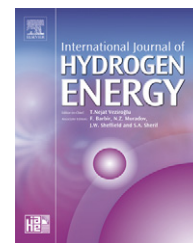


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Discussion

Rebuttal of J.M. Calo's comments on R.M. Santilli's HHO paper

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ARTICLE INFO

1. Introduction

It has been brought to my attention that J.M. Calo published critical comments [IJHE, vol. 32, 2006, pp. 1309–1312] on R.M. Santilli's HHO paper [1]. This note is intended to show that Calo's comments have no scientific value due to the lack of prior re-runs of basic measurements, limited knowledge of the technical literature in the field, the addressing of a draft with evident mix-up caused by format conversion, and other reasons. The author would like to express his appreciation to the IJHE, because exchanges of this type are very important for the serious search of basic advances.

2. The substantive scientific issues not addressed by Calo

Quite properly, Santilli states in the Introduction (bottom left of page 1114) "Independent verifications by interested chemists of the various measurements reported in this paper are solicited." Calo carefully avoids addressing this central scientific need and enters instead into various criticisms of peripheral character, such as misprints, mix-up of symbols caused by format conversion, and other comments addressed in the next section, thus disqualifying ab initio his criticism. Criticisms of Santilli's HHO paper are indeed welcome, but, to have a scientific value, they should be based on the re-run of at least the following new measurements:

(1) A first basic novelty of the HHO gas vs. a stoichiometric mixture of $2/3\text{H}_2$ and $1/3\text{O}_2$, qualified as "apparent" by

Santilli because the need of independent verification, is that the HHO gas instantly melts a tungsten bar or a brick, while said stoichiometric mixture does not, as I can personally testify. The reason is given by the data recalled by Calo, namely, that the combustion flame temperature of hydrogen in oxygen is quite smaller than that needed for melting a brick. This is the first comparative measurement Santilli recommended for independent verification.

(2) Under the pains and penalties of perjury, the Director of the Adsorption Research Laboratory of Toledo, Ohio, released a signed statement (copy mailed by Santilli to the IJHE editor) stating that GC-MS scans indicate the HHO gas to be different than a stoichiometric mixture of H_2 and O_2 , with anomalous species appearing in the range of 15–18 amu and over the O_2 peak (see Santilli's Figs. 1 and 2). It is evident that no conclusion on Santilli's HHO paper can be drawn until these needed measurements are confirmed or denied.

(3) Also under the pains and penalties of perjury, the Director of the PdMA Laboratory in Tampa, Florida, released a signed statement, also mailed in copy to the IJHE editor, according to which IRD measurements on the same sample of the HHO gas used in Tests (2), confirmed the GC-MS detection of various anomalous species (Santilli's Figs. 2 and 3, as mere samples of numerous scans). It seems obvious to scientists in good faith that these sworn results cannot be denied without independent re-runs.

(4) Also under the pains and penalties of perjury, the Directors of additional independent laboratories, such as Toxic LTD Laboratories of Los Angeles, California, released independent confirmations of the presence of a large number

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of anomalous species in the HHO gas confirming the GC-MS and, independently, the IRD scans (Santilli's Figs. 8–11 merely illustrating a large number of scans). The dismissal of “anomalous” species, that is, species outside the chemistry of the 20th century, cannot be dismissed with nomenclatures and personal beliefs without a re-run of the measurements, because there is too much at stake for the environment to assume nonscientific a priori positions.

(5) Again under the pains and penalties of perjury, the Director of a major analytic laboratory, Southwest Research Institute in Texas, released a signed statement of anomalous adhesion of the HHO gas to ordinary diesel, namely, an adhesion that cannot be quantitatively (i.e., with formulae) explained via the chemistry of the 20th century. Since the HHO gas is made up of hydrogen and oxygen, such anomalous adhesion has evident major ecological possibilities of cleaning fossil fuel combustion, as clearly stated by Santilli in his paper. In fact, this feature alone has stimulated serious interest by major petroleum companies, the HHO gas having already seen millions of dollars of industrial investments and being in regular production and sale in the USA, Europe and other countries. Clearly, an “anomaly” with such vast industrial and ecological implications cannot be dismissed with nomenclatures, i.e., without actual counter-measurements.

As indicated above, and as stated by Santilli, samples of the HHO gas are readily available. Calo is affiliated with a qualified college (Brown University) expected to have all the necessary equipment to conduct the needed re-runs before venturing judgments. Hence, the main issue here addressed is the following: why did Calo elect to avoid Santilli's call to re-run the measurements of “anomalous” melting, chemical composition and adhesion of the HHO gas to such an extent of opposing clear environmental needs and the interests of the powerful petroleum industry?

3. Lack of scientific value of Calo's comments

Calo states in the Introduction that Santilli “does not have an elementary grasp of the analytic techniques that were used and/or the proper interpretation of the resultant data.” Quite humbly, Santilli implicitly admits these criticisms, because he is not a chemist but an applied mathematician (formerly from the Department of Mathematics at Harvard University under five DOE grants to develop the theory so opposed by Calo). In fact, Santilli has been nominated by the Estonian Academy of Sciences among the most illustrious applied mathematicians of all times, jointly with Newton, Weyl, Hamilton, Lie, etc., and is the recipient of many other honors as anybody can see by inspecting his CV available from google.com. As a matter of fact, Santilli insisted in the re-runs by “interested chemists,” because he is not a chemist. The point disqualifying Calo's comment is that Santilli merely reported signed statements by directors of laboratories. Hence, the above quoted statement by Calo is de facto referred to laboratory directors who may feel justly offended by it.

The initial paragraphs of Calo's “Specific comments” are devoted to an epistemological discussion essentially based on Calo's seemingly studious misinterpretation of Santilli's use

of the word “evaporation” compared to “electrolytic separation” and other issues so manifestly inessential for the real scientific issues, the “anomalies,” that I feel demeaned to comment on them.

The second paragraph of the second column of page 1310 confirms the lack of scientific value of Calo's “comments.” In fact, Calo correctly quotes 3080K flame temperature for hydrogen combustion in oxygen, but graduate students in chemistry at Brown University are expected to know that such flame temperature cannot instantaneously melt tungsten and bricks.

Then, Calo enters into detailed criticisms on the main features of the electrolyzer producing the HHO gas. However, Santilli has never shown how it works, because of patents pending, and he stresses that the objectives of the paper is a study of the HHO gas and not of the way it is produced. How can any serious scholar think that, under these premises, Calo's criticism of the HHO electrolyzer could have any scientific value for the issues here at stake?

Subsequently, in page 1311, Calo dwells at length in a clear misprint in the HHO paper, $(2/3)2 + (1/3)32 = 11.3 \text{ g/mol}$ (rather than the obvious value of 12 g/mol). In reality, due to the huge delay in the publication of the HHO paper following its acceptance (delay of about three years), readers should know that IJHE ended up publishing an old draft, rather than the final version, as eyewitnesses at the Institute for Basic Research in Florida can testify. The publication of a draft, rather than the final paper, is confirmed by various mix-ups in the printed version, such as HBN_2 and others. I am shocked to see that a person expected to be a serious scholar at an expectedly serious college could possibly dedicate about one full page of criticisms of evident misprints and clear mix-ups caused by Elsevier format conversion.

The subsequent statements by Calo on the difference between “gas chromatograph” and “gas chromatographer” is truly naïve. The use of scientific space for this type of discussions while ignoring the real issues, re-run of the measurements, is so off-balance to raise serious issues of scientific accountability.

At the bottom l.h.s. of page 1311, in reference to a species with 33 amu, Dr. Calo states “How could any chemical species be of nonmolecular nature? What does this term even mean?” A “true scientist” is a scholar who studies in detail the quoted literature in the field before venturing judgments, but he candidly admits to be completely ignorant of the vast literature underlying the novel “nonmolecular” species herein addressed; literature now estimated to have surpassed the mark of 15,000 pages of post Ph.D. Research published in refereed journals the world over. The new species is today known as Santilli magnecules; the new mathematics specifically discovered by Santilli for its treatment is known as Santilli iso-, geno-, and hyper-mathematics; the new physics is today known as hadronic mechanics; and the resulting new chemistry is today known as hadronic chemistry (see the references quoted below and the various independent monographs carrying Santilli's name in the title). While academicians are agonizing under the shock of novelty, Santilli magnecules are seeing multi-million dollar investments from the industry. I feel repugnance to even consider a review of these advances since they require serious study of Santilli's

new numbers [3], new differential calculus and functional analysis [4], new irreversible formulations [5], new physics and chemistry [6,7], and quoted literature.

Calo then attempts to reduce all anomalous species to century old molecular structures. As one example, he claims that the 33 amu species in the HHO gas is the “ion” HO_{2+} , when the absence in HHO of any charged species in macroscopic percentages has been proved (not by the applied mathematician Santilli, but) by professional directors of analytic laboratories. At any rate, assuming that his nomenclature is correct in that instance, what happens to all of the other anomalous species detected in the gas? By using his argument, should we say that the species at 5 amu is the “molecule H_5 ”? Also, quite scientifically, Santilli presents his theoretical interpretation of the anomalous species as “tentative” beginning with the title of Section 3 of Ref. [1].

The new chemical species of Santilli heavy hydrogen is now internationally denoted with the new chemical symbol MH [2]. I am hereby referring to additional statements, signed by laboratory directors under the penalty of perjury, refer to the existence of a new species with specific weight of 15.06 amu, namely, over seven times the specific weight 2.016 amu of H_2 , while being 99.2% hydrogen.

Chemists should be reminded that the US Military decided in the 1970s to terminate funding of academic research. As reported by Santilli in his writings [4,5] (he became a US citizen following research for the Air Force), an officer of the USAFOSR motivated the decision as needed “to prevent the security of the United States of America becoming hostage to ‘pet theories’ preferred by academia.”

4. The serious insufficiencies of quantum chemistry

The huge limitations, insufficiencies and sheer inconsistencies of quantum chemistry, have been denounced by Santilli in his seminal monographs on the covering hadronic chemistry [6,7] and are now known the world over, such as

(1) The notion of valence bond underlying Calo’s main comments is a pure nomenclature without serious scientific (i.e., quantitative) content because, as thought at Brown University and other colleges, the current notion of valence: (1) Does not identify quantitatively, that is, with formulae, the physical or chemical origin of the electron valence bond; (2) does not prove quantitatively that such a bond is indeed attractive; and (3) does not prove that the attractive force represents experimental data. At any rate, identical electrons repel and definitely do not attract each other according to quantum mechanics. Far from any claim of final character, hadronic chemistry has indeed provided a scientific, that is, quantitative, resolution of the century old problems (1)–(3) [6,7]. Why, for God’s sake, should any scientist in good faith prefer the nomenclature of quantum chemistry over the above initial and tentative, yet quantitative treatment?

(2) When used without adulteration of the basic axioms, quantum chemistry has been unable to reach an exact representation of the binding energies of simple molecules, such as the H_2 , due to the historical missing of 2% of the experimental data, with embarrassing deviations for complex

molecules. The approximate validity of quantum chemistry under these conditions is beyond scientific doubt. But then, equally beyond doubt is the nonscientific character of denying the need for a covering theory. Santilli has dedicated his scientific life to the achievement of a covering of hadronic chemistry that did indeed achieve a numerically exact and invariant representation of molecular binding energies, that is, exact to the desired decimal [6,7]. The iso-, geno-, and hyper-branches of hadronic chemistry have been proved to be directly universal for all possible systems of the classes considered. Hence, claims of a new covering of quantum chemistry are plagiarism and/or fraudulent.

(3) In the late part of the past century, chemists introduced the so-called “screenings of the Coulomb law,” namely, the multiplication of the Coulomb law by a function, $V^*(r) = f(r)qq/r$ and, in so doing, they did improve the representation of molecular binding energies, trivially, because the arbitrary function $f(r)$ is fitted from the experimental data. What a great scientific achievement! Unfortunately for the orthodox chemists, they kept calling the resulting theory “quantum chemistry.” Even an undergraduate student nowadays knows that the notion of quantum can be formulated only for the Coulomb potential $V(r) = qq/r$, and absolutely not for screened potentials $V^*(r) = f(r)qq/r$. Additionally, the orthodox chemists entered into all sorts of purely theological argumentations, published in the best chemistry journals for posterity to judge, claiming all sorts of quantized orbitals under the screened Coulomb law, when everybody knows that quantum orbitals are solely possible for the Coulomb law. One of the technical reasons is that the transition from the Coulomb potential to its screened form can only be done via a unitary transformations (because the Coulomb potential is a central invariant of quantum mechanics). Hence, all these papers in the best chemical journals dealing with “quantum” treatments of screened Coulomb laws are outside the class of unitary equivalence of quantum chemistry. Hadronic chemistry did cut out this inconsistency beginning with the central hypothesis that quantum mechanics itself is not exactly valid for the region of space of the order of one Fermi containing deep wave overlappings of the wavepackets of valence electrons. These interactions cannot be represented by a Hamiltonian, and consequently the correct theory has to be nonunitary. It then follows that all possible screenings of the Coulomb law are trivial particular cases of hadronic chemistry [6,7]. These advances touch the real issue here: Santilli’s discoveries are a threat to organized interests in chemistry due to their basic novelty,

(4) A graduate student in Russia has proved the statement in monograph [6] that quantum chemistry predicts all substances to be paramagnetic, in huge disagreement with reality. The proof was elementary and based on the weakness (better, the absence) of a real bond between valence electrons, in which case orbitals can indeed be individually oriented under a sufficiently strong external magnetic field, thus implying universal paramagnetism. The use instead of Santilli strong valence bond [6] has resolved this additional, century old, gross inconsistency of quantum chemistry precisely in view of the strength of a new attractive force between valence electron pairs in singlet coupling. It then follows that the H_3 routinely detected in GC-MS cannot be a

molecule, because of the known impossibility of a third electron (with spin $\frac{1}{2}$) to be bonded to a valence electron pair (the pair having spin 0). H_3 is then an elementary Santilli magnecule $MH_3 = (H - H) \times H$, where $-$ represents valence and \times represents magnecular bond. A similar situation occurs for most of the anomalous species in HHO gas [1], as well as in MH_7 [2].

(5) Santilli has stated in his works [6,7] that he refused research contracts from publicly listed companies for research via quantum chemistry on thermodynamical events or chemical reactions on ground that it would be “a fraud of public money.” I agree that the errors of quantum chemistry, even though seemingly small in binding energies, are a large multiple of the thermodynamical quantities to be computed. Another graduate student has proved that, following the reaction $2H_2 + OH_2$, quantum chemistry predicts a finite probability of the spontaneous decay $H_2OH_2 + O$, in gross violation of causality laws, energy conservation laws, etc. Serious chemists do not need to redo calculations since the prediction can be derived from the time reversal invariance of the orbitals in Bohrs hydrogen atom (technically, the occurrence originates from the assumption of the same, time-reversal invariant, Hilbert space for the initial and final states, in which case the prediction becomes unavoidable under Heisenberg’s uncertainty principle). Chemists do teach the synthesis $H_2 + OH_2O$, but studiously suppress the prediction by quantum chemistry of the spontaneous decay H_2O , evidently because that would invalidate the preferred theory. In hadronic chemistry the spontaneous decay is proved to be impossible, because of the incoherence of conventional Hilbert spaces for the initial states and the iso-Hilbert space for the final state. To comprehend this point, chemists should study Santilli memoir [5] (published by the Italian Physical Society) beginning with the study of the new structurally irreversible genomathematics needed for credible treatment of irreversible chemical events.

5. Conclusion

Serious research is the pursuit of new knowledge and definitely not the maintaining of old scientific doctrines, as preferred by Calo. When facing new experimental knowledge, serious chemists are expected to repeat the measurements, study in detail all pre-existing literature, and admit that “science will never admit final theories” [1–12].

While Calo proffers final scientific knowledge to the end of time, quite humbly, Santilli presents his solutions of the above insufficiencies as “tentative” and calls for collegial efforts to achieve an irreversible covering of quantum chemistry as an evident necessary pre-requisite for the

conception and industrial development of much needed, notoriously irreversible, new clean energies and fuels.

It is hoped that these comments will suggest caution to scientists expressing criticisms of Santilli’s research.

Acknowledgments

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