radiative energy E_0 - and moving while keeping that speed constant - will experience an addition of energy equal to" [28]:

$$E_0/[1 - (v^2/c^2)]^{0.5}$$

"Thus" - continues Einstein - " the kinetic energy of that body is given (...) by" [28]:

$$E_k = [m + (E_0/c^2)]c^2/[1 - (v^2/c^2)]^{0.5}$$

This formula, as per de Broglie's own relativistic theory of "Matter-Waves", corresponds, in reality, to what de Broglie called the total energy, E_T, which encompassed both the rest mass-energy of the object and the kinetic energy proper acquired from the radiant field:

$$E_T = [\underbrace{(mc^2)}_{E_{\delta n}} + \underbrace{(E_0/c^2)c^2}_{E_k}]/[1 - (v^2/c^2)]^{0.5}$$

The formula suffers, of course, from the notion that the field energy E_0 is directly assimilable to photon energy in the form $h\upsilon$. It isn't. In electrodynamics, E_0 is precisely the maximum modal energy of the accelerating electric field, not the energy of the electromagnetic fields that result from given *decelerations* of the charges that previously were *accelerated* by the electric field. Notwithstanding this, the above expression contains far worse errors still.

First of all, the kinetic energy acquired from a field may be E_0 or less, far less, with varying distributions depending on many other factors. Secondly, the kinetic energy term is not reducible to some mass times c^2, as if it were just a variant of mass-energy; indeed, to say -

$$E_k = (E_0/c^2) * c^2 = m'c^2$$

is to utter some sort of a pleonasm where $E_k = E_0$, while coupling it to an arbitrary mass-energy relation, $m'c^2$, where m' is an imaginary quantity, not an empirical one. As we shall see ahead, the rest mass of each particle under acceleration does matter both for purposes of mass-conservation (and thus inertia) and for purposes of defining the disproportionation curve between the input field energy and the effective velocity of that massbound particle.

Thirdly, in no way does v, that defining function of what is kinetic, appear in this relation for E_k. Fourthly, there is the matter of the Lorentz transformation term. All these errors combine to arrive at the postulated increase of mass -

$$m + m'$$

that will supposedly happen to a body as a function of its increasing velocity v. Thus Einstein proposed that the inertial mass of a body was not constant, but varied in proportion to the energy affected to the body. Mass, inertial mass, would be a measure of the total energy of a body.

To us, this all seems like just a way to misunderstand the nature of "things". In fact, we have suggested in the next volume of AToS that the real challenge lies in understanding what happens when a particle accelerated to near luminal speeds tries to conserve its rest mass and thus presents inertia. As we also showed elsewhere [29], up to a particular value of kinetic energy, there is a proportional relation between speed and energy acquired from a field. But, just as it also happens in Relativity theory [29], when the field energy approaches a significant ratio (near 1 but <1) between 'kinetic energy' and rest energy, a growing asymptotic disproportionation will begin to be observed. Now, we wrote 'kinetic energy' in quotes because what relativists and particle physicists call kinetic energy is not really the kinetic energy of the particle (E_k) but the input field energy (E_{in}), also measured calorimetrically. The field energy grows disproportionately larger, precisely as the real increment in kinetic energy (disproportionately) asymptotically decreases.

We must therefore differentiate field energy from kinetic energy: up to the moment that field energy 'equals' mass-energy, there is no decreasing proportion to the equivalence of modal kinetic energy with the field energy. Since the field energy is electric, not photonic (not E_0 in Einstein's terminology), we should like to write it as $E_{\varepsilon in}$ to signify it is massfree ambipolar energy that is input locally as a field. So, up until a threshold (which in conventional physics would be called near-relativistic) where the ratio v/c becomes substantial (ca 0.85c), we can write

$$E_k = E_{\varepsilon in}$$

For a given particle with a given rest energy, or mass-energy, there is thus a threshold, a "vacuum-singularity" if you will, that occurs at a particular value of an accelerating field, when the input energy equals the mass-energy. However, this input energy is split by the particle into field energy that it absorbs as real kinetic energy (E_k) and field energy that it "refuses" to absorb, that it deflects or reflects ($E_{\alpha R}$). When $v/c \approx 0.85$, this portion ($E_{\alpha R}$) is ~38.5% of E_k, or ~28% of $E_{\varepsilon in}$, with E_k being ~72% of $E_{\varepsilon in}$.

What is *first wrong* with Relativity, in this respect, is that *it mistook the value of the accelerating field for the value of the kinetic energy of the particle*. Then, that it misunderstood the field as electromagnetic energy. And lastly, that it invoked the Lorentz transformation to obtain the near-luminal speeds of particles, when the real (aetherometric) function for the disporportionation between input field energy and effective kinetic energy invokes no such transformation and a different equation - as we shall examine in detail in the next volume of AToS. In fact, this different equation invokes a function with a spread of values around the "lawful" value of the reciprocal fine structure constant, α^{-1}, which are strict functions of its square root * 10^{-1}, ie the aetherometric η constant. For any given

rest energy/vacuum resonance coupling, the same set of equations apply. In summary form:

The input field energy is -

$$E_{in} = E_k + E_R$$

and typically of ambipolar form, E_ε; E_k is the real kinetic energy of the particle, and E_R (or $E_{\alpha R}$), the residual massfree 'field energy' whose absorption is resisted by the particle. E_R is neither electromagnetic nor electric. The total energy of a particle is defined as the sum of its rest (mass-)energy and kinetic energy terms:

$$E_T = E_{\delta n} + E_k$$

The near-luminal disproportionation occurs between $E_{\varepsilon in}$ and E_k, or between $E_{\alpha R}$ and $E_{\varepsilon in}$. The disproportionation ratio can be expressed as:

$$\Upsilon = (E_{\varepsilon in}/E_{\delta e})^{-1} = [(E_k + E_{\alpha R})/E_{\delta e}]^{-1} = \{[(E_T - E_{\delta e})/E_{\delta e}] + (E_{\alpha R}/E_{\delta e})\}^{-1}$$

For each particular massbound particle, at different values of $E_{\delta n}$, the linear velocity function is a function of β, in turn defined as a function of Υ:

$$v = \beta\, c = c/(\eta\, 10^{-2})^{\Upsilon}$$

so that

$$\beta = [(\eta\, 10^{-2})^{\Upsilon}]^{-1}$$

This results in the same curve being transposed at different energy levels.

The mass-energy being defined as -

$$E_{\delta n} = m_n c^2 = \int = \lambda_n c^2$$

the kinetic energy becomes an effective function of the mass-energy:

$$E_k = E_{\delta n}\, \beta^2 = (\lambda_n c^2)/[(\eta\, 10^{-2})^{\Upsilon}]^2$$

while E_{in} becomes measured in mass-energy units:

$$E_{\varepsilon in} = \Upsilon^{-1}\, E_{\delta n}$$

The residual field is then given by

$$E_{\alpha R} = E_{\varepsilon in} - E_k = (E_{\varepsilon in} + E_{\delta n}) - E_T = (\Upsilon^{-1}\, E_{\delta n}) - (E_{\delta n}\, \beta^2)$$

For the electron, the effective kinetic energy E_k is at all times:

$$v = \beta\, c = \sqrt{(W_k\, W_v)} = c/[(\eta * 10^{-2})^{\,(E_{\delta e}/E_{\varepsilon in})}]$$

The general formula being

$$v = \beta\, c = \sqrt{(W_1\, W_v)} = c/[(\eta * 10^{-2})^{\,(E_{\delta n}/E_{\varepsilon in})}]$$

where W_1 is the magnetic wave function of the given massbound particle.